A review of the hydrodynamic and plankton modelling conducted by ASR Ltd on behalf of Environment Bay of Plenty

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Environment Bay of Plenty

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Executive Summary

Environment Bay of Plenty (EBoP) commissioned NIWA to review the two reports:

a) Longdill, P. & Black, K.P. (2006). Numerical hydrodynamic modelling: aquaculture management areas. (Draft)

b) Longdill, P. et al. (2006). Bay of Plenty Primary Production Modelling: Aquaculture Management Areas; Primary production modelling, and assessment of large-scale impacts of aquaculture management areas on the productivity within the Bay of Plenty (V. 2, Revision A).

Both reports were provided to NIWA in draft-form by Environment Bay of Plenty.

EBoP required that our review:

- Assess the assumptions used for the basis for the models.
- Assess the methodology and robustness of the hydrodynamic and productivity models.
- Make a statement on the confidence that can be placed on using these to make assessments of aquaculture impacts on the productivity and sustainability of the Bay of Plenty's coastal waters.
- Provide comments on the reports in general, or aspects of the work that would appreciably benefit from further development or alternative approaches.

In the remainder of this summary, we will address each of these bullets in turn – first for the Hydrodynamic Model, and then for the Productivity Model.

1.1 Hydrodynamic Model

1.1.1 Assess the assumptions used for the basis of the models

We are satisfied with the general approach that has been adopted by ASR. We note however, that the hydrodynamic model takes no account of the influence which farm-structures will have upon local-scale hydrodynamics. This will not influence bay-scale circulation patterns, but may imply that the details of transport and vertical mixing in the immediate vicinity of farms will not be adequately

reproduced. Taking account of these local-scale influences is at the cutting edge of present research and we do not yet know how important they are in determining farm-induced plankton change.

1.1.2 Assess the methodology and robustness of the hydrodynamic and productivity models

The assumptions that underlie the hydrodynamic model are robust. The hydrodynamic model successfully captures the broad-scale hydrodynamics, but fails to capture many finer spatio/temporal scale events. This is not unexpected given the paucity of boundary condition data and limits in the wind-forcing data.

1.1.3 Make a statement on the confidence that can be placed on using these to make assessments of aquaculture impacts on the productivity and sustainability of the Bay of Plenty's coastal waters

On the basis of the evidence provided in the report, we are unable to determine whether or not the hydrodynamic model is reproducing the key characteristics relevant to the subsequent biological modelling. We believe that the requisite additional information (see below) could easily be inserted into the report.

1.1.4 Provide comments on the reports in general, or aspects of the work that would appreciably benefit from further development or alternative approaches.

We recommend that the authors of the hydrodynamic report add some more material to bolster their claims that the hydrodynamic model is performing adequately, and in order to provide ready access to information that is relevant to the biological modelling. Relevant information includes: (i) comparisons of observed and simulated residual currents and (ii) cumulative vector plots and (iii) details of thermal stratification in the model.

1.2 Productivity Model

1.2.1 Assess the assumptions used for the basis of the models

We will draw a distinction between the *plankton sub-model* (comprising all parts of the *Productivity model* except that used to determine mussel filtration/clearance rates) and the *Mussel model* (comprising of the methods by which individual mussel clearance rates (m3 mussel^{-1} d^{-1}) and within-farm volume-specific mussel clearances rates (m^3 filtered m^{-3} of AMA d^{-1}) were calculated).

The nature of the productivity model is such that it is not well-suited to reproducing short-term (transient) phytoplankton physiology-driven demographic responses to brief environmental fluctuations (eg upwelling events), however, with judicious parameterisation, we believe that it should

be able to reproduce seasonal-scale plankton dynamics. This level of temporal resolution appears adequate to the issue at hand. We do, however have some reservations regarding some aspects of parameterisation and formulation (see sections 3.2.2-3.2.4, below).

We find the descriptions of the methods by which individual mussel clearance rates and subsequent volume-specific clearances rates to be entirely inadequate (see section 3.2.5, below) – though it is clear that the former are calculated using a model that has been superseded.

1.2.2 Assess the methodology and robustness of the hydrodynamic and productivity models

The method by which instantaneous photosynthetic rates are calculated from instantaneous PAR relies upon a poor approximation (see 3.2.2, below). It will have resulted in biased estimates of primary production. The nature of the bias may have varied in both space and time. This will certainly have influenced predictions of phytoplankton standing stock, but may not have dramatically influenced predictions of farm-induced relative change. Some other details of the production-model's parameterisation and formulation are a little unusual. The report does not provide the information that would be required to determine whether these will materially influence predictions of farm-influences.

The report presents only incomplete evidence to demonstrate the performance of the calibrated model, and has not been subjected to validation tests against independent field-data.

Whilst we agree with the authors that their model is reproducing the spatio-temporal dynamics of phytoplankton (in the absence of farms) in the calibration data moderately well, we also believe that there is evidence that it is systematically over-estimating chlorophyll concentrations in the near-surface. As noted previously, the authors of the biological report failed to properly describe the means by which they calculate mussel-clearance, but the over-prediction may induce an under-estimation of estimated depletion. There are two reasons for this. Firstly, their clearance-model appears to have been based upon data which indicate mussel clearance rates are maximal at phytoplankton concentrations which are less than those simulated (and closer to those observed). Secondly, there is also some evidence in the calibration-plots that that the model yields an overly dispersed vertical distribution of phytoplankton. This will have lead to the model under-estimating mussel-induced plankton depletion since a smaller fraction of the simulated plankton will have passed through the farms that may occur in reality.

1.2.3 Make a statement on the confidence that can be placed on using these to make assessments of aquaculture impacts on the productivity and sustainability of the Bay of Plenty's coastal waters

Despite the aforementioned reservations, we believe that forecasts of farm-induced change stemming from the model will be *qualitatively* correct (subject to the hydrodynamics being adequate). We

suspect that the forecast levels of change may be under-estimates, but we are uncertain of the magnitude of under-estimation.

Assuming that the forecast levels of plankton change are approximately correct, we concur with the conclusions expressed in Section (4) *Ecological Implications* of Longdill et al. (2006).

1.2.4 Provide comments on the reports in general, or aspects of the work that would appreciably benefit from further development or alternative approaches.

We believe that the model's sensitivity to key details of biological model's formulation and parameterisation (*cf* sensitivity to hydrodynamics) needs to be examined and presented.

The authors of the biological modelling report have shown only comparisons between observed and modelled chlorophyll in support of their claim that the model is behaving adequately. Even this comparison is only for a calibration period. Further evidence should be provided. In particular, we suggest the report should contain evidence that:

1. The model is able to reproduce the magnitude and location of the relatively tight summertime 'deep chlorophyll maximum' that is evident in the field data. Given that the mussel farms occupy only a restricted part of the water-column, it is important that simulated vertical distribution of the phytoplankton biomass be correct relative to the mussels.

2. Observed nutrient concentrations are adequately reproduced by the model.

3. Simulated zooplankton dynamics are plausible.

Whilst it will not be possible to verify the performance of the full model (including the farms) for the Bay of Plenty region, the productivity component could usefully be verified. The remote-sensed chlorophyll data that have been analysed for this region provide one data-set that could be used for this.

1.3 Closing summary

None of our reservations stem from a belief that either the models are fundamentally unsound. For the most part, they have arisen because key pieces of information are absent from the ASR reports.

Whilst we suspect that the forecasts of farm-induced plankton change may be under-estimates, we are satisfied that they are qualitatively correct. Based upon the evidence provided in the report, we cannot offer an assessment of the quantitative accuracy of the forecasts, but assuming that the forecast

magnitudes of farm-induced change are not serious under-estimates, we concur with conclusions expressed in Section (4) *Ecological Implications* of Longdill et al. (2006).

If further effort (in addition to that required to address the issues raised in this review) is to be devoted to better determining the impacts of large-scale aquaculture in the Bay of Plenty, we suggest that this would be better directed towards gathering additional field-data. Such data would be useful for two purposes. Firstly, it would help to build a more complete picture of the 'natural, pre-farm state of the system'. This will prove valuable when endeavouring to interpret corresponding monitoring data that may be gathered should the mooted farms be established. The data would also prove valuable should further numerical modelling become warranted in the future.

1. Introduction

Environment Bay of Plenty (EBoP) commissioned NIWA to review the two reports:

a) Longdill, P. & Black, K.P. (2006). Numerical hydrodynamic modelling: aquaculture management areas. (Draft)

b) Longdill, P. et al. (2006). Bay of Plenty Primary Production Modelling: Aquaculture Management Areas; Primary production modelling, and assessment of large-scale impacts of aquaculture management areas on the productivity within the Bay of Plenty (V. 2, Revision A).

Both reports were provided to NIWA in draft-form by Environment Bay of Plenty.

These reports have been prepared for Environment Bay of Plenty by ASR limited who were commissioned to establish a program of field-data collection and develop/implement numerical models of hydrodynamics and nutrient/plankton dynamics for the coastal waters of the Bay of Plenty. Information stemming from these two sources will contribute to decisions regarding management of these coastal waters – particularly with respect to the designation of Aquaculture Management Areas.

EBoP required that our review:

- Assess the assumptions used for the basis for the model.
- Assess the methodology and robustness of the hydrodynamic and productivity models.
- Make a statement on the confidence that can be placed on using these to make assessments of aquaculture impacts on the productivity and sustainability of the Bay of Plenty's coastal waters.
- Provide comments on the reports in general, or aspects of this work that would appreciably benefit from further development or alternative approaches.

We aim to full-fill those requirements within this report. Our report consists of: (i) a brief re-iteration of the environmental issues associated with aquaculture; (ii) a review of the report concerning hydrodynamics (Longdill & Black 2006) together with

relevant recommendations; (iii) a discussion of the report concerning plankton production (Longdill et al. 2006) together with relevant recommendations.

1.1 Environmental issues associated with aquaculture

Cole (2002) provides a review of the environmental impacts associated with aquaculture. These include: changes in the physical, chemical and biological properties of the water-column and the underlying sea-floor. Direct physical changes may include: changed wave-climate, local modification to currents and patterns of vertical mixing, provision of new habitat (the farms themselves). Secondary changes may include changed local-scale seabed characteristics (due to changes in nearbed currents, and deposition of shellhash etc.). Chemical changes may include changed patterns of nutrient cycling (addition of particulate food for finfish farms, increased availability of reactive dissolved nitrogen (due to excretion from the crop)).

The aquaculture assessments performed by ASR Ltd. consider only shellfish aquaculture. These crops consume components of the naturally occurring seston (phytoplankton, micro-zooplankton (protozoa) smaller meso-zooplankton (including eggs, nauplii and perhaps also copepodite stages of crustacean zooplankton) and the planktonic egg and early larval stages of larger organisms such as some fish and wild-stock shellfish. Both crop shellfish and larger marine organisms such as fish and wild-stock shellfish rely directly or indirectly upon plankton for their food. Thus, all parties have an interest in ensuring that the standing stock and composition of the phytoplankton community will not change unduly following the development of large-scale aquaculture.

2. Hydrodynamic modelling

Whilst some plankton are motile, all are strongly influenced by hydrodynamics. Current-driven transport will have a strong influence upon the spatial-extent and location of any 'plume of change' induced by the farms. Furthermore, it is well-established that the net per-capita growth rates of phytoplankton is strongly influenced by the statistical characteristics of the vertical mixing processes (depth of the mixed-layer, strength of stratification, frequency of mixing events (Huisman et al. 1999). Indeed, in a recent comparison of 1D plankton models applied to the Arabian Sea Friedrichs et al. (2006) concluded that forecast dynamics are more sensitive to the nature of the physical forcing than to changes in the details of the biogeochemical model. If numerical models are to be used to assess farm-effects upon plankton production and standing-stock, a robust hydrodynamic model is a fundamental requirement. The hydrodynamic model will need to reproduce patterns of vertical mixing in order that phytoplankton growth rates can be simulated adequately, and it will need to reproduce both short-term and medium-term (ie sub-tidal scale-, and residual-) currents adequately if the spatial extent and location of plumes of plankton change are to be adequately forecast.

2.1 Model assumptions

The assumptions used for the basis of the hydrodynamic model are reasonable. The domain is sufficient to enable generation and development of wind-driven residual currents. The methods used for setting up the initial and boundary conditions are satisfactory, and the temperature nudging scheme keeps surface temperatures stable in the stratified model. The hydrodynamic model takes no account of the influence that farm-scale structures will have upon current patterns and mixing – however these influences will be restricted to the immediate vicinity of the farm. Developing a better understanding of the influences – and of their consequences for farm effects upon plankton is at the cutting edge of scientific research.

2.2 Methodology and robustness

The robustness of the hydrodynamic model is unavoidably compromised by the difficulty in accurately specifying open boundary conditions. Numerical modelling of open-coast hydrodynamics is notoriously difficult, because large-scale oceanic processes can propagate into the area and dominate over local processes and the easily-predicted tidal motion. It will not be possible to exactly reproduce the hydrodynamics on a domain of this size without extensive boundary information that was not available – though they could be derived from larger-scale hydrodynamic

models. Thus, although the model cannot reproduce much of the short-term variation, the reproduction of the general currents trends in the *calibration* is *encouraging*. It is also encouraging that the model improves with the inclusion of stratification – this demonstrates that the coarse features of the density field and its interaction with currents are being represented. The reproduction of the temperature field is good during the calibration period and clearly benefits from the nudging scheme used. Salinities appear reasonable based on the limited dataset available.

A criticism of the calibration is that the agreement between the model and the calculation results is simply "eyeballed". There is no formal comparison of the results of the calculation with the field data so that there is no statistical measure of the "goodness of fit". For example, it should be possible to make specific comparisons of the temporal record of temperature and flow between the field measurement locations and the data generated by the modelling exercise, such as scatter-plots with linear fits to illustrate the correlation between observation and simulation. My impression from the data presented in Figures 26 and 27 is that such a formal comparison would not be very complimentary to the modelling. The "eyeballing" technique is useful, but the authors should temper their enthusiasm when making unqualified statements such as "… the modelled currents replicate the measured currents accurately…", when clearly the model misses almost all the high-frequency events and at best reproduces the general current trends. Thus, the authors need to be clear about how they derive their conclusions and be honest about the limitations of the modelling.

In my opinion there are also inaccuracies in the interpretation of the data as "eyeballed". For example, in section 4.5 the authors suggest that northwesterly flows on 7, 16, 27 Oct 2003 and 19 Nov 2003 are not associated with longshore wind stresses. Comparing Figures 10 & 11, to my eye, the last three of these events are clearly associated with northwesterly wind events measured by QuikScat. The hydrodynamic response lags the wind forcing by 1–2 days, as might be expected. In support of this argument, there are other southeasterly events, e.g., ~30 Sep, 21 Nov that seem to show an obvious relationship between alongshore wind stress and alongshore currents.

The authors observe that it is "standard practice" to undertake a model *verification* following calibration, but have not presented such. Verification gives confidence that the model can correctly reproduce the hydrodynamics at other times when background densities and forcing conditions might be quite different. In the context of this report, the presentation of verification data would give confidence that the calibration period has not simply been chosen because it represents the period when the model performed best. The authors claim to have undertaken such verifications elsewhere,

and the biological model was coupled to a 1-yr hydrodynamic simulation, thus it should be easy to reproduce verification data for this report. I would suggest that verification data be shown from a different season, such as the late summer/autumn when stratification is most strongly developed. This extra information should be presented in the report.

The report would benefit from general attention to detail. For example, many of the references on p. 9 and in other areas of the report are missing, Appendix 1 (referred to on p. 18) is missing, page numbers are missing after p.20. In section 4.3.2 northwesterly flows are specified as positive alongshore, but in section 4.4 they are specified as negative alongshore – this information should be presented in the figure captions to aid their interpretation. Although not major, these inaccuracies are frustrating to the reader and indicate a lack of care.

2.3 Statement on confidence

While the general trends shown in Figures 26–29 are encouraging, we cannot have confidence that the hydrodynamic model is reproducing the key parameters of importance to the biological modelling without further information, such as proper verification, residual current plots, and representation of thermal stratification in the model (e.g., see below). Most of this extra information should be readily available from the work done so far and able to be supplied at little effort.

2.4 Further development

The hydrodynamic modelling would appreciably benefit from the following approaches that could be made at little effort.

Verification data should be included, as discussed above.

The report would benefit from better links to other aspects of the study, the biological modelling in particular. A section is needed that outlines why the hydrodynamic modelling was undertaken (to force the biological model), the key outputs of the hydrodynamic model to the biological model, and the sensitivities of the biology to inaccuracies in the hydrodynamic parameters. Then, in the conclusions, draw attention to how the model has performed in context of these key outputs.

The biology is particularly sensitive to the effects of residual currents and to temperature stratification, with the representation of stratification and vertical mixing being more important than small inaccuracies in absolute temperature. Figure 20

demonstrates how the thermocline depth varies in the Bay of Plenty, from measured data. Since this is a key parameter for the biological model, the model's representation of the thermocline should also be plotted alongside, and would provide a useful measure of model skill over an annual basis.

More emphasis should be placed on exploring the degree to which the model reproduces the residual currents, as these are difficult to judge from plots such as Figures 26 and 27, but are fundamental to the biological results. Cumulative vector plots provide a clear indication of residual drift, and these could be plotted at small effort for both modelled and measured data. These plots should also form part of a verification section.

It would also be instructive to draw some general conclusions about the hydrodynamics on this section of coast that may be of relevance to the biological application, such as flushing times through farm areas.

3. Productivity modelling

3.1 Statement of confidence

Considered in isolation, the productivity modelling report (Longdill et al. (2006) does not provide sufficient evidence to convince us of its conclusions. Nonetheless, it is our opinion that, in principle, the model 3DDLife is capable of yielding qualitatively accurate maps of the spatial distribution of farm-induced impacts upon the plankton ecosystem – provided that it is driven by appropriate hydrodynamics (see preceding section) and other reservations (see below) are properly dealt with. We suspect that the magnitude of farm-induced change may be under-estimated (see below).

Assuming that the simulated magnitudes of change are not substantially under-estimated, we concur with the conclusions that are drawn under Section (4) *Ecological Implications* of Longdill et al. (2006).

We present a more complete discussion of reasons behind our 'statement of confidence' in later sub-sections of this report, but in brief:

i. The model has not been verified against independent field data and the comparison with calibration data is incomplete.

ii. Some aspects of the parameterisation of 3DDLife are unusual, and one aspect of its formulation is biologically implausible.

iii. We also have some reservations concerning: (a) the choice of mathematical description for the rate at which mussels filter sea-water, and (b) the plankton model's ability to reproduce plankton dynamics in a highly dynamic environment.

Despite all the aforementioned, we are inclined to accept that, if referring to model performance in the absence of farms, Longdill et al. are correct when they state that '*little improvement to the model may be achieved without further field data*', - though we believe that it may be possible to achieve a similar performance with a less unusual parameterisation.

3.2 Model assumptions

ASR refer to their biological model as 3DDLife and offer a mathematical description of the model in Appendix 1 of their report. Surprisingly, they provide no indications of the model's origins. It is not clear whether it is a re-implementation of previously existing model, whether it is a derivative of one or more pre-cursor models, or whether it has been developed from scratch. This should be made explicit so that (i) interested readers can delve into the appropriate literature to learn more concerning the model's properties, and (ii) (if appropriate) due credit is given to the original authors of the model.

It is worth noting that, the field of biological modelling is less well developed than that of hydrodynamic modelling. In the case of hydrodynamic modelling, there is collective agreement over what mathematical equations must be solved. Debate instead focuses upon the numerical schemes which are used to solve the equations, and 'pragmatic issues' such as availability of suitable forcing data. In contrast, there is no collective agreement amongst biologists about what constitutes the appropriate set of mathematical equations to describe the biological system. Certainly, most plankton models share similar underlying assumptions, but they differ in both superficial and more fundamental ways. In ascending order of magnitude of difference the spectrum is as follows:

1) common mathematical formulation, but differing parameterisation;

2) differing mathematical forms to describe particular processes. In some instances, the differing forms yield functions of qualitatively similar shape; in others the qualitative shape of the response differs;

3) differing choices regarding which characteristics/processes are explicitly represented.

In the context of the model adopted by ASR, examples of level (2) differences include:

a) adoption of the Steele (1965) description of the photosynthesis/irradiance relation in preference to other similarly 'static' descriptions such as the Smith (1936) one;

b) adoption of a harmonic mean nutrient-limitation term in preference to alternatives such as Liebig-limitation (most-limiting nutrient limitation);

c) adoption of an Ivlev-type description of the zooplankton functional response in place of alternatives such as a Michaelis-Menten one.

Examples of level (3) differences include:

a) the decision to represent the entire phytoplankton community using just a single state-variable (rather than distinguishing between differing size-classes or taxa – each of which have differing physiological characteristics);

b) the nature of the mortality 'closure term' at the 'top' of the food-web (in the case of 3DDLife: an explicit dynamic zooplankton grazer population which suffers a prescribed (first-order) mortality, rather than choosing to extend the model higher into the food-web, or to curtail the dynamic representation at the level of phytoplankton. The nature of this closure term can have a dramatic influence upon a model's dynamics (Steele & Henderson 1992);

c) adoption of fixed-stoichiometries in the phytoplankton (and zooplankton). In reality, the elemental-ratio within an individual phytoplankton cell can vary several-fold. When environmental conditions are favourable, cells have the ability to consume carbon and nutrients in excess of their immediate growth requirements. Substantial stores can be accrued in a matter of hours and these can fuel continued growth during subsequent short-term (~days) unfavourable periods;

d) the decision to adopt a 'static' representation of the photosynthesis/irradiance (P/I) relationship. In reality, individual phytoplankton cells have some capacity to adapt to ambient light levels. At low light-levels, those which have experienced low light for some time (hours) will exhibit higher carbon fixation rates than those which have previously experienced high light levels. Conversely, if low-light adapted cells are suddenly exposed to higher light-levels, their photosynthetic rate will not immediately rise to match that of high-light adapted cells. Indeed, over the first few hours, progressive development of photo-inhibition may lead to a fall in their photosynthetic rate. Subsequently, the low-light adapted cells will recover and become adapted to high-light conditions.

Given that there is no collective agreement over what constitutes the appropriate mathematical description of the dynamics of a plankton system, it is important that the attributes and limitations of any one description are clearly spelled out so that non-specialists can easily form an opinion as to the suitability of the chosen formulation to

the questions at hand. Whilst Longdill et al. (2006) have made explicit some of the key assumptions of their model, they have failed to discuss the implications of many of these. To this end, we offer the following comments on the plankton and mussel components of 3DDLife.

3.2.1 3DDLife: formulation and parameterisation

Relative to many recent models of plankton dynamics it is simple. Nonetheless, given the level of detail that it does include, the mathematical formulations that have been adopted are not unusual. Simplicity is often a desirable attribute in models, and the important question remains: "is the model adequate for the stated purposes?". With some provisos (discussed below), we believe that it is.

Like any model, 3DDLife is based upon simplifying assumptions. In our opinion, the key assumptions are:

a) having adopted a fixed phytoplankton stoichiometry, and 'static' P/I curve;

b) having only one class of phytoplankton;

c) the particular functional form chosen to describe mussel clearance;

d) it is possible that some of the formulation and parameterisation details of the zooplankton model are also crucial, but the report does not provide sufficient information to permit us to assess this.

The latter two assumptions will be discussed later in this report (sections 4.2.4 and 4.2.5). Here, we will discuss only the first two. The first assumption implies that whilst the model may be capable of performing well during periods when individual phytoplankton cells are likely to experience near constant conditions, the model will certainly perform less well whenever cells experience rapidly changing conditions. In particular, we anticipate that it should perform best during periods when the water-column has a well-developed, shallow mixed layer. We anticipate that it will be less capable of performing well during- and in the days immediately following transient upwelling/mixing events. Nonetheless, the model has a large number of calibration parameters, and provided that the frequency of disturbance events is similarly (in space and time) low relative to the time-scale of biological change (so that transient-time represents only a small fraction of the total time), or much shorter than the time-scale of biological change (so that, in a sense, the plankton perceive the system to be 'stable', and parameters appropriate to this pseudo-stability can be chosen by

calibration), the model should perform adequately. The proviso that the frequency of fluctuation should be similar throughout the domain if calibration is to adequately compensate for the model's lack of physiological/behavioural plasticity, provides an *a-priori* reason to suspect that the predictions of plankton change in the immediate downstream of farms may sometimes be no better than semi-quantitative.

With respect to the second assumption, we note that the phytoplankton community of many coastal waters often exhibit a well-documented switch from domination by relatively large diatoms when nutrients are plentiful (winter and spring) towards domination by smaller diatoms, phytoflagellates and dinoflagellates when nutrients are scarce (summer/autumn). Each taxa has differing physiological/behavioural characteristics. Some of these characteristics may influence their response to farms (maximum growth rates, nutrient requirements, motility). Thus, the differing taxa may show differing responses to large-scale aquaculture.

The fact that 3DDLife has only one phytoplankton class has two implications. Firstly, it implies that the model cannot be used to address questions of related trophic change arising from changes in the composition (*cf* total abundance) of the phytoplankton community. There is good evidence that differing phytoplankton taxa have differing nutritional value to their grazers (Ban et al. 1997; Kleppel 1993), that changes in the phytoplankton community can drive changes in the zooplankton community and perhaps even in higher trophic levels (Zeldis et al. 2005) and that large-scale aquaculture can induce changes in the composition of the phytoplankton community (Broekhuizen et al. 2004).

Secondly, and, perhaps more importantly, whilst having only one phytoplankton class reduces the number of parameters required by the model, it makes the problem of selecting suitable parameter values more difficult – because the appropriate value will change as the composition of the community changes through time and/or space.

Having made some general comments concerning the model, let us turn to some specifics.

3.2.2 Phytoplankton growth

On page 41, it is stated that: *This equation is integrated over depth and the mean value for the depth layer in question is used.* It is not entirely clear from the phrasing, but I infer that a layer-average light intensity for the layer in question is substituted into Eq. (5) to derive an instantaneous light-limitation term. This makes for a poor approximation. McBride (1992) provides an alternative, biologically more plausible

formulation. The discrepancy between the two formulations is especially important because: (i) the model's layers are relatively thick – implying that the layer average light intensity will be a poor approximation to depth-specific intensities, and (ii) Eq. 5 is highly non-linear. Given that Eq. 5 is not an asymptotic function of light intensity, it is unclear whether the formulation error will have led to depth-averaged primary production being over-estimated or under-estimated. It is probable that realized photosynthetic rates will have been over-estimated in some places/seasons, and under-estimated in others. It seems likely that forecasts of standing crop will be more sensitive to the nature of this description than will forecasts of relative depletion.

The description of the light-attenuation model (Appendix 1) implies that 3DDLife does not consider differential absorption of 'red' and 'green' components of light; however, Table 2.2 lists separate long-wave and short-wave 'decay' constants – suggesting that differential absorption may be accounted for. This confusion needs to be removed, and if a ‘two-band’ model has been used, the wave-lengths associated with each band should be specified. The attenuation coefficients listed in Table 2.2 are indicative of relatively clear water (assuming that the units for this quantity are m^{-1} rather than N/A as stated in the Table). This appears plausible for much of the Bay of Plenty, but one might expect higher values in the immediate vicinity of river mouths and close inshore. It appears that the light-attenuation model takes no account of absorption by chlorophyll. This needs to be made explicit.

We are mildly surprised that the 3DDLife model incorporates an assumption that net growth rate is determined by the product of the light-limitation term and the harmonic average of the nitrogen and phosphorus limitation terms. Adopting a harmonic average nutrient-limitation term, implies that a plentiful supply of the less-limiting nutrient can (partially) offset growth-restriction by the more limiting nutrient. For this reason, use of the harmonic average is unusual (but not unheard of). This implies that realised net growth will be slower than that implied by the light-limitation or nutrient-limitation terms operating individually.

3.2.3 Zooplankton Growth

3DDLife incorporates an assumption that zooplankton will cease feeding when phytoplankton concentrations fall below a prescribed threshold (which was determined by calibration, Table 2.2). This assumption is commonly adopted in models in order to prevent the zooplankton grazing the phytoplankton down towards absurdly low concentrations and initiating violent 'predator-prey' oscillations. It is, however worth noting that whilst there is a little evidence that some copepods exhibit feeding

thresholds (for example, references cited in Hansen et al. (1997)), other significant components of the zooplankton (notably protozoa) do not (Strom et al. 2000).

Should feeding by mussels reduce phytoplankton concentrations below the zooplankton feeding threshold, grazing upon the surviving phytoplankton will cease temporarily and the recovery time of the phytoplankton population will be reduced. Conversely, the recovery time of the zooplankton population may be increased (but see next paragraph). Thus, model predictions may be sensitive to the value of the threshold parameter (see also, the ensuing section for comments on the numerical value chosen for this parameter).

3DDLife incorporates an assumption that zooplankton respiration falls to zero when the consumption rate falls to zero (Eq. 17, 18 & 19). This assumption might be considered valid for protozoal grazers (Caron et al. 1990; Fenchel & Finlay 1983; Verity 1985), but is not appropriate to multicellular zooplankton such as copepods. There is therefore a minor inconsistency in that the grazing function might be considered representative of metazoan zooplankton whilst the respiration function is more representative of protozoal zooplankton. More importantly, because mussels consume phytoplankton (perhaps to the extent that, in the model, those zooplankton which survive passage through the farm will cease feeding), 'starvation' losses amongst the zooplankton population downstream of the farm may be under-estimated. Consequently, zooplankton population-recovery will be too rapid, and phytoplankton recovery too slow.

Longdill et al. (2006) do not explicitly state what elemental composition they have assumed their zooplankton to have, but the equations imply that they share the same composition as phytoplankton. This is acceptable, but should be made explicit.

3.2.4 Parameterisation

Most of the parameters that have been adopted in the 3DDLife model are unexceptional, but we note the following:

a) A large proportion of the parameters were derived by calibration rather than from independent literature (from Table 2.2: 17 of 29; assuming that those for which no literature references are provided were estimated by calibration even when this is not made explicit).

b) Given the scarcity of field data for some of the model characteristics, we question whether the parameterisation is robust. Whilst the report considers a

variety of hydrodynamic scenarios (wind/season etc.), there are no details of trials aimed at investigating the model's sensitivity to biological parameters.

c) Modelled phytoplankton and zooplankton are measured as g DW m^{-3}. Field measurements of phytoplankton abundance were made as mg Chl-a m^{-3}. Longdill et al. (2006) acknowledged that a conversion was necessary, but failed to mention of the conversion factor that was used. In practice, the appropriate value can vary several-fold dependent upon the taxonomic composition of the phytoplankton community, and the physiological state of individual cells. (EPA 1985).

d) The *Phytoplankton maximal non-predatory mortality* (0.7 d^{-1}) seems high. Mortality rates are notoriously variable and difficult to measure. For the Hauraki Gulf, shipboard incubation studies have yielded estimates of total (ie, inclusive of predation) mortality ranging from less than 0.3 d^{-1} to more than 1 d^{-1} (J.A. Hall, NIWA, *pers. comm.*). Models which include explicity include dynamic grazers (such as 3DDLife) often ignore non-predatory mortality altogether (implicitly setting this term to 0 d^{-1}).

e) The *Threshold phytoplankton concentration below which feeding* (by zooplankton) *does not occur* has been set to 0.1 g m^{-3}. This may be an over-estimate of the concentration at which clearances ceases: Reeve & Walter (1977) (and references cited therein) suggest that clearance rates may start falling at around this concentration. We wonder whether the authors found themselves being forced into adopting a high value for the non-predatory phytoplankton mortality term in order to compensate for under-estimating the predatory load suffered by the phytoplankton (but see also the next point)?

f) Given the threshold concentration for feeding (P_T =0.1 g DW m^{-3}), and the half-saturation constant for zooplankton grazing (K_Z =0.05 g algal DW m^{-3}) that zooplankton achieve a weight-specific feeding rate that is equal to 50% of their maximum at a food concentration of 0.15 g DW m^{-3}. In a review of the zooplankton feeding literature, Hansen et al. (1997) concluded that the average half-saturation constant was approximately 240 mg C m^{-3}. Converted to carbon, the 3DDLife half-saturation concentration is approximately 30% of this value (though still comfortably larger than the minimum value reported by Hansen et al. (1997)). For a given food concentration, (zooplankton-) weight-specific feeding rates increase as the half-saturation constant falls. We wonder whether the high feeding-threshold (preceding point) was necessary because the half-saturation rate has been set too low?

g) What assumption was made concerning the vulnerability of zooplankton to consumption by mussels? The reasons that this might be relevant are discussed later in this report.

h) Table 2.1 lists the data which are required as initial/boundary condition data for 3DDLife, but the report contains no description of how these (especially the boundary data) were derived from the relatively sparse field data (what form of spatio-temporal interpolation).

i) We strongly recommend that a new column be added into Table 2.2. This column should list the symbol that has been used to denote each parameter within the model's equations. This would facilitate comparison with the contents of Appendix 1.

3.2.5 Mussel Feeding model – formulation

The description of the mussel feeding/physiology model (see P. 15 and Appendix 1 of Longdill et al.) is inadequate. Several items require clarification:

a) The description of the means by which clearance rates were derived is inadequate. On page 15, it is stated: "Based on these densities and using formulae published in James et al. (2001) and Marsden & Weatherhead (1999) dry mussel weight densities, clearance rates, excretion rates and respiration rates were calculated". Both these sources offer several different parameterisations for clearance and/or excretion, but Longdill et al. (2006) provide no indication of which were used. They should list the mathematical relationships used, and mussel-size-class-specific clearance rates, excretion rates etc. derived from these. Given that: (i) the clearance rate is 'defined by the user' (see *Musssel Feeding Dynamics, Appendix 1*), and (ii) we infer that the model by which values were calculated relies upon a chlorophyll concentration term, what concentration term was assumed? Was this concentration derived from field data, the model data or elsewhere? We note that the data in James et al. (2001) span a chlorophyll of ~0.3 – 1.2 mg Chla m^{-3}. In contrast, Figures 2.2-2.4 indicate that both observed and simulated chlorophyll concentrations at the depths at which it has been assumed mussels will be farmed (15 – 25 m) were often greater than 1 mg Chl a m^{-3} for substantial parts of the year. This raises the possibility the user-supplied clearance rate was based upon an extrapolated relationship. Inevitably, this raises additional questions as to the fidelity of the simulated clearance rates and subsequent patterns of farm-associated change are accurate.

b) Given the observational evidence that mussel clearance rates are influenced by the concentration of particulates in the water-column, were the user-supplied clearance rates season-dependent? Was there any explicit (or implicit) feedback between farm-induced depletion of particulates, and the filtration rates imposed by the mussels of the farms?

c) Whilst the report identifies assumptions regarding individual backbones (p. 15), they fail to state what figure has been adopted for the *number of backbones ha^{-1} of AMA*. Instead, the report refers only to 'planned stocking levels'. Without information on backbone density, it is impossible to calculate the biomass of mussel stock within the AMA (eg tonnes/ha of AMA, mussels m^{-3} of AMA). Thus, it is impossible to put their results into context by comparison with assessments made for other farms (see below).

d) The horizontal resolution of the model is coarse relative to the size of the proposed AMAs. This implies that each AMA occupies only a few horizontal cells in the grid. Given that, in reality, the AMAs are unlikely to have been perfectly aligned with the model's horizontal grid, the authors should clarify the manner in which they treated model control-volumes which were partially occupied.

In addition to the preceding clarification issues, it is worth noting that Ren & Ross (2005) provide a more recent description of feeding, respiration and excretion by Greenshell mussel than has been adopted by Longdill et al. (2006). Ren & Ross developed their model using data from a variety of sources (including that referred to by Longdill). One of the key observations made by Ren & Ross (2005) is that chlorophyll concentration is often a poor predictor of mussel clearance rates. They argue that total seston concentration is a better predictor. Seston is composed not only of phytoplankton, but also other small, suspended particulates. By mass, organic detritus and inorganic silts often dominate the seston. The relative phytoplankton content of seston is very variable in both space, and time.

Without detailed knowledge of seston concentrations, and the chlorophyll content of this seston, it is not possible to determine whether the formulation adopted by 3DDLife will consistently over-, or under-predict mussel clearance relative to the values that would be yielded by the Ren & Ross description.

3.2.6 Numerical details

It is unclear whether 3DDLife is embedded within the 3DD-hydrodynamic model, or whether it is a separate code which takes a time-series of currents, salinities and temperatures (provided by 3DD) as a part of the required physical forcing data. The documentation states that 3DDLife was run with a time-step of 0.01 days (864 seconds, Longdill et al. 2006 Table 2.2) whilst 3DD used a time-step of 10 seconds (Table 3.2.1 Longdill & Black 2006). We infer that 3DDLife is a separate code – but acknowledge that the differing times-steps may merely imply that a time-splitting integration algorithm has been applied within a unified code). If 3DDLife is a separate code, was the temporal resolution of the time-series of currents etc. 0.01 d (the time-step used in 3DDLife), or was it coarser? If coarser, how much so, and what form of interpolation was used?

Longdill et al. (2006) state that the equations of the biological model were solved using an explicit time-step integration scheme. We believe that more details should be provided. What order of accuracy does the scheme have? Using a variety of nutrient/phytoplankton/zooplankton models (some of our own design, others of independent design), we have found that it is not uncommon for state variables representing nutrient concentrations to become negative from time-to-time when fixed time-step, explicit integration schemes are used (even when the scheme is of relatively high order). Negative concentrations are physically implausible, and whilst negativity can always be cured by reducing the time-step sufficiently, it is difficult to select an appropriate time-step *a-priori*. What steps were taken to ensure that 3DDLife did not yield negative concentrations, or to correct such concentrations if they did occur?

3.3 Methodology & robustness

3.3.1 Model calibration

Longdill et al. (2006) have calibrated 3DD life against data from five shipboard surveys made over a period of approximately nine months. Unfortunately, they have not provided any details of the techniques adopted when endeavouring to calibrate the model. Formal optimisation techniques for models having as much complexity as 3DDLife are at the cutting edge of research. We therefore suspect that calibration has been via manual trial-and-error parameter fitting. We do not object to such an approach, but we do believe that Longdill et al. need to be more explicit as to what methods were used. In particular, did the calibration process employ formal goodness-of-fit statistics, or was it merely 'by eye'. What was the relative weighting given to each state-variable?

Longdill et al. (2006) present comparisons of simulated chlorophyll and measured chlorophyll at 4-5 depth-levels at three different sites. They claim that the model '... *reproduced the variations in phytoplankton, both in time and through the water-column...*' and that '... *the scatter in the field data is similar to the scatter between the model and the measurements. The model prediction is mostly (not always) within the range of the 3 sets of* [chlorophyll] *measurements* ...'. They further claim that *'other variables in the model also calibrate equally well',* but show no data to permit the user to verify this statement.

Before discussing their conclusions, it is worth emphasizing that despite the simplicity of 3DDLife (relative to many models of the plankton system), 3DDLife has many poorly known parameters. Calibration is no trivial matter. Furthermore, given the relative simplicity of the model it would be unreasonable to expect that model-data and field-observation will always be in close correspondence.

Given the (temporal) scarcity of data, we suggest that the time-series plots of simulated and observed time-series should be accompanied by scatter-plots illustrating the correlation between observation and simulation. We firmly believe that the report should present comparisons between observed and simulated nutrient concentrations. A table of statistics (for example correlation coefficient and percent-of-variance in data explained by the model for each time-series of data) would be informative. In addition, though there may be no zooplankton data for the region under consideration, we suggest that the report should illustrate the simulated zooplankton concentrations and compare these with estimates of zooplankton abundance typical of coastal waters.

We agree that in a qualitative sense, Figures 2.2-2.4 of Longdill et al. (2006) indicate that model is reproducing the broad-scale temporal dynamics of phytoplankton. The field-data are too sparse to permit comparison of fine-temporal scale behaviours. Both data and model indicate that chlorophyll concentrations tended to be higher in October and December than in March, April and May. Nonetheless, visual inspection leads us to conclude that modelled chlorophyll exceeds observed chlorophyll concentrations in a substantial majority of instances (rather than being *mostly within the range of the three sets of measurements* as claimed by Longdill et al. (2006)). This is especially true in the upper part of the water-column (where the mussels will be farmed). Even after acknowledging the uncertainties associated with converting between algal dry weight (the quantity used in the model) and chlorophyll (the quantity observed at sea) this suggests that the model may suffer from a systematic bias. Depending upon the manner in which the user-supplied clearance rates were calculated (see our earlier request for clarification), this may have resulted in a systematic bias in simulated clearance rates, and hence a systematic bias in estimated farm-associated depletion.

Figures 2.2-2.4 also demonstrate that the model is reproducing the broad-scale vertical distribution of phytoplankton. Unfortunately, this is not enough since the mussel farms occupy only a comparatively narrow depth stratum (15-25 m). The simulation results indicate that phytoplankton biomass is relatively evenly distributed down to at least 35 m (Figures 2.2-2.4). In contrast, there is a weak indication in the field data on those figures that phytoplankton biomass is maximal somewhere in the 5-25 m depth interval during the summer months. This is even more apparent in the data-reports that are held by EBoP (Figure 10 of Park (2005) clearly shows that fluorescence (a proxy for chlorophyll abundance) has a maximum in the 10-25 m depth interval). We therefore suspect that the model is not placing a sufficiently large fraction of the depth-integrated phytoplankton population inside the depth interval where the mussels will be located. This could be verified if plots of the observed and simulated vertical profiles of chlorophyll (inferred from modelled phytoplankton DW and measured fluorescence) were presented. If our inference is correct, the model will have under-estimated the magnitude of depletion that the phytoplankton population will suffer.

3.3.2 Model verification

Confidence in the predictive capacity of a model requires a substantial validation effort. The productivity model has not been verified. There are two aspects to verification. Verification involves applying the calibrated model to an independent data-set. In the context of these investigations, verification would comprise two distinct components: (i) determination of whether the model could reproduce an independent set of spatio-temporal data (in the absence of farms), (ii) determination of whether the patterns of plankton change inferred from comparison of with-farm and without-farm simulations are consistent with field observations of farm-associated change. Subject to having access to further field data, the former is relatively easy. The latter is much more difficult – largely because of difficulties associated with identifying farm effects from at-sea data. Nonetheless, we do know of one case where such a verification has been made (Broekhuizen et al. 2005).

3.3.3 Simulation of farm effects

Farm-associated changes in the plankton community have been inferred by comparison of with-farm and without farm simulations.

Inspection of the false-colour plots in Longdill et al. (2006) reveals many instances in which there are sharp changes in phytoplankton concentration close to the model's seaward boundaries (particularly the eastern one). This suggests that the model's 'internal dynamics' are inconsistent with the boundary conditions. It may be another

example of the previously mentioned tendency of the model to over-predict chlorophyll concentrations relative to data. The observation that the gradients are sharp suggests that the influence of the boundary conditions does not extend deep into the domain – which is encouraging. In some instances the plots of percentage change also show steep declines in percentage change close to the domain edges. This suggests that, were the model's domain larger: (i) the 'plumes' of change would be still larger, and that (ii) close to the boundaries the magnitude of change is likely to be under-estimated. We consider it unlikely that this under-estimation extends deep into the domain, but it might be appropriate to repeat some of the simulations on a larger or smaller domain in order to be assured of this.

In comparison with similar analyses that we have made for farms in the Firth of Thames, the magnitude of change (percentage change) in the immediate environs of the farms is smaller. Several explanations are possible:

a) Differing stocking densities. This explanation could easily have been tested had Longdill et al. (2006) provided information on the density of backbones within the AMAs.

b) Differences in the hydrodynamics of the two regions (in particular, the magnitude of the residual currents in the vicinity of the respective AMAs). To assess this, it would be useful if ASR were to produce vector plots illustrating the tidal residuals.

c) In the Firth of Thames, droppers span a larger fraction of the water-column. In consequence, the water-column has less 'buffer capacity' to offset depletion occurring within the range of depths occupied by crop.

d) Differences in the formulations of the biological models used for the respective investigations. Two distinct models have been applied in the Firth of Thames (Broekhuizen et al. 2004). Initial results from the two models of plankton dynamics revealed similar local-scale patterns of change but marked discrepancies in the far-field patterns of change (Broekhuizen et al. 2004), however after some refinements to one of the models, both now yield quantitatively similar patterns (Broekhuizen et al. 2005). Of the two models used in Broekhuizen et al. (2004), the one which is most similar to 3DDLife has three distinct phytoplankton taxa, each has variable stoichiometry (but no photo-adaptive/photo-inhibitive response). Zooplankton are not explicitly modelled. Rather phytoplankton suffer a prescribed grazing mortality. The mussel feeding model is that of Ren & Ross (2005).

It would be disturbing to all parties if (d) proved to be the explanation of the differing magnitudes of farm-associated change. Whilst differences in the colour-scales used by Longdill et al. (2006) and Broekhuizen et al. (2005, and unpublished results) make comparisons difficult, we believe that the spatial extent of the Bay of Plenty plumes is greater than that found in the Firth of Thames – despite exhibiting smaller near-farm change. This suggests that flushing may be more rapid in the Bay of Plenty than in the Firth of Thames – as one might expect. We therefore speculate that explanations (b) and (c) are the dominant causes of the differences. Nonetheless, we believe that (d) warrants further investigation (particularly with respect to the mussel-clearance model).

It is a little surprising that Longdill et al. (2006) have found depletion to be greatest during late summer (p 29 and elsewhere in their report). Our modelling in the Firth of Thames has suggested that depletion will be greatest in late winter/early spring – a finding that is supported by field experiments (Ogilvie et al. 2003). It has been speculated that, during summertime, losses of phytoplankton to mussels are offset by increased production of the remaining phytoplankton – fuelled by an increased conversion of detrital nitrogen into inorganic nutrient that is mediated by mussel consumption/excretion. Phytoplankton of the 3DDLife model are less able to respond to the increased pool of inorganic nitrogen in the immediate vicinity of the farm because they lack any ability to store nitrogen in excess of immediate growth demands.

3.3.4 Other comments

The report was provided to us in draft form. It will need careful checking prior to finalization. Some cited material does not appear in the bibliography. There are several instances of misspellings and other typographical errors. In Table 2.2 the source (ie calibrated versus literature) for some parameters is not made explicit. In Table 3.1, we suspect that the units are $\mu g\ L^{-1}$ rather than μL^{-1}. The legend to this that table should also clarify whether the percentage change figures are calculated over the entire domain, or over the individual AMA, and over what depth-range the figures were calculated).

There are several instances where we feel that the report does not properly acknowledge sources that have been drawn upon as part of the work. Specific examples include:

- Failure to adequately identify/acknowledge the 'existing reports' (p. 15) that were used to inform the mussel-farm specifications.

- The discussion of thresholds for growth and reproduction of zooplankton (p. 32 of the draft report) appears to be a reiteration of a literature review related to AMA designation in the Firth of Thames (Broekhuizen et al. 2002, p. 79).

- The description of the seasonal patterns of change to the phytoplankton community in the Hauraki Gulf region (p. 33 of Longdill et al. (2006) report)) appears to be drawn almost word-for-word from Broekhuizen et al. (2003, p.63).

3.4 Further work

We believe that, in addition to minor work required to amend typographical errors etc. the two draft reports (Longdill & Black (2006), Longdill et al. (2006)) should be augmented to:

1. Provide answers to the issues of clarification that we have raised.

2. Provide further evidence of satisfactory performance of the calibrated version of 3DDLife (illustrations of observed and simulated nutrient dynamics, vertical profiles of observed and simulated chlorophyll, scatter-plots, goodness of fit statistics as suggested in the preceding text).

3. Though not part of the original project scope (S. Park, EBoP *pers. comm.* 28 July 2006), some discussion of the zooplankton results may be in order. Credence in the model would be increased if the zooplankton dynamics were shown to be plausible. Furthermore, since the higher trophic levels feed mainly upon zooplankton rather than phytoplankton examination of farm impacts upon zooplankton is relevant. In a modelling study, Broekhuizen et al. (2004) showed that the magnitude of farm-effects upon zooplankton was very sensitive to assumptions regarding the relativity between zooplankton vulnerability-to-predation and maximum growth rate. Under some combinations, farms have a larger effect on zooplankton than on phytoplankton.

4. A demonstration of the model's sensitivity (particularly in terms of relative farm-induced change) to key aspects of the biological model's parameterisation and formulation is highly desirable. In particular, we would recommend trials to:

a. examine sensitivity to the functional form adopted for the mussel clearance model;

b. determine sensitivity (of both phytoplankton and zooplankton post-farm responses) to the magnitude of the assumed threshold food concentration for zooplankton feeding and the half-saturation constant for zooplankton feeding.

Given that the biological model has limited capacity to respond to transient events, it might be appropriate to query the results from the hydrodynamic modelling to estimate the statistical characteristics (frequency, duration) of such events in each season and under a range of climatic scenarios and compare these with the time-scales of demographic change associated with physiological adaptation.

The report has focussed on assessing effects at a bay-wide spatial-scale, and the annual temporal scale. If there are concerns regarding change around particular localities, or particular times of year, the existing simulation data could be queried appropriately.

Though it would represent a major piece of additional work, verification of the productivity model is to be desired. Remote sensed chlorophyll data might represent one target data-set, but verification against nutrient and zooplankton data would also be useful.

The biological model is predicting farm-induced changes that are very small relative to the magnitude of natural spatio-temporal change. Should sensitivity trials (as outlined above) support this conclusion, we do not believe that substantial further numerical investigations are warranted at this stage. If additional money is to be spent, we suggest that it would be better allocated towards gathering more 'pre-farming' environmental data in order that a robust 'base-line' can be established. This will prove very valuable when endeavouring to interpret corresponding monitoring data that may be gathered should the mooted farms do come into existence. They would also prove valuable should further numerical modelling become warranted in the future.

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