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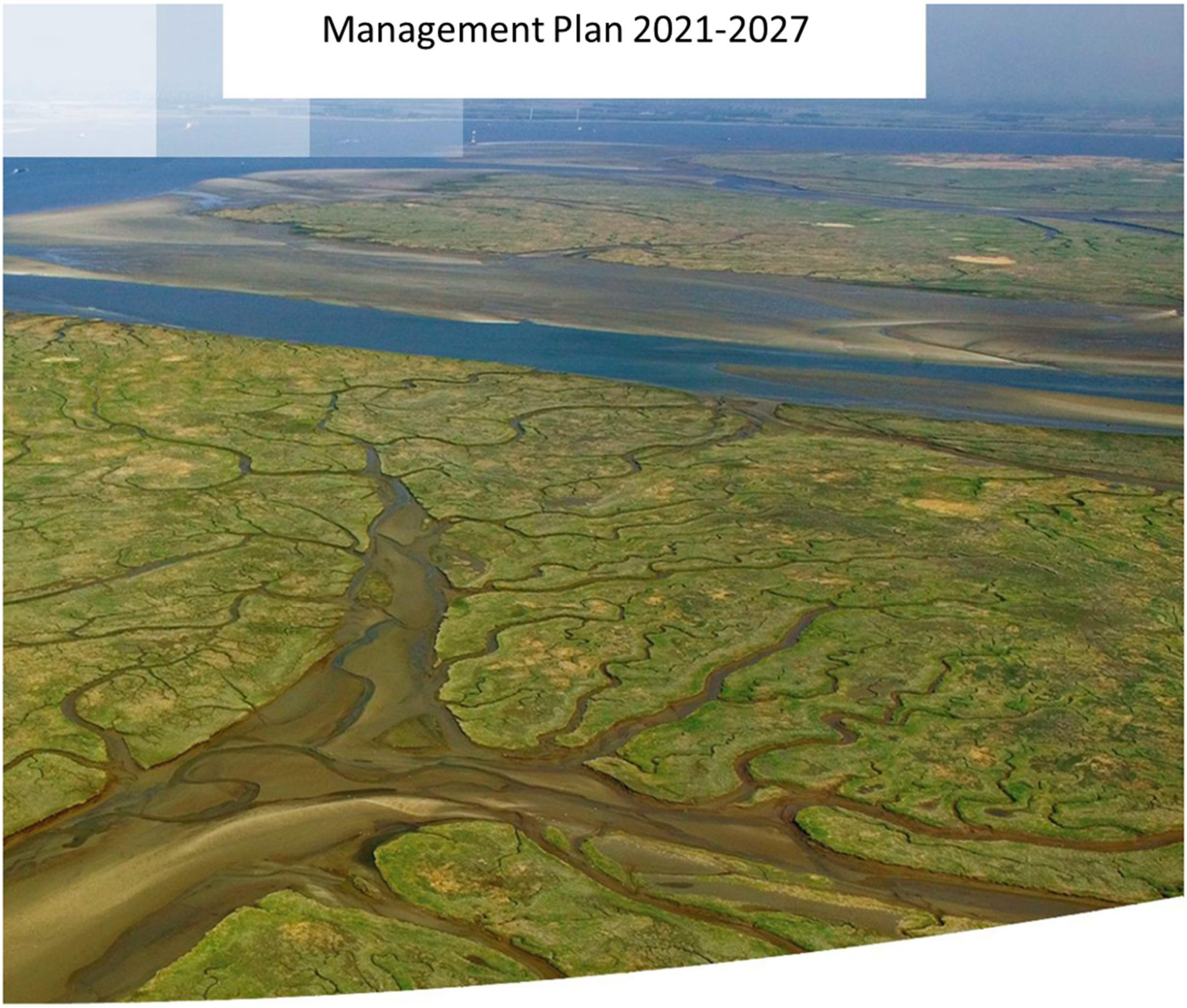
Comments on the "Recommendations for continued development of models and methods for use in the River Basin Management Plan 2021-2027



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Requested by: Ministry of Environment and Food of Denmark

Date: 26-4-2019

Summary

In this report I reflect on the plans for the next River Basin Management Plan 2021-2027. This plan was developed following comments and recommendations by an international Expert Panel on the models used in the previous period. The reflection is based on the documents prepared by the researchers and stakeholders, as well as on the discussions in a workshop with these parties.

Overall, I was impressed by the thoroughness with which the researchers have analyzed the recommendations and reflected on new strategies to solve existing problems. I think that most of the strategies outlined in their document are feasible and well justified. With a refined typology, cross-system modeling and a more transparent methodology for the determination of MAIs, I estimate that more consistent and spatially-resolved estimates of required efforts will be made.

I identify remaining problems in some areas. The choice of a coherent, informative and useable set of indicators has not been entirely clarified. It remains subject of controversy with stakeholders. This point needs further clarification and clear choices.

Commendable steps have been set to give more attention to the roles of both phosphorus and nitrogen as nutrients determining the level of eutrophication in coastal waters. Plans have been developed to investigate options for changed seasonal spreading of nutrient loadings. A full analysis of all theoretically conceivable scenarios of varying N or P reduction, as well as varying seasonal loading, would probably be too vast. It could profit from the definition of a few concise and practically feasible scenarios. It also creates the need for a cost-benefit analysis of different measures for nutrient reduction, that could be taken across different parts of society.

Introduction

Following the recommendations of the International Expert Panel (IE) on the models developed for the previous River Basin Management Plan, researchers from Aarhus University and DHI have written a follow-up report in which they analyse the comments and recommendations of the IE, and propose the general approach and methodological modifications to be implemented in the next phase of the Management Plan (2021-2027). As the chairman of the IE, I have been asked to evaluate these proposals. To that end, I participated in a workshop with stakeholders and researchers (Copenhagen, March 18, 2019). A report of this workshop is attached as an Annex to this report. I received translated copies of the researchers' report, as well as reactions of the stakeholders.

This report contains my observations and evaluations of the work presented in the report, as well as of the discussions during the workshop. For the structure of the report, I have followed the structure of the researchers' report, followed by a short reaction to the stakeholders' remarks. This chronological discussion is preceded by a short general overview of the state of the discussion.

General comments.

I commend the researchers for the thorough way in which they have summarized and reflected upon the many comments and discussions provided in the IE report. I found their summary to be complete and adequate. The reflection on these points was generally very thorough and well thought of. In what follows I will not explicitly mention all the points I agree with but highlight those issues that can still give rise to discussion. I hope that this will not bias the view on my appreciation of the efforts done by the researchers and the high quality of the work invested in this response.

With respect to the future modeling strategy, the follow-up document largely follows the recommendations of the IE. Two parallel modelling strategies will be maintained, one based on statistical modeling and the other on mechanistic modeling. More water systems will have both modeling approaches applied, facilitating comparison and validation of the approaches. Statistical modeling will pay more attention to the cross-system approach. The procedure to arrive at MAI will improve in consistency and transparency. Better uncertainty estimates will be made, based on both mechanistic and statistical models.

The rough typology used in the previous phase will be refined, and research to underpin this change is underway. Close examination of data availability has revealed that IE was probably too optimistic in its estimation that system-specific models could be made for all individual 119 water bodies, but efforts are being devoted to make the estimations of required effort as system-specific as possible, while maintaining similar standards and procedures throughout.

The most difficult points in the recommendations and evaluations of IE concern the following aspects: the detailing of the typology, choice of indicators and their relative weight in the evaluation, consideration of possible effects of Phosphorus (P) alongside nitrogen (N) in the models and in the nutrient reduction scenarios, consideration of seasonal planning of nutrient loading. Many of these aspects are interrelated. For instance, if the choice of the main indicator remains summer chlorophyll-a, which is an intercalibrated and internationally well-established indicator, this may create bias towards the importance of N versus P, as N is usually the limiting nutrient in summer, while P may be limiting in spring. This aspect, in turn, is linked to the problem of seasonally varying the inputs.

It was my general impression that, while many aspects of the future modelling strategy have been well established, there is still uncertainty and some vagueness concerning the choice of indicators and their relative weighing. The discussion is torn between two opposing tendencies. On the one hand, from an analytical point several different indicators provide more information on the ecosystems' response to eutrophication than a single indicator can convey. On the other hand, for normative purposes it may be better to have only few indicators, provided these have a clear relation with the stressor (nutrient input) or with the target (derived from 1900 conditions) and lead to unequivocal calculations of MAI. In their comments, the stakeholders emphasize mainly the second aspect, while the researchers, especially those involved with the statistical modeling, tend to emphasize the first. In the following sections I provide some comments on this tension. I recommend paying sufficient attention to this problem in the near future and aim at reaching consensus on the procedure with the most important stakeholders. The choice of indicators is probably the most sensitive of the remaining uncertainties of the modeling approach.

The researchers are aware of the complexities of nitrogen versus phosphorus limitation of primary production. Their proposals for inclusion of this aspect in the work seem feasible and promising. It can be foreseen, however, that results of the research will also cast uncertainty on who in society can and should contribute most to the nutrient reduction that is needed to obtain good ecological status. I recommend investigating what contributions to nutrient reductions (e.g. use of different types of fertilizer in agriculture, land management, wastewater treatment) are most effective, what their cost is and who is carrying this cost. A basis will be needed to discuss the distribution of efforts across society at some point.

It would also be advantageous to describe some feasible scenarios for how agriculture could change the seasonal pattern of nutrient loading, and/or the ratio between N and P loading. These scenarios will be needed to study the impact of seasonal adjustment measures, without losing track in the vast number of theoretical possibilities that can be thought of. Only scenarios that are feasible from a practical point of view are worth considering.

Detailed comments on the follow-up of the researchers.

1. Typology

IE was probably a bit too optimistic with regard to the data base and the possibility to estimate MAI separately for each water body. The researchers know the situation details better and provide a very reasonable approach: extend modeling efforts to cover more water bodies in detail; improve typology; include dilution models and depth distribution in the typology.

This is a good and realistic approach that combines maximum exploitation of existing data with full geographical coverage. I was pleased to see the inclusion of dilution characteristics, as I think this is a key parameter explaining much of the responses in the different systems.

During the discussion with the stakeholders (also reflected in their written contribution) much emphasis was placed on the consideration of system-specific characteristics and on maximal inclusion of these into the typology. A practical suggestion could be to make a small review of each of the 119 water bodies: what is known about them, what makes them stand out (if so), is special care needed because of this in calculating MAI. This can easily become unwieldy, but I think it is nevertheless a good idea. I propose to limit the effort to a fact sheet of not more than half a page per water system, except when good reasons exist to deviate from the general approach. In those cases (limited in numbers) a separate discussion on consequences can be given.

2. Indicators

The discussion on indicators is still rather vague. Plans are still under development and, judging on the presentation during the workshop, have evolved since the writing of the report. In particular, the development of the macroalgae indicator is now further advanced. With respect to the indicators, there are several important topics to mention.

Summer Chl-a and Kd will remain cornerstones. The first is intercalibrated, responds fast and reliably (with few exceptions) to N loading and has a reliable data base. The second is very important as a proxy for eelgrass development and has a historic reference that cannot be undervalued.

With respect to Chl-a there is little discussion on its usefulness and on the need to continue giving it a prominent place in the evaluations and calculation of MAI. The main point of discussion is whether to use only summer Chl-a or also include spring bloom values. The intercalibration of summer Chl-a is probably more important than other considerations, hence I support the continued use of summer chlorophyll-a. If P is important in a water system and excess P loading leads to very strong spring blooms, this will likely also show in summer (accumulation of organic N and remineralization during summer). Nevertheless, I recommend exploring the relation between spring and summer Chl-a, and in case spring Chl-a contributes significantly to the interpretation of ecological response to nutrient loading, aim for intercalibration of this indicator as well.

With respect to Kd the discussion is more complicated and many elements have been mentioned in IE and in the response. For me the most important aspect is that there is evidence that Kd does react to eutrophication, but not at a yearly time scale. That results in difficult estimation of the slope value (change in Kd per unit change in N load) because time series are relatively short and year-to-year variation is relatively important in the total variance (hence also in the statistical models). In the mechanistic models Kd is one of the most difficult variables to model, as it depends on elements (resuspended sediment, vertical mixing of fine mud particles, colour of the water, fluffy organic matter in the system) that are themselves quite difficult to model precisely. In addition, the response of Kd to nutrient loading may be too slow for the modelled time period to show clear responses. An alternative approach may be needed, but there are opportunities as the 1900 values are known (in principle). One could compare actual values of Kd to 1900 values and estimate the slope by dividing difference in Kd by difference in nutrient loading (an interesting question is whether that should only be N, or some N/P combination). After setting target values for Kd (1900 values times a factor), the required nutrient loading reduction could also be estimated. Note that this approach does not allow for testing the significance of the relationship. However, decadal values in recent decades could be used to validate the relationship. In addition, inter-system comparison may prove worthwhile, as can be done in a regression approach across sytems.

I consider the occurrence of nutrient (N) limitation as a redundant indicator. There can be no correlation between chl-a and N load if nitrogen limitation does not play a significant role. This aspect should therefore already be covered in the chla-loading relation and inclusion of this nutrient limitation indicator is likely to convey little information, but potentially quite some noise.

A few of the proposed ancillary indicators actually function as *modifiers* of the main (chl-a, Kd) indicators. Oxygen depletion susceptibility and benthic grazer dominance are examples.

Oxygen depletion is one of the key ecological indicators, as far as tangible consequences of eutrophication are concerned. There is no disagreement with the stakeholders on the importance of avoiding oxygen depletion events. However, I think oxygen depletion is less predictable, especially on short time scales (years) than chl-a. Events also depend on weather patterns. A probabilistic approach (How susceptible is a system to oxygen depletion?) can be established at decadal time scales (and compared, at these time scales, to nutrient loading and chl-a concentration). It could be considered to include susceptibility to oxygen depletion into the typology, leading to a type of systems that would require stricter limits on nutrient loading because they have physical characteristics (depth, vertical mixing) that makes oxygen depletion more likely.

Benthic grazer dominance *could* be another example, if it is mainly dominated by habitat factors (e.g. availability of suitable substrate) and not by energetic constraints. In the first case, benthic grazer potential would be a system-specific feature that can be incorporated into the typology. In the latter case, one would expect non-linearity between nutrients and chlorophyll to arise (high nutrients -> high biomass-specific primary production -> high benthic grazers -> lowering of chlorophyll compared to ungrazed systems: likely a sigmoid response between nutrients and chl-a, expected to occur across many comparable systems). In any case, the occurrence of benthic grazer dominance could be a factor leading to a modification of the functional relationship between nutrient input and chl-a. It could lead (if properly justified) to a modification of the target value calculation, while chl-a could remain the main indicator to be used.

3. N versus P

The researchers propose an exploratory strategy, that takes into account the full complexity of nutrient limitations and seasonal patterns in Danish waters, as well as the diversity amongst those waters. I endorse this exploratory strategy. It also involves improvement of the mechanistic models on critical features.

With respect to future strategies, this is not just an intellectual discussion on the best model to predict chl-a concentrations. Ultimately, the aim is to achieve the best possible ecological status with the least efforts. Some of the measures to reduce N loadings may also result in reduction of P loadings as a side effect. Stakeholders also argue that not all possibilities to reduce P loadings from point sources (wastewater) have been realized. I cannot judge on these options, but I think that a thorough inventory of different methods to reduce nutrient loadings, including simultaneous N and P loading reduction, is needed. The researchers' plan to investigate to what extent this would affect the ecological status of the coastal waters, would be ideally complemented by such a research.

4. Meta models

The researchers propose a realistic strategy for the metamodeling, considering data availability and modeling possibilities. I do see, however, some danger in mixing a regression-based and typology-based approach. If this gives rise to two parallel lines, it would add noise to the project that is not desirable.

5. Development of the statistical models

In general, I agree with the proposed approach. It is true, of course, that it will need to be tested before we can judge its utility. In the discussion, the point of the need for additional indicators is touched upon again, taking grazing control of phytoplankton as an example. I am not convinced that this would require additional indicators, as it may well be covered by a non-linear eutrophication-chla response (this should be investigated cross-system). In general, I agree that the use of multiple indicators helps understanding the system in an exploratory analysis. However, calculating MAI is a normative activity where the introduction of noise from additional indicators can also result in less certainty on the necessity of reductions, and loss of transparency in the social process of implementation of measures. Those aspects should be weighted appropriately.

6. Development of mechanistic models

I agree with the proposed approach.

The researchers discuss the importance of the one-out-all-out principle in the context of the mechanistic modelling. This is a difficult point. On the one hand, it is imposed as a methodology by the Water Framework Directive. On the other hand, uncertainty and variability in the indicators are inevitable, and this variability will inevitably be exaggerated by the one-out-all-out principle. Weighing of different indicators can give a more robust measure of ecological status, as long as it is very clear that all of the indicators are causally related with the nutrient loading. Including too many indicators, especially indicators with a poor relation to the nutrient loading, will introduce noise and mask the effects of eutrophication (or of sanitation measures) on ecological status. Striking a balance between these different considerations is not easy. I agree, however, that a well-documented and transparent procedure weighing several important indicators, is preferable over the rigorous application of an imposed procedure.

7. Other pressures

I endorse the efforts made to inventory other pressures on the ecosystems, that may add to poor ecological status. I re-emphasize the need to evaluate whether these pressures are additive to eutrophication, or synergistic. I do not think that, in general, these aspects will change the conclusions on MAI dramatically. However, the documentation of single systems may point out some cases where it is needed to account for other pressures on the system. Also, as is pointed out by one of the stakeholders, it would be very unjust to impose severe nutrient limitations to farmers while leaving other stakeholders, that also affect the good ecological status, untouched.

8. Certainties

I agree with the point of view of the researchers. Evaluating the uncertainty of calculations is important, but it is an illusion to obtain results that are 100% certain. The calculations are also too complicated to arrive at a fully formal derivation of uncertainty estimates. However, whatever effort can be done to improve on this estimation will be beneficial during the stage of implementation of the measures.

9. Coordinating with Germany and Sweden

I agree with the proposal

10. Continued focus on measurements

The importance of continued high-quality monitoring cannot be overestimated. It will, eventually, prove to be of utmost importance in the process of implementation of MAI.

In section 3 (Application) the researchers re-order their plans and proposed approaches presented and defended in section 2. Most points of discussion have already been mentioned. Some additional points are the following:

- Change of base period. The researchers' suggestions seems appropriate, but having no complete overview of the data base it is difficult for me to make a judgment. The most important aspect of this choice is the maintenance of consistency across all systems.
- The analysis of seasonal variations in N input is interesting, but potentially also a vast area of investigation. In principle, there are a large number of ways in which N (and/or P) input could be varied over the seasons. As was discussed during the workshop, it may be wise to restrict these calculations to a limited number of scenarios that are feasible from an agronomical point of view: where are the degrees of freedom for the farmers, and how could they be guided in their choice of options? A similar comment applies to N/P combinations, and possibly these too could be combined with the seasonality into the possible reduction scenarios.
- The discussion on the inclusion of climate impacts shows that a full account of possible climate impacts is not possible within reasonable bounds of effort. In such a situation, it may be wiser to restrict the question to a qualitative analysis of the most important aspect: what are the chances that, given climate change, the proposed efforts are too high? In other words, is climate change likely to improve the ecological status or reduce the negative influence of nutrients on the ecological status? I personally see little reason why this would

be the case, but careful analysis of the models in the light of predicted qualitative changes in climate will help to increase certainty of such a qualitative appraisal.
Of course, it is still possible that climate change would work the other way, rendering the proposed efforts insufficient. This, however, can be remediated in the future once the uncertainty of climate effects has decreased. It should therefore not be of immediate concern.
In general, although climate projections are highly relevant to society, I would not advise to place too much emphasis on climate effects in the present project.

Stakeholder comments.

KL-local government Denmark stresses the need to develop models that are as water-body specific as possible and sees a positive evolution here. KL also stresses the need to include both N and P in the considerations and points out the specificity of the shallow parts of the fjords.
The first two points are addressed by the current plans, and I see no discrepancies here. The last point may be important but poses a major problem. If full spatial coverage of all fjords, especially the vast shallow parts of them, are needed in the monitoring, this may require an impossibly large effort. I do not think this is realistic, especially considering that monitoring efforts should be maintained in the long run and only become really valuable after a few decades. In order to better evaluate the importance of this aspect, I recommend to closely study the outputs of the high-resolution mechanistic models for any dominant spatial trends across the shallow parts. Models can also be used to evaluate how presentative the monitoring points are for the entire area.

Bæredygtigt Landbrug mentions several points, that are summarized and commented upon in the following list:

1. *Instead of focusing on single stress factors (such as looking only at nitrate), the interaction between many factors must be considered.* – I feel this is adequately covered by the attention to N/P joint influence, study of seasonal patterns of input, and the separate study on other stressors on the ecosystems
2. *Estimation of good ecological status should be differentiated in a way that includes both the spring and the summer levels of chlorophyll. This links closely to the influence of P and N on eutrophication.* – I think that this aspect is covered in the plans by the researchers, with the attention for P and N and attention for seasonal patterns. I want to stress that understanding the 'memory' of the system for P retention is important in this respect. It is true that past eutrophication may play a role in some systems, as P has been accumulated over the years and may still influence the systems' response. However, this memory is not indefinite. Models can be used to estimate the time constant of this memory mechanism, that may be responsible for decreased year-to-year correlation between nutrient inputs and chlorophyll levels, but should on the longer term still result in significant correlations between nutrient inputs and eutrophication indicators.
3. *The present models do not include, or only insufficiently include, phosphorus.* – see previous points
4. *The issue of oxygen depletion should be studied further before implementing nutrient limits.* – I think this may be solved by considering susceptibility to oxygen depletion as an

essential system characteristic in the typology (and/or in a regression analysis). It forms part of the system specificity of the models.

5. *There is no connection between the Secchi Depth and N.* – In its generality, I think this statement is not true. There is clear evidence of a link between nutrient loading and water transparency at the longer time scale. Whether it is only N or combined N and P playing the key role here is uncertain. Addressing this point is important but is part of the plans
6. *There are coastal waters where even a 100 % reduction will not lead to achievement of the goals.* – I feel this is connected with the previous point, at least with respect to Kd. It was rare for chlorophyll a, but occasionally did occur. This may be due to variability, or also to system specifics (e.g. influence of benthic grazers). In the latter case it should be covered by the present plans.
7. *For chlorophyll there is a common goal for all coastal waters/water bodies (3.6 mg per m^3). The typology should be clarified further, and the chlorophyll objectives should be individually determined. Please refer to study on eelgrass from 2011 as mentioned in the report.* – I do not fully understand this remark. It is not true that the same target value for chlorophyll is used in all systems. The connection with the eelgrass study is not clear.
8. *Other plants than eelgrass (within the same family) should be used.* – This now is part of the plans.

Two additional points are raised. One concerns the possibilities for eutrophication abatement by further purification of wastewaters. I cannot judge on the potential of this point, but recommend it be studied separately, as it will continue to be an important argument in the discussion on implementation of measures in agriculture. In my opinion, it is also important to consider cost-benefit aspects of different measures in different sectors, as well as the problem of distribution of efforts over different sectors in society. This is not part of the current project but does require sufficient attention.

The second additional point concerns the possibilities of eelgrass restoration as a sanitation measure. I want to stress that the success described in the cited American study, cannot be generalized easily. There are many more failed than successful eelgrass restoration projects, and predictive capability on the potential success is limited. However, as already recommended in IE, better understanding of the eelgrass system may help to explore these paths further. This is, however, a research goal at a longer time horizon than can be achieved in the presently planned project.

Danish Agriculture and Food Council has given two responses, one before and one after the workshop. Many of the points raised are similar. I react here to the document filed after the workshop.

This stakeholder discusses indicators and suggests system-specific analyses leading to correlation (or not) of the indicators with nutrient loading. – I agree with the general position that adding more indicators is not always an advantage in order to arrive at reliable reduction scenarios, as it may also introduce noise. I also agree with the suggestion on system-specific analyses but propose to keep them short and manageable. I want to add that analysis of the relation between indicators (especially Kd) and nutrients should not always be attempted at the short (yearly) scale.

This stakeholder states in general that averaging a ‘good’ and a ‘bad’ result does not necessarily lead to a better result than only keeping the ‘good’ estimate. While this is obviously true, provided one has absolute knowledge of the quality of the subresults, it raises the question of how to judge whether a model result is ‘good’ or ‘bad’. I think it is extremely difficult to devise transparent and generally accepted criteria for this, as results may be influenced by random noise, apart from mechanistic relations. A good procedure should not be arbitrary, let alone influenced by the social desirability of the result. It should also be of general value, so that all systems are treated equally. Therefore, I am in favor of the procedures proposed by the researchers, which in my opinion have weighted the different methods and averaging procedures appropriately.

With respect to other stressors, this stakeholder mentions that it is untenable to ask farmers to reduce nutrients severely, while other stressors persist and good ecological status may not be reached because of these other stressors. This is a valid and important point, both from the point of view of effectivity and of social justice. I think the study of other stressors will shed more light on this aspect. In my opinion, it will rarely lead to a reduced need for nutrient restrictions, but it may lead to additional constraints in other societal sectors. Here, as was discussed with respect to wastewater treatment, cost effectiveness of measures to be taken in different parts of society will become an important aspect to study during the implementation process.

Finally, this stakeholder proposes to address the problem of seasonal nutrient inputs based on realistic scenarios. I endorse this proposition.

Concluding remarks

The documents provided by researchers and stakeholders have convinced me that great steps have been made towards convergence on methodology for the next phase of the management plans. I am also pleased to see that several research projects on prerequisites for this modeling, e.g. the typology, have already started or are near completion. This greatly facilitates the model development and will help the process of reaching a well-justified conclusion.

I identified some remaining problems or areas that need further clarification and specification. The workshop with stakeholders has convinced me that while discussion and further refinement of the plans is still needed on these points, a consensus is within reach. I am hopeful that the open process I could experience during the workshop will further contribute to a set of models and conclusions on nutrient loading limits that is of the highest achievable scientific quality.

Appendix: report of the workshop Copenhagen 17/3/19

1. Typology and development of models

The researchers present preliminary results of the research project on typology of the Danish waters. A number of variables have been determined for all water bodies, including geographical position, salinity, mixing characteristics, retention time, temperature, substratum type, depth, freshwater influence. An MDS ordination shows several groups of stations. Some stations are very different from other, while the majority forms a relatively closely related group with some correlated gradients. A cluster analysis on the results has further made the differences between stations explicit. Correlation analysis has shown which physical factors contribute most to the differentiation of the water bodies.

On-going projects aim at the further development of mechanistic and statistical models.

For the statistical models, a Bayesian approach with cross-system effects is followed. The backbone is formed by the 27 stations with long time series. Other stations with more limited data bases will be included as validation. Not all water bodies are sufficiently covered by the data base. For some, a regression-based approach may be used to estimate required efforts. However, the typology will also be needed, as some deviant systems may not be sufficiently covered by this approach, but can be characterized by looking at closely related (following the clustering and typology) systems.

For the mechanistic models, seven new models are under development: two regional sea models, six estuary-specific models, and three local models will be available at the end of this project. The North Sea water bodies will be covered by a specific model, which is needed because the physical drivers are quite different from the Baltic systems (e.g. influence of tide, but also nature of nutrient limitation).

With respect to the estimation of MAI, estimates from the two modelling approaches will be made more independent from each other. However, researchers state that averaging the approaches will be needed at the stage of estimating the target value, as they do not want to present two different estimates of the target values for the same water system.

The discussion focuses on the need to continue using a typology, on the variables used in the typology, on the question whether some systems may show qualitatively different behavior, and on the inclusion of all available knowledge on all of the water bodies in the estimates of MAI.

The use of typology will not be entirely avoidable, but it is stated that the typology to be used in the next phase will be finer and better founded in data than in the previous phase. It is suggested that at the final stage of the procedure, after estimating MAI for each of the water bodies, a water-body specific review of existing knowledge is added to evaluate the robustness of the conclusions. In particular, stakeholders request to keep an open eye for qualitatively different behavior of some of the systems, e.g. because they are more dominated by suspension feeding benthos than others, resulting in a different response of chlorophyll concentrations to nutrient loading.

Some discussion also focuses on the question whether turbidity should be considered as one of the characterizing variables for the typology. This is difficult, however, as turbidity (in particular Kd) is also used as an indicator, and circularity of reasoning may easily arise.

2. Indicators

Researchers state that their aim is to have a suite of indicators to capture the diversity in ecosystem response. They also stress that not all indicators can easily be used, as there are a number of requirements on an indicator before it can be used in the procedure. They make a plea to use operational 'useful' indicators, even if these are not ideally corresponding to all requirements. Most emphasis will be on summer chlorophyll-a, as this is an intercalibrated indicator with good and fast response to nutrient loading. Consideration of spring chlorophyll-a may be needed to better reflect the role of P in the systems. K_d will also be used, not only because light is very relevant in the systems, but also because there is an exceptionally good reference base. A macrophyte index is being developed that includes macro-algae and other angiosperms than eelgrass, so as to represent the entire class of 'macrophytes' better.

Other indicators will be used as 'supporting indicators': additional proxies for eelgrass, spring chlorophyll-a, indicators of hypoxia and anoxia, organic N and P, total N and P and maybe others. The precise relation of these supporting indicators to the main indicators, and their exact position in the calculations of MAI, is not yet entirely clear and is subject of further methodological development.

The discussion focuses on the degree of correlation between the different indicators, and on the need to include many different indicators in the process. Inclusion of more indicators may improve the estimation of MAI if all indicators have clear relationships (at the same time scale) with nutrient loading, but if not may also add noise to the system. Attention is also asked for hysteresis in ecosystem responses, particularly in the case of eelgrass systems. It is asked to further investigate what could be the conditions needed for eelgrass restoration. One suggestion is to use supporting indicators in the analysis of system-specific, qualitatively deviant, responses and in the analysis of the specific vulnerability of some systems. However, this will demand further methodological checking, as it is not clear how to incorporate this into a transparent procedure.

3. Other pressures, roles of N and P, seasonality of nutrient inputs.

Researchers present preliminary results of a research project evaluating the relative importance of P as a limiting nutrient in the different water bodies. Overall, the map shows greater importance of P in the North Sea stations than in the Baltic stations, but the picture is not entirely clear-cut. There is also a clear seasonal effect, with P in general more limiting in spring and N in summer.

A 'climate change effect' project has been started. In particular, it will investigate the effect of changes in rainfall and temperature on the indicators used in this project.

A project has also been started to investigate the importance of other stressors on the coastal ecosystems. Fisheries, sluices, dredging, TBT, Sargassum invasion have been identified as significant pressures, but nutrients are confirmed to play a major role in all systems. There are options to include the additional pressures into the mechanistic models.

The effect of seasonality of nutrient inputs on the indicators will be investigated in a separate project.

The discussion focuses on the how to handle release of phosphorus from the sediments. Some systems continue delivering P from the sediments for a long time after the peak loading. There are some estimates of the residence time of P in the water bodies, but it is likely that this depends on the specific characteristics of the water bodies, as P release is strongly determined by oxygen dynamics, whereas the transport of the released P is subsequently also a function of the flushing and mixing characteristics of the systems.

Some discussion is devoted to the importance that stakeholder attach to the inclusion of P and of seasonality of nutrient inputs into the models for MAI. Stakeholders stress that their aim is to broaden the portfolio of possible measures for nutrient reduction. At the same time, the discussion reveals that the possibilities to change the seasonality of nutrient loading are constrained. As there is a problem of properly orienting the model studies (many theoretical combinations of changed seasonality and relative reduction of P and N are possible), it is suggested that the model explorations could be guided by realistic scenarios for reduction, so as to limit the number of possibilities to be investigated to the most relevant ones.

4. Overall discussion

In the general discussion, most participants expressed satisfaction with the openness of the process and the level of dialogue developed.

Fears are expressed by several participants that there will be no substantial change in the models used. Researchers do not agree with this representation of their current work.

Stakeholders welcome the use of more specific models for the different water bodies. They make a plea for incorporating available knowledge, and supporting indicators, in water-body specific analyses and for the opening up of more possibilities for nutrient reduction than only reduction of the yearly N load.

It is stressed by some that uncertainty will remain, while other voices differ from this point of view and demand that high certainty be reached before any action is taken. Uncertainty will remain an important point in the discussion.

Nature conservation stresses that there is no uncertainty about the fact that many water bodies are not in Good Ecological Status. Therefore 'no-regret' action can already be taken now. Farmer organisations, however, stress that action is already being taken and that much of the low-hanging fruit is already picked. Timing of actions is an important point of discussion.

The need to have transparent procedures that are clear and can easily be explained to farmers, is stressed by stakeholders.

Growth conditions for eelgrass, hysteresis effects and conditions needed for eelgrass restoration are again stressed as important points on the knowledge agenda.