UNDER THE Exclusive Economic Zone and Continental Shelf (Environmental Effects) Act 2012 (the Act)

IN THE MATTER OF A Decision-making Committee appointed to consider a marine consent application by Chatham Rock Phosphate Limited to undertake mining of phosphorite nodules on the Chatham Rise

STATEMENT OF EVIDENCE OF ELIZABETH ANN FULTON FOR THE CROWN
12 September 2014

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EXECUTIVE SUMMARY

A. Pinkerton (2013) is a well implemented trophic model that indicates the Chatham Rise is a pelagic production dependent system where habitat forming groups, such as corals, have little trophic influence on the dynamics of the ecosystem.

B. Pinkerton (2013) uses a well respected and accepted modelling approach to capture the major ecological groups on the Chatham Rise and to consider trophic interactions. This model is well constructed but, as acknowledged by Pinkerton (2013), can only consider trophic interactions. It cannot consider the other ways in which seabed mining could impact the ecosystem, such as via habitat interactions and, as such, the model does not consider the ecosystem impacts of the proposed mining activities. This is noted and discussed in the statement of evidence of Dr Matt Pinkerton (29 Aug 2014; e.g., paragraph 6).

C. The Environmental Impact Assessment (EIA) largely accurately reflects the Pinkerton (2013) body of work and acknowledges the aggregated form of the model and the lack of seasonal and interannual variability, which Dr Pinkerton’s evidence notes are limitations of the model. However, the EIA does not discuss the implications of the model’s structure on the conclusions presented. Particularly, it does not acknowledge the fact that the model can only consider trophically-mediated effects and cannot address the other ways in which the mining operations could disrupt local ecosystem functions. For example, while the model contains habitat forming groups, it does not represent interactions mediated by habitat. These kinds of effects are where habitat is critical for the survival or growth of a life history stage (e.g., juveniles) or for a process or behaviour (such as spawning). As these uses of species as habitat do not involve preying upon those species, a trophic model cannot consider them (and it is impossible to say which is more important without explicitly considering both kinds of processes).

D. On the basis of evidence presented by Dr O’Driscoll, Susan Baird and Alistair Dunn, it appears that, for at least some fished species, habitat mediated effects may not be high. I refer to their evidence below where relevant, but note that it remains to be tested. However, uncertainty remains for species that are not key fishery targets (e.g., Pinkerton’s small demersal fish group).

E. The trophic model also does not address other ecosystem processes of relevance to the question around ecosystem effects of mining, such as nutrient cycling. Disturbance of the seabed, direct removal or mortality of species in the mining
path and potential smothering of benthic species under the plume have the potential to modify local scale nutrient cycling which could further exacerbate effects on bottom communities and detritus based foodwebs. While such webs may not have import for the entire ecosystem productivity (which appears from the Pinkerton model to be largely pelagically driven), it could affect detritus dependent fauna. Explicit representation of this is beyond the scope of the existing trophic model; perturbation scenarios could be run using the model (where the effect is imposed to look at consequences) but none were reported.

F. The scale of a model can be important for the inferences drawn from its output. The Pinkerton model is for the whole Chatham Rise and, as such, has veracity for considering broad patterns. The response at specific locations may be significantly different depending on local composition, dominant processes and strength of interaction.

G. In summary, the work by Pinkerton is well done and acknowledges limitations. However, I consider the Pinkerton (2013) model in isolation is not sufficient to make statements regarding ecosystem impacts (as the model only deals with trophic effects) and that the EIA does not resolve a number of the uncertainties (e.g., the influence of benthic habitat on ecosystem dynamics). Before Chatham Rock Phosphate (CRP) can be said to have considered more than just trophic impacts, further model assessments or data collection is required (as listed in sub-paragraphs a-f) below. On the basis of evidence presented by Mr Alistair Dunn at least one of these recommendations has already been addressed (as noted in sub-paragraph e below).

a. As a first step the Mixed Trophic Impact assessment should be extended to the detritus to clarify the role of that as a basal food source for the benthic foodweb.

b. Additionally a perturbation analysis (e.g., using the approach of Dambacher et al. 2003) should be performed using the existing model, but with the addition of habitat mediated connections, to consider the outcomes of perturbing different parts of the ecosystem.

c. A broader sensitivity analysis based on the existing model would also be encouraged (and should be informed by the assessments undertaken by Mr Alistair Dunn).
d. The typical next step if these additional model-based analyses suggested that there were non-negligible potential effects due to any of the ecosystem pathways would be further modelling or data collection to resolve the remaining uncertainties. For instance, a more resolved model, or additional targeted models, that allows for direct representation of the non-trophic processes would be beneficial for reducing uncertainty about risks associated with the ecosystem’s non-trophic impact pathways.

e. Ultimately (but most expensively) the ideal would be data sets dealing with the true extent and importance of the habitats likely to be affected. Baseline surveys of habitat and sediment substrate distributions and data around the role of the habitats in population-level health (e.g., of demersal fish species) using the region would reduce uncertainty and allow for more definitive (and reliable) statements around ecosystem effects. Some of this (e.g., the potential role of the area in spawning of some species) is addressed in the evidence of Susan Baird) but the validation of the habitat extents would be very beneficial.

f. If suitable data cannot be readily obtained then a range of models (of differing degrees of complexity) could be used to explore “what-if” scenarios around the potential modification of important ecosystem processes at various spatial and temporal scales, as Mr Dunn has already done for hake, hoki and ling. It would be infeasible to do this for all species, but it could be done to a level that the influence of the other ecosystem pathways on groups that are ranked as important in the Mixed Trophic Impact assessment (e.g., small demersal fish) can be considered.

H. More information and alternative models and analyses would also be required to give full consideration to potential cumulative effects.

INTRODUCTION

Qualifications and experience

1 My full name is Elizabeth Ann Fulton. I am a Senior Principal Scientist, Head of Ecosystem Modelling, for the CSIRO Australia, Oceans & Atmosphere Flagship. I have a PhD on ecosystem model complexity from the University of Tasmania and a BSc Hons I (university medal) in Marine Biology, Mathematics and Statistics from James Cook University, Far North Queensland Australia. I have 18 years’ experience in the field of integrated marine ecosystem modelling, ecomathematics and marine biology. I have been the lead developer on three whole-of-system
modelling frameworks for use in coastal and marine systems, which span from climate drivers and habitats through the foodweb to the human use and management of the systems. These modelling platforms have been adopted by researchers in 26 countries so far. I have authored more than 70 publications to date and have over 3000 citations and have supervised 10 graduate students and six post doctoral fellows. My research expertise was acknowledged in 2007 when I was awarded an Australian Science Minister’s Prize for Life Scientist of the Year for my achievements in marine ecosystem modelling and the impact of the work on regional marine planning, managing the impacts of fishing, and understanding and managing climate change. In 2010 I received a Pew Fellowship in Marine Conservation to develop models for assessing how marine biodiversity is affected by pressures such as overfishing and climate change; and I was a contributing author to the IPCC WGII AR5 Chapter 6, Ocean Systems.

This evidence is my own testimony and any conclusions, as well as any views or interpretations expressed herein, are my own and do not necessarily reflect those of the Australian Government or the Commonwealth Scientific and Industrial Research Organisation (CSIRO).

Code of Conduct

I have read the Environment Court’s Code of Conduct for Expert Witnesses in the Environment Court Consolidated Practice Note (November 2011), and I agree to comply with it. My qualifications and experience as an expert are set out above. I confirm that the issues addressed in this brief of evidence are within my area of expertise. I have not omitted to consider material facts known to me that might alter or detract from the opinions expressed.

Material considered

In preparing this evidence I have read and given consideration to:

a. the Marine Consent Application (comprising the Environmental Impact Assessment (EIA), and Appendices 1-35iii, dated May 2014) (hereafter referred to as the application);

b. Appendix 22 – Ecosystem Modelling of the Chatham Rise (Pinkerton 2013);
c. CRP ‘Response to Request for Further Information (Request Nos. 12, 13, 41 to 43)’, dated August 2014 and lodged on the EPA website under the heading “Further information – Response 12” (FIR);

d. Statements of Evidence of the following expert witnesses for Chatham Rock Phosphate Limited (CRP), all as lodged on the Environmental Protection Authority (EPA) website as at 31 August 2014:

   i. Matthew Pinkerton, in relation to trophic models;

   ii. Judith Elaine Hewitt, in relation to benthic communities;

   iii. Ashley Alun Rowden in relation to benthic communities and spatial planning;

   iv. Alistair Glen Dunn, in relation to fisheries population impact assessment;

   v. Susan Jane Baird, in relation to ling fishery;

   vi. Michael John Page, in relation to effects of sediment plume on fish eggs and larvae;

   vii. Richard Lyell O’Driscoll, in relation to commercial fisheries and fish spawning;

   viii. Ian David Tuck, in relation to commercial fishing and cumulative effects from fishing and mining; and

    e. EPA Staff Report, Chatham Rock Phosphate Marine Consent Application, August 2014.

**SCOPE OF EVIDENCE**

5 This evidence addresses:

a. the matters that would need to be addressed by any model used to consider the ecosystem impacts of a mining operation of the kind proposed by CRP;

b. the ecosystem model of Pinkerton (2013), its content, capabilities and results;

c. the use of the Pinkerton (2013) model and its outputs in the EIA;
d. the EIA’s conclusions regarding potential ecosystem and cumulative impacts;

e. the uncertainties and gaps in the ecosystem model of Pinkerton (2013); and

f. additional research possibilities.

ECOSYSTEM MODELLING IN GENERAL

6 Lindeman in 1942 originally defined an ecosystem as “An ecosystem composes of
physical-chemical-biological processes active within a space-time unit”. It has since
been recognised that human actions are integral to ecosystem function too. This
means that many different kinds of interactions underlie the functioning of marine
ecosystems, including habitat use, biogeochemical cycles and trophic connections
(the foodweb).

7 I agree with Pinkerton (2013) (page 9) that “a coherent and consistent picture of
ecosystem function, once developed, can be used to explore potential relative
sensitivities of the ecosystem to effects of mining”. This is the role of models, but
the models will have limitations around the processes they include or omit. In
terms of extractive activities such as seafloor mining, there are a number of direct
and indirect pathways of impact that need to be considered when discussing
potential ecosystem impacts. These include:

a. direct physical disturbance of the seafloor substrates and any species living on or
in those sediments;

b. the physical impacts (on water column and seafloor communities and processes)
of returning unwanted materials to the seafloor;

c. modification of habitats (by direct removal, smothering, changed water quality
conditions, changed sediment and benthic nutrient and detritus cycling,
changed community composition and relative ecological interaction strengths);

and

d. disturbance of the life history of flora and fauna in the vicinity of the mining
operations – either through direct interaction with the mining operations,
vessels, associated noise, plumes or any waste discharges or indirectly via
modified habitat availability or predator-prey fields (i.e., changed abundance of
community components can change the amount of prey available to a species,
but also predation pressure on a species; this is what Pinkerton (2013) was
focused upon).
Based on his evidence (e.g., paragraph 52), Dr Pinkerton appears very aware of these requirements (e.g., the need to resolve prey items of commercially-important fish stocks and the need to represent modified reproductive capacity of commercially-important fish populations as a result of trophic or habitat effects). His cautions in the original Pinkerton (2013) report and his evidence reflect this understanding and his expertise.

Human activities can also interact, either via operation in the same area or by putting pressure on different parts of an ecosystem in such a way that the combined outcome shifts ecosystem state. Such combined, or cumulative, pressures can act in a non-linear way, which can be hard to anticipate if they are operating via indirect connections. This can occur even if the activities are disjoint in time and space. For example, if different activities influence different parts of the life history of a species (e.g., juveniles and adults) or different (but connected) species in a foodweb or habitat-species pair.

Synthesis of understanding in a quantitative model and analysis of the outcomes of perturbations of the model state is one means of assessing the potential effects of an activity like seabed mining. This can be done in isolation or as one information source used in a broader assessment. Either way, the list of potential pathways means that for a relatively complete assessment of potential impacts of seabed mining any ecosystem model would need to:

a. explicitly represent relevant impacted group types (e.g., benthic invertebrates, fish and top predators which use the location) and potentially represent different life history stages (if there are differential effects or potential bottlenecks if a key life history stage is disproportionally exposed or vulnerable);

b. include trophic connections (so that trophically-mediated impacts are appropriately transmitted through the foodweb; as has already been done by Pinkerton (2013)); and

c. include habitat associations (so habitat mediated interactions and the effects of their loss could be considered).

Resolution of biogeochemical processes (e.g., nutrient cycling) which could also be influenced by seabed mining disturbances would also be beneficial. While these likely have limited spatial extent and likely little influence on productivity of overlying waters, they do have the potential to influence benthic animals,
particularly those dependent on detritus based foodwebs. Explicit consideration of the topic would help verify the level of associated risk.

The type of model chosen, the kinds of information used to create the model and the number of processes included dictates what is an appropriate use of the model. The EIA refers to the Pinkerton (2013) model when discussing impacts on the ecosystem. This is appropriate to the extent of considering how altered biomasses of one group (which may occur as a result of mining activities) might influence the biomasses of other groups via trophically-mediated interactions. This is not the same as considering all ecosystem impacts or even the dynamic changes possible in a foodweb as a result of seabed mining. This appears to be the point that is being made by Dr Pinkerton in his evidence (paragraph 6) and in CRP’s “Response to Request for Further Information (Request Nos. 12, 13, 41 to 43)” (FIR) on page 4 where it states “The model presented in the Pinkerton trophic model report is a static model (i.e., it shows the structure of the food-web but cannot be used to investigate changes in the ecosystem resulting from perturbations (e.g., due to mining or any other external influence)).”. Other forms of dynamic model, which explicitly include the processes outlined above, would be needed to consider broader ecosystem effects.

Dr Pinkerton’s evidence notes a dynamic model using the Atlantis modelling framework may be available in the next 2-4 years (paragraph 50). The FIR also mentions that such a model may be available in the future, but suggests the time frame is 1-2 years (page 4). Given the complexity of such a model and the expertise required to see it address multiple uses (or at least uses beyond fisheries scenario analysis), it is my estimation that one year is an unlikely time frame for delivery given my understanding of the status of the project.

PINKERTON (2013) MODEL STRUCTURE AND CONTENT

Pinkerton (2013) uses a mass balance ecosystem model. It is a re-implemented form of a very commonly used ecosystem model – Ecopath with Ecosim (Christensen and Walters 2004). The model is primarily a trophic modelling approach, which uses biomass accounting to track the flows of biomass (or energy) through a foodweb.

Pinkerton (2013) uses a widely accepted modelling approach of resolving key species and then aggregating other species into functional groups defined based on diet, size and position in the water column (some taxonomic divisions are also
recognised, for instance invertebrates are typically kept separate to vertebrates). I agree with Dr Pinkerton’s evidence (paragraph 25) that the model’s construction was consistent with best practice internationally. Despite that, I would contend that a broader sensitivity analysis (using multiple parameterisations) would have been helpful for dealing with uncertainty (as discussed below).

The final form of the model includes 37 biological groups, summarised on page 7 of Pinkerton (2013) and as follows:

a. seabirds
b. two kinds of cetacean;
c. pinnipeds;
d. nine demersal fish stocks, resolving key commercial target species like hoki, orange roughy, warehou and oreos – the other demersal fish stocks are represented by aggregate functional groups;
e. four mesopelagic functional groups (which are based on diet and taxonomy) – mesopelagic fishes, squid, krill, salps);
f. 10 benthic invertebrate groups – which resolve habitat forming groups (such as coral) as well as the other major taxonomic, dietary and mobility classes in the benthos (other encrusting invertebrates, seastars and brittlestars, echinoderms, sea cucumbers, prawns and shrimps, large benthic worms, bivalves and gastropods, macrobenthos, meio-benthos);
g. plankton groups – represented by three size-based classes of zooplankton, as well as phytoplankton;
h. bacteria; and
i. detritus.

The number of groups is not large but it would not be considered overly aggregated either, key trophic links and feedback pathways have been resolved.

Based on which species are represented at a species level (commercially-targeted fished species) and which are aggregated (e.g., “small demersal fishes”), the model appears to have been developed to focus on questions other than seabed mining (such as fisheries). Alternatively the model structure may reflect the structure and
resolution of data initially collected for purposes other than the assessment of seabed mining. This means particular care must be taken around interpretation of its results with respect to the potential effects of mining, as it may not be straightforward.

Given many of the potential direct effects of seabed mining are likely to be habitat-mediated, it is an important limitation of the current form of the model (as noted by Pinkerton (2013) and in paragraph 4 of Dr Pinkerton’s evidence) that while habitat forming groups have been included no habitat use mechanism is represented, only their trophic role in the ecosystem. By using biomass pools for entire populations rather than life history stages (e.g., juveniles versus adults) and omitting habitat mediated effects, the potential influences on habitat dependent species cannot be considered using this model. For instance, the model does not consider any reduction in predation rates due to habitat use (or increase in predation rates if habitat is removed by seabed mining), which might occur for species like hoki (*Macrourus novazelandiae*) which has juvenile habitat on the ridge of the rise; nor does it have population growth rates (e.g., in terms of spawning success) mediated by habitat state for species such as ling (*Genypterus blacodes*).

The model has been parameterised based on a body of existing work. Pinkerton (2013) very clearly states intermediate assumptions made in reaching these parameter values, the sources of information used and the fact it is only one of many possible models. Pinkerton (2013) also acknowledges that the availability of data is not uniformly high over all species groups and where data is missing data from more well-known species is used as a basis for estimating values for the less data rich species. This is an accepted means of gap filling in such models. Overall, the model construction appears to have been done rigorously and thoroughly, using local observations where possible (the majority of cases). This increases confidence in the results being pertinent for the ecosystem in question.

In a model such as this one parameterisation is not enough to finalise the model’s form, a “balancing step” is required to make sure that flows into each biological group equal the flows out (i.e., “balances”). The steps taken in balancing the model (which is a key step in the construction of a model of this type) are quite clearly laid out. Given the intense use of local observations it is reassuring that most of the initial parameter values did not need to be changed by more than 10% (i.e., the local information adds up; if information from other locations had been used larger changes would likely have been necessary). Some of the more uncertain dietary
components did need to be changed by more than 10% (up to \( \pm 45\% \)), but that is not uncommon for such uncertain data in a model of this kind.

22 The weakest point of the model implementation is its validation. Pinkerton (2013) did not contain any comparison with local observations, in terms of net primary production versus satellite or other estimates rather than models from other locations, and at the time Pinkerton (2013) was written the isotope-based validation of the dietary pathways and resulting trophic levels was incomplete. Despite this, the model as presented in Pinkerton (2013) is as well validated as most ecosystem models; these extra comparisons would have just provided an extra level of rigour.

23 The scale of a model can be important for the inferences drawn from its output. The Pinkerton model is for the whole Chatham Rise and, in its current form, it is useful for considering the structure of the large-scale Chatham Rise food-web (as stated in Dr Pinkerton’s evidence paragraph 25). In terms of its usefulness at smaller spatial scales, such as at the scale of the mining area, the model can be used in two ways: (i) by modifying the biomasses by the amount representative of the relative depletion that would occur if the mining area is depleted and looking at the effect at the level of the entire Chatham Rise; or (ii) assuming scale invariance (i.e., that the relative proportions for the entire region are reflective of local composition and relative importance) and looking at the potential influence of changes in some groups on the performance and status of other parts of the systems as an indication of what might be key influences at local scales. The former is a robust use of the model, the latter can provide useful insights but is uncertain. It has veracity for considering general patterns but is uncertain with respect to the detailed response of specific locations as the local composition and effects may actually be quite different to the broad region structure and strength of interaction.

24 It is the concept of the use of the model for broad structural understanding that I think Dr Pinkerton is referring to in paragraph 27 of his evidence, where he states: “Notwithstanding these limitations, examining the large-scale overview of the structure of the Chatham Rise food-web at the present time can provide insights into the likely effects of activities within the marine environment on the ecosystem. By understanding the characteristics of the food-web, some inferences as to sensitivities and interactions between species in the system can be made should disturbances to parts of the ecosystem occur”. Dr Pinkerton is quite aware of the limitations of his model and is quite clear on the uncertainty around its use as he clearly states in paragraph 30c of his evidence where he states that: “effects at
smaller spatial and temporal scales, and effects concerning only parts of populations, are not resolved”. Further discussion around model uncertainty is left to a dedicated section below.

MODEL OUTCOMES

25 The model indicates that the ecosystem is mostly driven by pelagic primary production, with demersal fish and top predators feeding on mid-trophic groups (who in turn source feed from pelagic production via mesozooplankton). There is also a well-developed microbial loop. In itself this would suggest any trophic disruption of the ecosystem due to disturbance amongst the benthic groups (i.e., lost food due to the decline of benthic biomass as a result of direct removal by mining or smothering by sediment return) would be expected to be small.

26 Pinkerton (2013) used an index called “trophic importance” to consider the trophically-mediated overall effect on food-web structure of changes to the abundance of a group in the model. As described on page 7 of Pinkerton (2013), “trophic importance” is defined as the effect of one group on all other groups and the importance of a group to the structure and function of the ecosystem. The analysis used two indices of trophic importance, calculated in slightly different ways (effectively providing alternative weightings for weaker trophic connections and resulting feedback higher order effects). The original form of the index was originally developed by Libralato et al. (2006) but was modified by Pinkerton (2013) to address some issues with the original index around the importance of weaker trophic links (which may have a disproportionately important role in maintaining ecosystem function and stability Pinnegar et al. (2005)).

27 This analysis of trophic importance concurred with the view that pelagic not demersal basal species are key to the ecosystem function. Mesozooplankton was consistently found to be important regardless of the method of analysis used and phytoplankton was found to have an underpinning role in the system. The mid-trophic groups such as mesopelagic fish and arthropods were also found to rank fairly highly, as were small demersal fish. Some of the species targeted by commercial fishing (often referred to as target species) were also found to be relatively important. However, the trophic importance of some groups likely to be directly impacted by seabed mining, such as corals and encrusting invertebrates, was found to be very low.
The scenario analysis in Pinkerton (2013) focused on corals and encrusting invertebrates, which is a sensible first step given they are habitat forming groups that have been found to be vulnerable to disturbance elsewhere. However, not all important benthic components are visually obvious, above ground habitat structures. Soft sediments and the species they contain can have important roles in ecosystems too. The Mixed Trophic Impact assessment (trophic importance analysis) does consider the dynamics and influence of soft sediment and infaunal groups. Meiobenthos was found to have a potentially important trophic role, being ranked 8th with the TI2 index (Figure 6). This ranking is largely due to the influence of changes in meiobenthos on the holothurian group in the model. In the rank vs score plots (Figures 5 and 6 in Pinkerton (2013)), meiobenthos sits in a cluster of 7-8 species groups that show little incremental improvement in score per rank, but in both plots this cluster sits well above the scores of the cluster containing the corals and encrusting invertebrates. This indicates that in terms of trophic interactions meiobenthos is a more important group to focus on in this case than the corals or encrusting invertebrates that receive more explicit attention in the sensitivity analysis of Pinkerton (2013) and in the EIA.

APPLICATION OF THE MODEL IN THE EIA

Model use

The EIA uses data sources referred to in Pinkerton (2013) as sources of information for the EIA summaries for each group type – e.g., Hall et al. (2009), Hewitt et al. (2011), O’Driscoll et al (2009), “Gibson 1995, Annala et al. 2003a in Pinkerton 2013 – Appendix 22” – as well as directly referencing Pinkerton (2013) as a source for descriptive summaries of the components being considered. “Pinkerton (2013) (Appendix 22) provides a discussion of the benthos within the context of the wider ecosystem on the Chatham Rise” (page 110 of EIA). Most of these references are largely faithful to the content of Pinkerton (2013). However, there are some factual inaccuracies – e.g.:

a. on page 145, the EIA states that Pinkerton (2013) identifies that about half of the food of benthic macrofauna is taken from benthic sources and half from the water column, but Pinkerton actually states (page 29) that benthic macrofauna take 97% of their food from benthic sources and 3% from the water column; and
b. page 145 of the EIA also states that “decapods (shrimps etc.) take their food from the water column” but the diet matrix in Pinkerton (2013) does not seem to indicate any pelagic groups in the diet of decapods.

The EIA refers to the outcome of Pinkerton’s Mixed Trophic Impact assessment in many places – including sections on the benthos (page 145), pelagic groups (e.g., phytoplankton page 147, zooplankton pages 148 and 153), finfish (pages 171, 175, 180, 184, 187), mammals (pages 225-226), seabirds (page 235) and overall ecosystem (pages 236-237, 335-338). The findings of this Mixed Trophic Impact assessment are used in the EIA (e.g., page 6 of the summary at the front of the EIA) as the basis for the EIA’s conclusions that there will be few ecosystem impacts of the proposed mining activities on the Chatham Rise: “Scientific study of the food web on the Chatham Rise indicates that it is unlikely that the loss of benthic fauna in the mining blocks will have a significant impact on the Chatham Rise ecosystem”. I consider this conclusion goes beyond what can be validly stated based on the evidence presented in Pinkerton (2013). Pinkerton (2013) can only be used as evidence of little likelihood of a trophic impact, it cannot be used to make definitive statements on overall ecosystem impacts as there are impact pathways not explicitly considered by Pinkerton (2013) and their relative importance cannot be definitely judged without their explicit consideration.

The discussion of the results of Pinkerton’s Mixed Trophic Impact assessment in the EIA is largely a faithful representation of the findings given in Pinkerton (2013), but again there are some factual inaccuracies in the detail. For example:

a. page 145: “macrobenthos (ranks 10 and 13 respectively) and meiobenthos (ranks 14 and 8) ranked higher than all other benthic groups”. This is not strictly correct. The arthropods are classified as benthic megafauna by Pinkerton (2013) (page 167) as it consists of benthos and hyperbenthos; this group was ranked four and six. The cephalopods are largely squid, but also contain some benthic components (octopus; page 123) and were ranked five and 11;

b. page 153: “Mesopelagic fish ranked seven in both the TI1 and TI2 models. In the TI2 model, mesopelagic fish have positive effects on a number of fish groups (e.g., hoki) and high end predator groups (e.g., birds, toothed whales, pinnipeds etc.). This group also has negative impacts on groups such as hard bodied macrozooplankton such as krill”. Figures 3 and 4 in Pinkerton (2013)
show small positive relationships only for birds, hoki, cephalopods and mesopelagic fish and negative effects for mesozooplankton and phytoplankton;

c. page 236: “In the single-step analysis, there are positive (bottom-up, prey driven) impacts of the four key mesopelagic groups (mesopelagic fish, cephalopods hard- and soft-bodied macrozooplankton) on many demersal fish groups” but on page 32, Pinkerton (2013) actually says “In the single-step analysis, there are positive (bottom-up, prey driven) effects of the six key middle trophic level groups (small demersal fishes, arthropods, mesopelagic fish, cephalopods hard and soft-bodied macrozooplankton) on many demersal fish groups”. The role of the demersal groups has been dropped in the EIA; and

d. page 338: “The model indicates that the fish guild of rattails and ghost sharks is the most important demersal fish group in the model (3rd or 4th order of TI – refer Figure 120)” . Figures 4 of Pinkerton (2013) (reproduced as Figure 120 of the EIA) shows that small demersal fish rank two and are actually the highest rank fish group for TI2, demersal or otherwise, with rattails and ghost sharks ranking nine. The same relative order is also true for TI1 with small demersal fish again rank two and the rattails and ghost sharks rank 13.

32 The content of the Mixed Trophic Impact assessment is not always well reflected in the summary sections of the EIA. For instance, despite reporting on the ranking of all major group types, including meiobenthos which had a ranking of eight for TI2, the summary section on page 340 states: “The trophic model suggests that the potential impacts on the Chatham Rise ecosystem associated with the removal of benthic fauna from the mining blocks is minor when taken in the context of the marine consent area and the Chatham Rise environment as a whole”. This statement: (i) gives the impression of extending beyond trophic impacts; and (ii) seems to focus on emergent habitats or benthic groups (such as corals) rather than the entirety of the benthos (i.e., it seems to be more heavily weighted by the low trophic importance of corals and encrusting invertebrates rather than the higher ranking of meiobenthos).

33 The EIA acknowledges the aggregated nature of the model and the lack of seasonal and interannual variation (i.e., the model uses an average year). However, the implications of this, although acknowledged in Pinkerton (2013), are not elaborated upon. Critically, the EIA does not acknowledge anywhere that the Pinkerton (2013) model and results refer only to trophic impacts, not the full suite of
potential impacts. Nor does the EIA explicitly argue that the nature of the operations would mean that only trophic impacts would occur. This means that while the EIA conclusions may be correct for potential trophic impacts, they do not provide sufficient support for the claim of minimal ecosystem effects as such statements cannot be made without explicit consideration of the other potential impact pathways.

**EIA conclusions – ecosystem effects**

34 The EIA’s summaries for trophic importance of pelagic groups (sections 6.4.3.5, 6.5.6) and top predators (sections 6.8.4, 6.8.5.4, 6.9.5) do align with sensible conclusions from the modelling report based on the relative importance of different foodweb production chains. The very pelagic nature of the production (as captured in the model) means it is unsurprising that the depletion of benthic groups will have little trophic impact on the rest of the ecosystem. I would agree with the FIR (page 5) that it is unlikely that the classic pelagic primary production system will be impacted by the mining activities or sediment return. Consequently, groups dependent on such production should not see much trophic impact of operations.

35 The EIA’s treatment of potential impacts on water column species is not always satisfying however. The summary statement in section 8.6.8 does not discuss middle trophic level prey species effects (trophic or otherwise) and in section 8.6.5.3 there is only a cursory treatment on the topic, which states: “However, the probability that mesopelagic species will be significantly influenced by a sediment plume 50 m from the seabed is relatively low (as discussed below)”.

Where that discussion occurs is not clear. Consequently, it is difficult to ascertain how the EIA has reached this conclusion and whether there was consideration of the potential influence of plumes on these groups given the vertical distribution of these species can see them quite close to the sea bed (see evidence by Dr Pinkerton, paragraphs 56 and 57). If mesopelagic and hyperbenthic groups were to be affected then their predators could be too (Dr Pinkerton’s evidence agrees with this assessment, paragraph 62). As the predators of these groups include seabirds and marine mammals, as well as species (e.g., squids) that are themselves prey of seabirds and marine mammals, there could be unconsidered effects on species of conservation concern.

36 Neither the modelling report nor the EIA explore the potential impacts of disturbance of the foodwebs based on detrital pathways (a potential mechanism
also identified in paragraph 15 of Dr Hewitt’s evidence). Detritus-based pathways are likely to be equally important basal foodweb source for a demersal system at the depths consistent with the Chatham Rise. This may be why there is a small standing stock of detritus in those sediments. The moderate levels of overlying production (FIR page 7) suggests settlement of organic matter, produced in the upper ocean, should lead to local pools of detritus (this is seen on the Rise as reported in section 5.6.5.2 of the EIA, also see Table 7 of the EIA describing phytodetritus as one habitat type on the Chatham Rise and Figure 7 of Dr Pinkerton’s evidence for estimated flux rates to the sediments showing relatively high flux rates over the western end of the proposed mining area). The potential importance of detritus is captured by the trophic importance rank of meiobenthos, which feed solely on detritus and bacteria that would also live on the detritus. Meiobenthos rank more highly than the encrusting invertebrates and corals in terms of trophic importance. The explicit trophic importance of detritus cannot be commented on as it was omitted from the Mixed Trophic Impact assessment, however, while no rank of bacteria is given in Table 8 of Pinkerton (2013) bacteria are included in Figures 3 and 4 of Pinkerton (2013) and these figures indicate that benthic bacteria has a positive trophic impact on meiobenthos, arthropods and macrobenthos. The implications of the potential disturbance of detritus-based chains on meiobenthos are not discussed in the EIA, which tends to focus more on primary pelagic production and megabenthos (e.g., section 8.6.4.4). There is one explicit mention of detritus as a possible habitat type (Table 7) and five passing mentions as a potential food source (and three of those were referring to water column not sediment pools).

The EIA’s approach of only considering trophic effects is likely sufficient when considering potential ecosystem effects of the disturbance of soft bottom benthic communities (discussed in section 6.3). This is because the majority of the ecosystem interactions with such communities are trophic (meiobenthos of the kind found on the Chatham Rise do not create emergent habitat structures in the same way corals do) and thus the model already adequately represents the potential impact pathways. However, the EIA’s approach of considering trophic effects as a proxy for ecosystem effects (as that is all the model can provide) does not consider the habitat role of corals and other habitat forming taxa growing on nodules that may sit in/on soft sediments, such species cannot be dealt with via a trophic only approach.
One set of the ecosystem services provided by soft sediments that would not be covered by a trophic-only consideration of soft sediment communities is their role in nutrient cycling and biogeochemical processes in the immediate area of the mining activities. The information provided in the EIA indicates that soft sediments predominate on the Chatham Rise (section 5.6 and chapter 6). The evidence by Dr Rowden confirms that at least two of the five infauna communities found in the mining area are probably represented elsewhere on the Rise. For these communities relative area arguments are more easily justified, as soft sediment processes are more similar and homogenous spatially across areas where the communities are found. This means that the argument presented in the EIA and FIR that disruption of sediment biogeochemical processes will be limited and localised is probably valid for these groups. However, despite the EIA’s claim that: “Seabed sampling showed that the benthic communities and species within the marine consent area are not unique” (page 302), Dr Rowden states in paragraph 6 of his evidence that the nodule associated infauna have not been found elsewhere in the New Zealand EEZ. For these groups, the explicit consideration of the alteration of any biogeochemical cycling needs more explicit inclusion in statements on ecosystem effects.

Similarly, the role of habitat forming groups and any reduction in that role as a result of mining is required in statements pertaining to ecosystem effects; particularly as Dr Hewitt states in paragraph 30 of her evidence that all epibenthos in the mined area are classed as very sensitive with >25yr recovery time and Dr Rowden states in paragraph 48 of his evidence that larval dispersal would be the most likely route of recolonisation. There is a lack of data on both recruitment and growth of deep sea benthic communities so estimates of recovery time are difficult.

Hard bottom and habitat-forming benthos (either on hard bottom or growing on nodules) are widely recognised as having a disproportionately important for ecosystem functioning (e.g., coral reefs, temperate inshore and deep reefs of southeastern Australia). Much of the EIA dealing with benthic impacts (e.g., page 300 and following) acknowledges and discusses habitat removal, smothering and recolonisation. The EIA includes predicted habitat suitability maps for the different epifaunal community types (Figures 57-65), which can be of assistance when considering the relative extent of the potential disturbance of habitat mediated interactions – although as documented by Dr Rowden (paragraph 43) care must be taken in such an exercise because the broad-scale environmental data layers that inform the predictive models may not be complete or reliable (i.e., not at
appropriate scales) for some species (e.g., *Goniocorella dumosa*). The EIA recognises both direct and indirect impacts of mining on these benthic biota, but in reporting on “Changes to the Chatham Rise ecosystem” (section 8.6.7.2), the EIA does not discuss the ability of the model in Pinkerton (2013) to report on many of these impacts. Pinkerton (2013) cannot discuss such issues as the model does not include habitat mediated interactions, nor does it consider colonisation effects as the model is non-spatial. The EIA does not call on the model in those terms, but neither does the EIA discuss the model’s limitations around this point (the EIA and FIR discuss other limitations) or the implications of that when considering the trophic and ecosystem impacts.

When considering the impacts on finfish, ecosystems and conservation values, the EIA does discuss distributions and potential use of the area and its habitats (e.g., by hoki in Figures 79-80, hake in Figure 83, ling in Figure 84-85 and scampi in Figure 92). The evidence of Dr O’Driscoll and Ms Baird also deals with the importance of habitat in the proposed mining area for some of the demersal fish species it does not resolve the issue for all the demersal fish groups in the model. The EIA contains no discussion of the fact that, if any of those species (e.g. small demersal fish) have a habitat dependent life history stage or activity, then the model may be under-estimating the importance of the habitat forming group for ecosystem dynamics. Neither is there any discussion of how habitat and trophic impacts can interact. For instance, if the loss of habitat impacts the biomass of demersal species other than hoki, hake and ling (which have already been considered by Mr Dunn), then it is likely there would be further trophic impacts expressed through the foodweb, given the trophic importance analysis indicates that small demersal fish are the second most important group in the system.

The failure of the EIA to acknowledge that the modelling cannot address habitat mediated effects is concerning because of how the model output is treated in the summary of model results. For example, in section 8.6.7.2 the EIA (erroneously) identifies the “guild of rattails and ghost sharks is the most important demersal fish group in the model” and goes on to say “the worst potential impact on this group of demersal fish is expected to be proportional to the period that the mining block does not provide food for this group”. The perception left with the reader is that for this “most important” group there is little effect long term, with the implication that this means little ecosystem effect for this part of the foodweb. It should have been clear that this statement did not take into account potential habitat mediated
effects which could extend much longer and have the potential to be as, if not more, important for structuring some parts of the ecosystem dynamics.

While the evidence provided by Mr Dunn suggests there would be almost no change in the Chatham Rise population biomass of hoki, hake or ling as a result of direct mortality or habitat loss this would also need to be repeated (as far as possible given the data-poor nature of non-commercial species) for other groups ranked as important based on the trophic connections (e.g., small demersal fish) before confidence could be placed behind a trophic-only analysis.

**FIR conclusions on the implications of the absence of organisms**

The FIR states on page 6 that: “The model described in Pinkerton (2013) does not allow the assessment of impacts by the absence of any of the organism groups identified”. This is not strictly true; an investigation could be done (i) by reparameterising with group(s) at lower biomass levels (to represent the level of depletion across the mosaic of areas that have and have not been mined) or (ii) using the diet connection matrix as the basis of a press perturbation experiment using the same matrix algebra as used for qualitative models (Dambacher et al. 2003).

The FIR concludes on page 6 that: “the impacts are a proportional reduction in biomass of specific components (principally benthic, epibenthic and some hyperbenthic resources)”. Given the inherent nonlinearity of ecosystems, such a statement cannot be said with confidence unless it is actually explored using an explicit analysis. Having multiple models at multiple scales would have helped clarify all these points, it would also allow for analysis of press perturbation experiments on hyperbenthic organisms (which would help clarify the effects discussed on page 9).

**EIA conclusions – cumulative effects**

The EIA only considers overlapping impacts not the accumulation of impacts across the entire domain – concluding that there will be little cumulative impact as mining is not in the same areas as many fisheries activities and the mining area is “small compared to the area affected by bottom trawling” (EIA page 365). These conclusions are not drawn solely on modelling work, but they are contestable given the information presented in the EIA – which ignores the degree of patchiness of habitat, whether it is concentrated and the potential role of refugia. Refugia are locations that provide direct or indirect protection to a species, by
reducing exposure to a pressure (e.g., predation or fishing pressure, the BPA is an example of the later kind of refuge). This refuge role can either be on a permanent basis for a proportion of the population, which can then provide a biomass or reproductive buffer for the broader population, or it can be transitory (for short periods of time or life history stages).

It is impossible to say definitively if the loss of habitat in the proposed mining area would contribute to a population level decline in any fish or invertebrate species. There are habitat-dependent species of commercial interest that use the area during key life history stages (e.g., hoki, ling, scampi). The broader distribution of these species (and for hoki, hake and ling based on the modelling by Mr Dunn) suggests that the proposed mining area is unlikely to be the one critical area of habitat that supports the broader population (e.g., as would be the case if only larvae spawned their successfully survived to settlement). However, the data as presented is not comprehensive on that point for all fish and invertebrates and so definitive statements are not possible. The summary on page 340 of the EIA states: “no significant impacts are identified that affect key spawning, juvenile and young fish habitat off the main crest of the Rise and within a short distance of any mining block”. However it is unclear how they came to this conclusion as section 8.6.5.3 only discusses suspended solids not habitat, and section 8.6.7.1 does not explicitly discuss habitat either, the closest it gets is to discuss ling spawning in the marine consent area. The model cannot be called upon to resolve the issue, as the modelling cannot consider the issue at all. The evidence presented by Susan Baird appears to address this issue (though only for one species) as it states (paragraph 22) that the ling “spawning area is east of the three licence and permit areas” and so significant impacts on spawners for that species would not be expected, supporting the EIA’s position for that species.

This uncertainty is not addressed in the EIA’s discussion of ecosystem effects or cumulative impacts. Further, consideration of the region-wide implications of extra ecosystem disturbance (on top of existing activities like fishing) is required. The additional disturbance may be relatively small in areal extent, but (i) it overlaps with the BPA, which may have a refugia role for fished species and the ecosystem more broadly (with no guarantee that equivalent or better locations will be protected in its place a priori) and (ii) it ignores the multiplicative and nonlinear nature of cumulative impacts. Ecosystem responses are rarely linear, rather there can display nonlinear changes, such as threshold effects, where something that looks like a small incremental change can shift an ecosystem’s state or behaviour
quite substantially. As stated above, while it is unlikely that the marine consent area is the one key piece of habitat for a major species whose status has an important role in dictating ecosystem function, it is not impossible. The evidence of Mr Dunn is relevant in this respect. This evidence indicates that, at least for some species, if demersal habitat interactions are important the broader distribution buffers any losses within the mining consent area. I note that this inference currently depends on the relative proportion of the population in the proposed mining area and the modelling by Mr Dunn.

MODEL UNCERTAINTIES AND OMISSIONS

Parameter uncertainties

49 As discussed in the FIR, the trophic model is based on information that is not of uniform quality or quantity across the model groups. This is not an uncommon situation and is typically handled by having multiple parameterisations that between them attempt to cover that uncertainty. While the “trophic model was balanced taking into account this variation in knowledge of the different groups” (FIR page 3). The Pinkerton model may have used the best available information, but it is a single instance and so does not explicitly account for the uncertainty. Paragraph 30 of Dr Pinkerton’s evidence acknowledges that the model is “but one solution of many that may be considered ‘consistent’ with the available data”. The only place where the uncertainty is explicitly addressed (or reported) is when multiple biomasses are trialled for corals and encrusting invertebrates. Multiple instances of the model with alternative parameterisations could have been used for many more of the groups and thereby explicitly reflect the uncertainty across the ecosystem. This would not be infeasible given the static nature of the model and would provide greater confidence in the findings.

50 It is not sufficient to say that uncertainty will be handled by updating future models (FIR pages 2-4). It is important to always be explicit about uncertainties so that the level of understanding is clear and informed decisions can be made on the back of that. The original EIA does not discuss the implications of the uncertainty. Reporting Pinkerton’s discussion of the model parameterisation gives little indication as to how parameter uncertainty manifests in terms of the magnitude of uncertainty on the outcomes of the modelling result (this can only be reliably ascertained by completing a model sensitivity analysis).
The FIR at page 4 states that: “…our ability to reliably forecast what will happen to the Chatham Rise ecosystem as a result of mining will be limited. The current state-of-the-art in this area of science is that the behaviour of complex and poorly-observed systems (such as the Chatham Rise…) cannot be reliably predicted”. This relies on a very technical definition of forecast, which is to give a precise and accurate estimate of the future magnitude, trajectory and distribution of a system property (e.g., nutrient levels, temperature or biomass). Ecosystem models are not intended for such use, instead they are useful for projections (estimates of potential future gross state and system behaviour where the general levels and patterns are of the right form). The validation of past projections and model hindcasts (where models are run using older data to see if they project the current state) shows that used in this way ecosystem models can usefully project future states. So while I would agree with Dr Pinkerton’s evidence (paragraph 27) that predictions are not currently reliable from such models, I would argue that projections are more trustworthy and useful. This is a quite technical distinction, but it is important as it means that treatment of uncertainty can be usefully undertaken even with patchy or poor information. An ensemble model approach, where multiple models of the same system that include alternative model parameterisations and even alternative model formulations are now widely accepted as best practice in handling uncertainty for complex systems. Such an approach is possible with the available information. I consider discussions of the implications of uncertainty should have been included in the EIA regardless of the level of ecosystem complexity.

The recommended reference on dynamic ecosystem models given in FIR at page 4 (Salihoglu et al (2013)) is informative but does not reflect the diversity of relevant model types that exist for representing ecological systems, many of which do not require as much data but have their own set of limitations. A more complete discussion can be found in Fulton and Link (2014).

**Uncertainties around model structure and processes**

Amongst the greatest uncertainties in the model structure (and parameterisation) were around abundance of mid-trophic level prey species (especially decapods, small demersal fishes, mesopelagic fishes, hard- and soft-bodied macrozooplankton, and benthic macrofauna). Some of these, such as small demersal fish and mesopelagic fishes, were subsequently found to have a key role in the system so a reduction in uncertainties around their biomasses and diet connections would be of benefit for understanding the veracity of the results,
especially under changing conditions. Pinkerton (2013) (page 19) expressly states that having this information would better constrain the model.

Table 4 of Pinkerton (2013) also indicates that biomasses and diet parameters were uncertain for the megafauna groups (whales and seals). However, this is unlikely to substantially impact model based findings due to the small contribution these groups make to total system biomass.

The model is for an average annual period, it does not resolve seasonal or interannual differences in environmental drivers and resulting production. Similarly, as the model is non-spatial, it does not consider spatial heterogeneity or the possibility of multiple stocks or populations across the modelled area, which could have differential mortalities, productivities and subsequent trophic importance.

As discussed above, the model does not include habitat provision (i.e., the role of habitats in adding growth, survival and reproduction for some species) and so any habitat mediated ecosystem impacts would not be resolved by this model. This limitation of the modelling approach is recognised by Pinkerton (2013): “Although a trophic model such as that presented here is not, in itself, sufficient to assess the ecological effects of subsea mining on the ecosystem”.

Also as discussed above, nutrient cycling (and related biogeochemical processes) are not explicitly represented in an Ecopath with Ecosim style model of the kind used by Pinkerton (2013). Explicit consideration of such processes would require a different class of models. As these models were not considered by Pinkerton (2013) or the EIA (and relevant data pertaining to the topic was not presented), then the implications of the disturbance of these processes cannot be verified.

A final form of mediated interaction not considered by the model is where large bodied predators can force forage fish into upper water column layers, facilitating access by seabirds. The evidence presented suggests that impacts on such predators are likely to be minor so this omission is probably not serious. However, in terms of confirming that such assumed outcomes and assumptions are justified it would have been beneficial to explicitly consider it; or failing that to at least acknowledge the omission and provide a brief discussion of the implications.

The Pinkerton (2013) model does not use any ecotoxicological representations or transmissions. This means that if any metals or toxicants are biologically mobilised
by mining, the modelling does not consider any potential effects or transmission through the foodweb.

ADDITIONAL RESEARCH POSSIBILITIES

60 I agree with Dr Pinkerton (paragraph 49) that the model presented in Pinkerton (2013) is well implemented and is an effective synthesis of available information on the biological components of the Chatham Rise system (and as such is likely to be reliable as to the general structure of the food-web and trophic interactions). Nevertheless, it does not address all impact pathways (e.g., sediment nutrient processes, habituated mediated interactions), which would need to be addressed in other ways.

61 A staged approach can be used to address the questions over ecosystem effects. First, additional analyses can be done using the existing model and the results of that can then be used to dictate whether additional work is still required.

62 I consider the following should be undertaken as a minimum:

a. the Mixed Trophic Impact assessment be extended to the detritus to clarify the role of that as a basal food source for the benthic foodweb; and

b. a perturbation analysis (e.g., using the approach of Dambacher et al. 2003) be performed using the existing model, but with the addition of habitat mediated connections, to consider the outcomes of perturbing different parts of the ecosystem.

63 A broader sensitivity analysis based on the existing model would also be encouraged, particularly if some uncertainty remains for those species groups not already covered after the steps set out in paragraph 62 are undertaken.

64 If the steps recommended in paragraphs 62 and 63 above still have not reduced uncertainty around ecosystem effects (by resolving whether non-trophic pathways are important or not), then the development of models (or other quantitative assessments) that address more of the non-trophic impact pathways would also help provide insight into the broader set of potential ecosystem effects. Atlantis is one such model, but it will not be available for some time. Furthermore, it is only one of many modelling options, many of which are much simpler to implement and still allow for the exploration of scenarios around the kinds and magnitude of potential shifts in critical processes (and their expression via changed abundance,
etc.). For example, there are a few modelling options for considering habitat mediated effects:

a. the standard form of the Ecopath with Ecosim model allows for habitat mediated interactions and spatial versions of the model. One alternative is to include these in the Pinkerton implementation of the model or to re-implement the model in the standard software so this extra functionality can be explicitly included. Including age-based stanzas (the term used for life history stages in the kind of model used by Pinkerton (2013)) for the “ling guild” would also be beneficial for resolving processes specific to certain life history stages;

b. a specific habitat model could be developed, these could be targeted models of intermediate complexity (Plagányi et al. 2014). Such an approach could be useful and may be possible with existing datasets, or much smaller extensions of them – one such option would be to implement a dynamic metapopulation version of the habitat suitability maps (this was an approach that worked well for patchily sampled benthic habitats on the northwest shelf of Australia; Fulton et al. 2006); and

c. assessment models (or other forms of simple population model) could be developed for species found to be potentially impacted (as listed in the evidence of Dr O’Driscoll); especially those that have not already been assessed by Mr Dunn.

Having a broader ensemble of models would also help address one of the concerns raised by the EPA. The EPA staff report questions the ability of the model to consider (i) future trophic interconnectedness (paragraph 343, page 74) and (ii) cumulative effects (paragraph 348, page 75). The point on cumulative effects is in line with the points made above here. In terms of the appropriateness of the model for considering future trophic interconnectedness, the static nature of the Pinkerton (2013) model is sufficient for giving insight if conditions do not change substantially from the current state (in terms of environment, community structure and relative biomasses). However, if the structure of the ecosystem is significantly changed then it would no longer be appropriate. An alternative static model under the new state would be needed. Alternatively, a time varying (dynamic) model (as discussed above) could deal with looking at effects over multiple temporal scales and system states.
Targeted collection of data would be an ideal way of addressing uncertainties around poorly constrained groups in the model and resolving potential effects of modification of all the non-trophic impact pathways (although it is recognised that this may be prohibitively expensive to collect). Such information would be beneficial in terms of being able to make definitive statements about potential impacts. Baseline surveys of habitat and sediment substrate distributions would help resolve many uncertainties, as would clearer data-based direction on the role of the habitats for the population-level health of species using the region. Genetic tagging studies may assist such attribution, but such studies may be difficult to execute in practice given the scale of the area. I do note that collecting data on all the poorly covered species may not significantly change the outcomes of the trophic model and is technically challenging. Having clearer, more certain data on the extent of the various benthic habitats and their relative importance to the species in the region would be beneficial, and help lay remaining questions (e.g., around the potential effects due to habitat mediated interactions) to rest. It may not be possible to feasibly collect such information in more detail than already presented by Susan Baird and Dr O'Driscoll, however in his evidence (e.g., paragraph 30), Dr Pinkerton mentions that more data is being worked up and may be available in future to address trophic uncertainties and this would be a very useful start.

Dated: 12 September 2014

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REFERENCES


