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Prepared for:

RIKZ

Beoordeling Generiek Ecologisch
Model, GEM

Report

January, 2007

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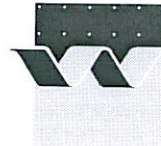
RIKZ

Beoordeling Generiek Ecologisch Model, GEM

K. E. van de Wolfshaar

Report

January, 2007



OPDRACHTGEVER:	RIKZ				
TITEL:	Beoordeling Generiek Ecologisch Model, GEM				
SAMENVATTING:					
<p>The Generic Ecological Model (GEM) has been developed by WL Delft Hydraulics. The biogeochemical model is used to simulate aquatic systems in a dynamic manner. The model has been applied to a variety of environments, both abroad and at home, in freshwater and in saline environments. The model has not yet been audited by an independent body.</p> <p>Rijkswaterstaat is interested in obtaining an independent judgment of the robustness and applicability of the model for their marine studies. To this end, Rijkswaterstaat and WL Delft Hydraulics have asked a panel of internationally renown foreign experts to subject the model to a review. The experts chosen for this assignment were drs Alain Ménesguen and Paul Tett. Both were given documentation and a questionnaire that would help them to review the model and a selection of its applications. Subsequently, the experts were given an opportunity to ask questions and demand clarification where needed from Delft staff (Hans Los, Leo Postma, Johannes Smits) on particular aspects of the model. A third expert, William Silvert, was asked to chair a concluding workshop. The client, Rijkswaterstaat, represented by Hanneke Baretta and Theo Prins also participated in the workshop. On the basis of the workshop and the completed questionnaires the chairperson was asked to prepare a concluding statement. The present minutes of meeting reflect the proceedings of that workshop. The minutes have been prepared by Maarten Kuijper. The final outcome of this audit consists of these minutes, the concluding statement and the completed questionnaires and additional notes from the external reviewers.</p> <p>This final report consist of the concluding statement of William Silvert, the minutes of the workshop and the response to the two questionnaires by Paul Tett and Alain Menesquen.</p>					
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I GEM audit Summary

Workshop on 5 December 2006

Report by William Silvert, Workshop Chairman

AM = Alain Ménesguen, PT = Paul Tett, Reviewers

DH = Delft Hydraulics, HBB = Hanneke Baretta-Bekker

I.1 General Summary

In this part of the report I have tried to integrate the workshop presentations by AM and PT with the material on their questionnaires. Some additional information came out in response to questions raised during the workshop.

The workshop opened with summaries by AM and PT. Both were generally favourable towards the model (PT in particular praised the 'Occam's razor' approach) but raised similar issues about several parts of GEM:

Phytoplankton – The growth rates for algae were seen as about an order of magnitude too low, except for the high value for dinoflagellates. DH explained that this must be a documentation error and that the lack of units on the relevant table was at fault. It turned out that there was an incorrect equation in one of the reports and the issue of algal growth rates was resolved to everyone's satisfaction, except that it still appears that the dinoflagellate growth rate is too high.

PT noted that he felt that DOM and pelagic microheterotrophs should have been included, although they may be considered implicitly represented by the respiration and mortality terms. He also raised some points about the calculation of light limitation.

BLOOM – The reviewers liked the BLOOM approach but raised several questions about components and especially microbial terms (which they felt were implicitly present). PT observed that microbes are important and should be included. The microbial loop part of BLOOM should be made more explicit and expanded. PT also felt that the closure terms were unsatisfactory, and said that natural mortality (such as that due to plankton being transported into areas of higher salinity) should be separated from grazing mortality.

HBB raised several questions about BLOOM, particularly the use of fixed grazing rates. There was a general discussion of the closure of the biological model and the degree to which the treatment of higher trophic levels should be expanded. Both reviewers felt that there should be more inclusion of grazer dynamics, including both suspension feeders and pelagic herbivores. AM also mentioned that invasive species like the Japanese oyster should be taken into account, and he pointed out the special importance of getting grazing right in shallow lagoons, especially ones used for shellfish aquaculture. DH responded that because the time and space scales were different this would be difficult, but it was suggested that

although it might be difficult to include the grazer populations as state variables in the model for this reason, long-term changes could still be modelled in other ways.

HBB pointed out that BLOOM is controversial and many Dutch scientists have expressed dissatisfaction with it. However the reviewers seemed to find it a reasonable approach, although with shortcomings. Much of the concern seems to be that most scientists are used to models based on solving differential equations, and the linear programming optimisation approach of BLOOM is unfamiliar. However the reviewers, especially AM, seem to find this a clever and innovative approach.

Sediment – There were considerable questions raised about the treatment of sedimentation, which seems to be one of the weaker aspects of GEM. AM pointed out that although sedimentation was modelled in detail, the model was not satisfactory for shallow water and gave poor results in the Wadden Sea. PT said that sediment mineralization is a problem, especially in the Wadden Sea; carbon masses are high so rates are critical. He was also concerned about the equation and parameters for light extinction. DH responded that sediment modelling as presented was a stopgap measure, there is a lot of work needed to develop a sediment model. DH added that GEM has been used in tropics with minor modification, although the only detailed application mentioned outside Dutch coastal waters was the Lagoon of Venice.

The use of a stochastic cosine function rather than actual calculation of erosion was the focus for considerable discussion, again leading to the observation by DH that this was a preliminary version and the sediment sub model would require much more work, and the cosine forcing was better than doing nothing.

PT also asked whether flocculation was significant, and DH replied that it was, but it received lower priority in the modelling work.

Several different and apparently incompatible sediment models were presented by DH, all of which seemed to have serious deficiencies – for example, only one included resuspension.

In general the sediment modelling was seen as incomplete and requiring considerably more work, but PT commented that sediment modelling is such a difficult field that the work carried out by DH could be considered satisfactory so far as it went.

Physical-Biological Coupling – During these discussions the feedback of biological components on the physical environment came up repeatedly. One important issue was perturbation of the sediments by burrowing and bottom-feeding organisms (bioturbation and bioirrigation). PT suggested that an abstract concept like a “bioturbation potential” might be useful.

Hydrodynamic Modelling – There was general satisfaction with the use of a curvilinear grid for modelling the Dutch coast, although AM pointed out that it might not be suitable in parts of the French or English coasts where there are fjords and long estuaries. DH responded that it might be feasible but may require too many fine cells to be practical. The transition from 3D to 2D models generated more discussion, and PT in particular was concerned about hydrodynamic modelling, the reduction of 3D models to 2D especially in seasonally stratified waters, locations near fresh water inputs, salinity, and related topics. He

found the scheme inadequate because of nonlinear effects of transient stratification on algal growth as function of light and nutrient in 'regions of freshwater influence'. AM noted problems with the salinity levels calculated near the coast and felt that there were spurious diffusion effects in the model. These were seen as open questions to be answered. The response of DH was that due to a lack of data, these issues are unresolved. AM commented that although the hydrodynamic modelling was not totally satisfactory, it was state-of-the-art and as good as could be expected. The use of Sigma (proportional) coordinates for describing vertical structure caused some concerns, especially in areas where there are strong coastal gradients. HBB raised the question of whether DH could test salinity discrepancies by using a 1D vertical model. DH agreed to consider this.

PT expressed reservations about the boundary conditions for the modelling, and DH concurred and said that the boundary conditions were being revised along the lines that he proposed. Both reviewers objected to the use of constant boundary conditions.

The issue of drying intertidal zones came up, and DH expressed confidence in the work of an academic colleague who did the drying zone modelling. PT raised some concerns about biological issues, especially those associated with the draining of water during ebb tide. DH responded that this was not yet treated, nor was temperature exchange with sediment. HBB asked whether GEM has been used in area with tidal flats and DH answered that this had been done only in Venice lagoon. It was agreed that work needs to be done on modelling the drying process.

In a related discussion, it was asked how the resolution of the biological models compares with the fine resolution of the hydrodynamic grid. DH responded that GEM runs biology on the hydrodynamic grid. There was also discussion of the Domain Decomposition method mentioned briefly in the Maasvlakte report. DH explained that this involves running several models simultaneously and is very tricky.

Parameter Estimation – PT raised the question of where all parameter values come from when not obtained directly from data. Many came from fitting the results and there was a concerned discussion about modelling vs. data fitting for parameter estimation. Data fitting was considered satisfactory for interpolation in cases where future predictions were not very different from what had happened in the past, but when moving into potentially new regimes it was felt that more attention to modelling based on knowledge about the system was needed, i.e., on fundamental principles rather than statistical fits.

Publication – There was repeated reference to the lack of published material and both reviewers emphasized the importance of submitting material to primary journals in order to obtain the critical evaluation of external reviewers and scientific colleagues. It was felt that much of the work was of publishable quality, so it was surprising that so little of it had been published. PT pointed out that without such publication the work will lack scientific credibility.

AM raised a question about the availability of the model for both use and review – whether the code was freely available, whether use of the model required a proprietary interface, etc. (Editor comment: GEM is accessible via the standard Delft-3D WAQ interface. In principle is the code available for research purposes when a licence and a maintenance contract are

agreed upon. The code is already available for Dutch institutions via earlier agreements about GEM.)

Presentation of Results – There was moderate dissatisfaction expressed with the reports, which the reviewers found a bit confusing in places. PT stressed the importance of clarifying the relationship between predicted and observed effects, and specifically called for phase plots showing predicted values vs. observations. There should also be emphasis on calculating confidence limits on predicted values and showing the ranges around simulation results. AM observed that there were lots of graphs in the reports, but a lack of information on which modules were used, and what the formulations were. There were too many detailed figures but not enough synthetic info (e.g., maps). It was not always clear what the model did and which modules, state variables, etc. were included.

User Interface – After some discussion and comments by HBB it was concluded that GEM should best be run by DH staff that prepare the simulations, analyse the results and prepare reports for clients. Although it is in principle possible for clients to run the simulations or analyse the results themselves, the task is highly technical and in most cases impractical.

Climate Change – HBB pointed out that one likely application of the GEM model would be anticipating the effects of climate change, and there was concern about how suitable the model was for that kind of extrapolation. Given that climate change may involve new conditions unlike what has been observed in the past, several observations were made:

Climate change involves extrapolation and it is important to derive parameters from fundamental principles rather than statistical fits.

Changes in the grazer populations must be taken into account.

DH responded that not much has been done so far to anticipate impacts of climate change – sea level rise, storm frequency, temperature shifts. While it would be possible to prepare and run specific scenarios, no special features had been incorporated to facilitate the process.

Additional Issues – There was again reference to problems modelling the Wadden Sea, especially the sediments, and also steep gradients near the coast. AM was very dissatisfied with the nutrient simulations for the Wadden Sea. The salinity levels near the shoreline were seen as incorrect. AM wanted to see data on phytoplankton succession and noted that *Phaeocystis* concentrations were not correctly correlated with nutrient loadings. PT noted problems with the relationship between nitrate and salinity.

PT raised the question of whether GEM could adequately model large perturbations to the system, such as might occur with climate change or major anthropogenic disruption of the system (oil spills, etc.).

1.2 Conclusions

The overall sense of the workshop was that GEM is a useful and valuable tool, but that many parts of it are incomplete and will need considerable further development. The favourable evaluation of GEM seems a bit paradoxical, since there is virtually no part that did not come in for criticism, but the feeling seemed to be that because the modelling challenge is so great, the work that has been done so far is satisfactory and should be accepted. However the conclusion remains that the reviewers felt that GEM was still very

much under development and should be seen as much as a research project as a management tool.

The modelling of the Wadden Sea came in for particular criticism, for reasons that point up the weaknesses of GEM – shallow water, complex sediment interactions, difficult geochemistry.

It is clear that much still needs to be done, both in terms of improvements to the models, expansion of the system being modelled (grazers, boundary conditions, closure in general), and in terms of exposure of the results to the scientific community through publication and better dissemination of the substance of the work behind the reports. Even so, the sense of the audit seemed to be that progress to date is satisfactory and that GEM should continue to be supported and developed.

William Silvert
6 December 2006

2 Minutes of the Workshop

MCM/MSE/Z4267/MK

To : Hanneke Baretta; Theo Prins
From : Maarten Kuijper
Subject : GEM Audit – Minutes of Meeting
Date : 7 December, 2006
Cc : Silvert; Tett; Ménesguen; Los; Smits; Postma; Blauw; van de Wolfshaar; Minns
Action: van de Wolfshaar; sec-MCM

GEM Audit – 5 December 2006

Present: External reviewers: William Silvert (chairperson; WS); Alain Ménesguen (AM); Paul Tett (PT);
WL | Delft Hydraulics: Hans Los (HL); Leo Postma (LP); Johannes Smits (JS); Maarten Kuijper (MK);
RIKZ: Hanneke Baretta-Bekker (HB) and Theo Prins (TP).
Date: 5-12-06

Introduction

The Generic Ecological Model (GEM) has been developed by WL | Delft Hydraulics. The biogeochemical model is used to simulate aquatic systems in a dynamic manner. The model has been applied to a variety of environments, both abroad and at home, in freshwater and in saline environments. The model has not yet been audited by an independent body.

Rijkswaterstaat is interested in obtaining an independent judgment of the robustness and applicability of the model for their marine studies. To this end, Rijkswaterstaat and WL | Delft Hydraulics have asked a panel of internationally renowned foreign experts to subject the model to a review. The experts chosen for this assignment were drs Alain Ménesguen and Paul Tett. Both were given documentation and a questionnaire that would help them to review the model and a selection of its applications. Subsequently, the experts were given an opportunity to ask questions and demand clarification where needed from Delft staff (Hans Los, Leo Postma, Johannes Smits) on particular aspects of the model.

A third expert, William Silvert, was asked to chair a concluding workshop. The client, Rijkswaterstaat, represented by Hanneke Baretta and Theo Prins also participated in the workshop. On the basis of the workshop and the completed questionnaires the chairperson was asked to prepare a concluding statement. The present minutes of meeting reflect the proceedings of that workshop. The minutes have been prepared by Maarten Kuijper. The final outcome of this audit consists of these minutes, the concluding statement and the completed questionnaires and additional notes from the external reviewers.

Minutes:

1. WS opened the meeting at 10:00 hrs and welcomed all present. Some introductory comments were given on the structure of the meeting followed by an explanation by MK on the products to be obtained from the GEM audit.
2. AM presented his overall findings on the GEM model referring to his summary statement at the end of the completed questionnaire. He considered that the GEM model was a comprehensive ecological model of the bottom of the food web. BLOOM constituted a very original treatment of phytoplankton diversity and physiology. In this respect, he stressed the need for a scientific publication that explains the behaviour of the sub model BLOOM in detail. Consequently, the model is little known by other researchers.
3. AM noted an inconsistency in the maximum growth rate. The growth rates reported in one of the documents were an order of magnitude too low and are in contradiction with published literature.
4. As far as the sediment sub model was concerned, AM felt that the documentation provided seemed to contain a description of all the relevant processes, but that there were no results to evaluate its performance. The results that were presented for the shallow Wadden Sea in the Maasvlakte report were disappointing. On the basis of these results one could argue that the model does not work in the Wadden Sea or similar environments. AM further enquired about the working of the erosion-deposition sub model. The stochastic forcing of resuspension by use of a cosine function used in the Maasvlakte study does not reflect current capabilities.
5. In response to the enquiry, HL apologized for the fact that the Maasvlakte report contained unreliable results of the Wadden Sea. The assignment for that particular study was given at a time that Delft Hydraulics had initially hoped to obtain much progress in the development that same year, but this turned out to be over-optimistic. WS concluded that the sediment model should be regarded as a model in progress.
6. AM further elaborated that two aspects of the sediment sub model should be considered, those being a geochemical part and the resuspension / sedimentation part. In this regard, he posed the question why a stochastic cosine function should be used that assumes steady state SPM, if the resuspension / sedimentation processes can be run independently.
7. HL replied that in the recent sand mining study the use of the cosine routine has been replaced by a new dynamical mode. The cosine works when only a representation of the light regime is required. AM still felt that there still could be discrepancies in its use.
8. LP noted that the approach followed seemed applicable to algal modeling. Now, Delft Hydraulics is focusing more on silt modeling. Ecologists and morphologist are working closely together now, and this has led to progress. The use of the cosine function may be obsolete. WS commented that although the sedimentary regime is unlikely to change, nutrient status can change in sand mining studies. LP argued in response that the conservation of mass approach followed by Delft Hydraulics is generally useful in studies that address the impact of large interventions in morphology / hydrodynamics.

9. AM continued with the general comments. He pointed out that the lack of a scientific paper, and apparent lack of parameter values derived from literature affects the acceptance of the GEM approach. He explained that the approach taken to model total chlorophyll and the succession of phytoplankton species and phenotypes is very interesting but that this must be published.
10. Following the general comments by AM, PT gave his general view of GEM. He commented that the Dutch coastal zone being highly complex, the use of – what he referred to as – the Occam’s razor approach adopted by Delft is commendable, that is that the model uses the simplest description available that yields accurate results for a particular problem. He did wonder however whether the model’s approach holds in cases where extrapolation is needed. Can the parameter values chosen still apply if for instance a 50% nutrient reduction scenario is introduced?
11. HL responded that the model’s performance does in fact show satisfactory results in both cases of high and low nutrient concentrations. Moreover, different parameter sets may be used in different environments as has been done in Hong Kong and Venice studies.
12. In response to the comments made by AM on the apparently incorrect growth rates, HL argued that there might be an error in the documents, not in the model. AM responded that even then, the dinoflagellate growth rates appear too high with respect to the growth rates of other groups. HL responded that he will have a look at it. WS stressed for the need of a verification step to see if code errors exist that cause a model to fail. Following actual checking of the incorrectly reported growth rates, HL agreed to amend the equations in the papers.
13. PT continued on his general observations and suggested that Delft should make more effort in presentation of results, in particular by including scatter plots of observed vs modeled. LP argued that there is a risk of phase-errors in this, but according to PT this can be resolved by phase-space plots.
14. With respect to Water Framework Directive requirements, PT comments that there is a definite need for a measure of reliability. If the GEM model is to be used in this context, reliability should be given due attention. In this regard, he questioned (i) whether the hydrodynamic model does adequately simulate the fresh or salt water budget; and (ii) if the hydrodynamic model can correctly simulate the (frequency of) alternation of stratified and mixed regimes.
15. HL replied that the lack of data on vertical profiles renders a validation difficulty. Only in 1989 there were sufficient measurements, but in that year no variation was observed between 1 m above sea bed, mid depth and 1 m below the surface. The smart buoy data time series appears to contain highly scattered data, and do not show the phenomena of alternating states.
16. AM suggested to use remote sensing data for this purpose. HL replied that this does not work in the vertical, although it could work in the horizontal. AM argued that this should be checked.
17. PT, referring to the depth averaged aggregation of 3D to 2D used in the GEM model, wondered whether the 2D parameterization is adequate, and if so, this needs to be documented. He particularly questioned whether the parameterization accounts for the non-linearity known to exist in biological processes, especially, in light-limited growth.

18. PT was intrigued by BLOOM. He argued that the approach adopted in BLOOM, in principle, reduces the level of uncertainty that is common in the building box approach where more species modeled imply less reliable results. He saw a need to publish the approach and validate the parameter values with mesocosm experiments.
19. PT referred to the heterogeneity in zooplankton, in particular with regard to protozoans and the microbial loop. Respiration of these plankters can account for as much as 8-10% of total respiration. BLOOM seems to factor this in an implicit manner. There is a need to document how BLOOM addresses the microbial loop.
20. PT also argued that GEM contains - what he calls - closure terms at each trophic level. Options for replacing closure by models for groups at higher trophic levels (e.g. mesozooplankton, benthic grazers) need further consideration. In this regard, he noted that the present mortality term for phytoplankton is unsatisfactory because it combines salinity effects (e.g. on freshwater algae) with some 'closure' at the microplankton and higher trophic levels.
21. In terms of the sediment sub model, PT questioned whether the physical filtration of water through tidal flats is adequately simulated. He alluded to the fact that most of our understanding of mineralization processes stems from deep sea diagenetic studies of relatively undisturbed sea beds. The phenomena here do not necessarily apply to shallow energetic waters. There is also a need to differentiate between short term and long term. At present, organic carbon can dominate the balance. The question is how to validate this knowing that there are no 100 year records around in energetic water. In this regard, the S1 and SWITCH type of modeling seem more appropriate because they are simpler.
22. LP referred to a comparative study of SWITCH and ERSEM and the sediment flux model of Di Toro.
23. PT noted that the suspended sediment modeling for the purpose of deriving a light climate seems acceptable. He did have a comment however on the intercept value of 0.067 reported in the light extinction equation in the response curves to nutrient reduction report. Normally, one would expect this value to be 0.027 in pure seawater. It would help if Delft could explain how the number was derived, and how it compares to theory and observations. LP responded that the equation was derived from regression.
24. PT also argued that the open boundary at the northern North Sea cannot be a constant value. HL commented that this was also noted in the latest study and that consequently the north boundary has been changed to reflect better the seasonal variation.
25. PT also wondered if riverine inputs were adjusted for estuarine processes. HL replied that the concentrations used were in fact measured at the outlet of the estuary and not upstream.
26. In line with earlier comments made by AM, PT also stressed the need for the papers to be published in refereed scientific journals.
27. On the question of the quality of the reports, HL explained that the way the report is written is not always comprehensive and much depends on the client's expectations and time remaining at the end of a particular study.

28. WS alluded to the situation in the US, where models tend to be used in an adversarial manner with each proponent having its own model. He mentioned that the GEM model seems to be used more in an issue-resolving manner. There is agreement on the use of the model both by opponents as proponents. With this in mind, he would like to know how the GEM model is used in the decision-making process in the Netherlands.
29. HB then gave a client's perspective on the present GEM Audit and the use of GEM. She explained that RIKZ would like to have an independent opinion on GEM. Her request for a GEM audit was partly triggered by three different types of questions, namely: (i) can a GEM calibrated for eutrophic conditions also be applied to oligotrophic conditions? (ii) what is the effect of the more or less fixed grazing rate in changing situations? (iii) how can we test the validity of the model, knowing that opinions in the Netherlands on the BLOOM model are somewhat controversial? In essence, the basic questions to be addressed in the GEM audit are: what is now in the model?; how is it used; and what can be done in the near future?
30. Referring to the earlier discussion on the hydrodynamic modeling, HB wondered whether a 1DV approach for testing salinity vis-à-vis a smart buoy data is not feasible. LP replied that if riverine outflow is modeled with enough detail, one can achieve an adequate simulation of the initial state. HB can be assured that the 1DV approach has been tried.
31. With reference to the question on the treatment of grazers as a driving forcing function AM suggested that an attempt should be made to have grazer intensity change as a function of primary productivity. One could begin with adding suspension feeders as a state variable. HL responded that there are already a number of grazer modules in GEM and that they are all technically working. What is lacking though is that their performance has not been tested yet. In particular, the impact of adding a grazing module on existing processes in GEM (such as phytoplankton mortality) needs to be further assessed. AM pointed out that a recent paper of Riegman in the Wadden Sea may have value to Delft. WS summarizing the discussion, explained that in order for grazing pressure to be realistic, a responsive forcing function needs to be used.
32. WS wondered whether the model was actually fit for a situation where one wants to extrapolate to unknown conditions as is the case with climate change. For instance, climate change is known to have affected the distribution of small zooplankton in the open ocean to the extent that changes in (selective) grazing pressure are influencing phytoplankton production and composition. HL saw no problem with extrapolation in the case of eutrophication as the model performs adequately within a wide range of nutrient concentrations. But he agreed that in the case of climate change, changes in biological community may occur that with our current state of knowledge cannot be predicted beforehand.
33. Following up on the issue of climate change, WS questioned to what extent the model could actually be used in studies aimed at determining appropriate operation of the storm surge barrier under accelerated sea level rise. HL responded that in theory this is not a problem, but that we have yet to deploy the model in such study. The question of climate change is addressed in the future research agenda prepared by Delft. What happens though is that although climate change looms, and everybody is talking about in an abstract manner, there are no immediate calls for modeling studies being done.

34. WS argued that the degree of uncertainty associated with climate change can be turned into an advantage. We can use scenarios such as a 10 cm sea level rise or a certain increase in the intensity of storms. Using such scenarios we can evaluate whether the model is robust. This favors models and parameters that represent our fundamental understanding of the system over phenomenological models.
35. AM asked HB to elaborate about the perceived concerns with BLOOM. HB replied that the philosophy of BLOOM is not always clear. LP added that there is also concern with respect to the linear optimization approach used, even if from a mathematical point of view it can be demonstrated that this concern is not justified.
36. JS explained the rationale behind the development of DELWAQ-G referring to the comparative study carried out between different model concepts. Four models were evaluated for their possible use in GEM. These correspond to the S1 approach that basically tracks the detritus pool; the SWITCH approach that consists of a structured model, with 4 layers and different state variables of which 2 are steady state (N, O₂) and the remainder are fully dynamic; ERSEM that includes dynamic layer thickness on the basis of steady state principles; the sediment flux model of Di Toro that contains a limited number of layers but has more chemistry. None of the models can deal with resuspension. The models were evaluated in a practical manner and a conceptual manner. In the practical manner, the models were incorporated in DELWAQ and a numerical comparison was carried out in different environments (Chesapeake, Lake IJssel and the Wadden Sea). The conceptual evaluation approach concerned a detailed examination of what is in the models and in what respect the models are different. The evaluation demonstrated that each of the models basically performs best in the condition for which they were developed in the first place. This led to the development of DELWAQ-G, which does incorporate resuspension and redox state (metals, sulphates).
37. WS saw the need to adequately simulate resuspension in intertidal environments. JS explained that the timescale used in DELWAQ-G is compatible with tidal action, but since processes are highly complex net settling is assumed. A more dynamical modeling approach is under development.
38. The afternoon session of the workshop took the form of brief responses / comments on a set of questions posed by the chairperson.
- Question 1: What are the underlying assumptions of GEM, and how generally valid are they? In particular, what potentially significant processes have been omitted, and how well justified are these omissions? Among these I would like to see discussion of the relevance of upper trophic levels, microbes and the benthos.
39. PT: With respect to upper trophic levels, there is often a need for models to explain the effect of eutrophication on fish, shellfish and birds. What can the GEM model contribute in this respect?
40. WS: To what extent are biological feedback mechanisms included, e.g. the effect that macrozoobenthos or fish may have on resuspension of sediments or through bioturbation?
41. AM: Should suspension feeders not be included, given their influence, e.g. the Japanese Oyster.
42. PT: Adding more species does not necessarily imply a better ecosystem model.

43. HL: The model focuses at the basis of the foodweb. There is a difference in the time scale at which fish and phytoplankton respond to changes in the environment. To a lesser extent this also applies to spatial scale.
44. WS: There are lots of processes that can be modeled as responsive forcing functions without having to be included in the model.

Question 2: Does the model make adequate allowance for exceptional events, such as storms, which often cause major perturbations in nutrient dynamics and sedimentation in a short time? Are there any questionable steady-state assumptions in the model?

45. PT: with respect to questionable steady states assumption, one should distinguish between numerical and descriptive aspects. If a model is to evaluate the impact of a 1:100 year storm then the model should be able to provide an adequate response to such forcings. A question to be asked in this respect is whether actually data exist to validate such events.
46. HL: Delft has an almost complete time series from 1975. The model can thus be applied realistically for conditions that occurred throughout these 30 years. Moreover, Delft is developing its model to work in near real time model in a similar way as is pursued in weather forecasting.
47. TP: As long as annual time scales are used, there should not be a problem.
48. PT: The 30 years of data available for input determine the spectrum of variability in the output of the model.

Question 3: Does the model adequately account for all anthropogenic impacts, including accidents (oil spills) and aquaculture? What about disturbance of the sediments by shipping, trawling, dredging, etc.? Are these effects adequately extrapolated into the future for predictive purposes?

49. HL: Anthropogenic impacts can in principle be modeled as an external forcing. This is not a problem in GEM. The impact of oil on phytoplankton has not been modeled.
50. PT: Could the use of a tracer (anti-growth agent) be considered to model a negative impact on growth, in the same way that a nutrient is modeled?
51. HB: An evaluation of the impact of a reduction of toxic compounds on biological processes may be added in this manner.

Question 4: How generic is the model? In what kinds of environments can it be applied, and where could it be applied with modifications? Since the client is RIKZ I assume that it is designed for use in Dutch coastal waters, but since WL Delft is an international consultancy it would help to know whether there are plans to use GEM elsewhere in the world, in which case the concept of “generic” becomes much broader. What are the limitations of the model?

52. PT: Refer to the response on question 3D-2D.
53. AM: Problem of the sediment sub model. Without proper validation, the model cannot be applied in very shallow dynamic environment such as the Wadden Sea.
54. PT: The possible use of the model in tropical environments should be evaluated critically. There is a possibility to include temperature-dependency factors. HL: This has been done in the Lake Victoria Study.
55. WS: In summary, the model can be used quite widely, but changes and local tuning may be necessary.

Question 5: Since the model has both physical and biological components, and these are usually modeled at different space and time scales, is the linkage

implemented correctly? Is the curvilinear variable grid model adequate? How well does the Domain Decomposition approach work? Is the choice of when to use 2D and 3D models suitable?

56. WS: There is often a poor connection / coupling between physical and biological components, e.g. in the Severn Estuary, 42 boxes were used to represent physics and 7 for biology. HL: In fact, the same spatial scales were used in the selected modeling case studies. In this regard, he explained the Domain Decomposition concept using parallel computing.

57. There are particular difficulties in modelling transport in waters with steep gradients in the sea-bed. PT: Sigma coordinates (proportional layer depth) does normally work in coastal environments, but it created problems in areas with steep gradients. LP: Sigma is often used in tidal areas whereas lakes are modeled using a Z-coordinates approach (fixed layer depth).

Question 6: Does the physical sub model deal adequately with drying zones and other difficult aspects of estuarine modeling?

58. LP: Provided an explanation on Stelling's concept of drying/wetting modeling in the hydrodynamic model. In this approach, volumes always remain positive. In terms of water quality, this implies that processes are either auto shut-off when water levels become too low or the cell is only allowed to simulate transport processes.

59. AM: It would be interesting to have temperature modeling in dry cells. PT: The approach precludes an adequate description of beach or tidal flat drainage upon emersion and hence of temperature modeling on dry flats. HB: It thus appears that GEM application is not working well yet on tidal flats. HL: In Venice Lagoon, tidal flats have been adequately modeled. HB: There is an interesting paper by Kohlmeier of the University of Oldenburg that may be useful in this regard.

Question 7: With regard to the case studies that have been distributed, are the results all reasonable? Are there any unexpected predictions that might suggest either errors in the model formulation or computational problems?

60. A number of imperfections noted by the external reviewers relate to:

- Poor behavior of sediments in the Wadden Sea;
- Steep gradients near the coast are not always modeled accurately;
- Salinity along the shoreline shows discrepancies;
- There is no observation on phytoplankton composition with which the model results can be compared;
- The model shows an increase in Phaeocystis if N is reduced and P is not. What is the explanation for this?

61. PT: Noordwijk 10 shows a discrepancy in modeled and observed salinity vs nitrate. A simulated peak nitrate in winter corresponds to low salinity. The observations do not show this. A plot of the nitrate-salinity relationship for observations and simulation should be drawn for comparison. HL: Nitrate and salinity are both tracers in winter. There is no explanation for the apparent mismatch between observed and modeled.

Question 8: Is the model relatively easy to run in terms of set-up and data requirements?

62. TP & HB: Normally RIKZ commissions the model work to Delft Hydraulics. The actual modeling is done by Delft. Reasons are that the interface is not particularly user-friendly and the many possibilities in the model imply that the model is not relatively easy to use unless one is an experienced user.

Question 9: Is the output of the model in a form that is reasonably clear and transparent to clients, or does it require a team of specialists to interpret and translate it?

63. AM: Both reports did not provide enough information for a specialist to comprehend the modeling approach adopted. HB: Although RIKZ is well aware of the contents of the report during and the underlying assumptions at the time of study, it would certainly help if more detail is provided in the reports, particularly so if one has to interpret the results of the report one year after. HL: Reports are written with a particular audience in mind. With clients that are less interested, less effort is made to provide a comprehensive scientific description of the model and modeling approach. Moreover, at the end of a project, budget is typically low, additional runs needs to be made and time is short. This affects the quality of the report. PT: You may solve this problem by either paying 120% or you retain 5% to allow a good additional analysis at the end of the project.

64. The workshop closed at 15:30 hours.

3 GEM review; Notes Paul Tett

GEM review - PT, second notes (updated evening, Monday 4 December)

3.1 Model set-up and process formulations; validation in general

I have thoroughly read Blauw & Los ms, which does provide a good introduction to the model formulation (but I see from the GEM documentation that other formulations are available), and also thoroughly read Los et al. ms about validation against southern North Sea data.

Netherlands coastal waters pose a challenge to modelers, both in physics (variability and gradients; optics) and biogeochemistry-ecology (high particle load, benthic-pelagic interactions, salinity effects).

General issues when considering any model:

- equations ('the model') - are they accurate (in the given context)
- implementation (algorithms and 'the code') - do they do what the modelers expect, i.e. do they accurately simulate test cases with known answers?
- validation - having tuned to one case, what is reliability (confidence limits) of simulation in this case, and what is reliability in a new case with and without tuning?
- application - how good is local tuning, availability of boundary data, etc? What are the confidence limits of simulation? Does the task involve 'controls' - e.g. testable simulation of known conditions - plus 'treatments' - small or large variations? (Small variations unlikely to affect pre-existing tuning, but larger variations might if there are real-world interactions not parameterized in the model).

Numerics - in physics, how are strong gradients dealt with?

- in biology - interaction between linear optimization in bloom and ode solvers?

Have read the 'Numerical aspects' document: a variety of numerical schemes are available to deal with transport in 1D, 2D and 3D models, and much thought has been given to their accuracy etc depending on conditions and purpose. As usual, it is the physical transport terms that dominate the solutions, the biological rates being slower, and it looks as if this allows the terms to be solved separately (as in e.g. COHERENS). But I can't find anything about the question of the interaction between the optimization and biological differential equation schemes.

Many processes are simply described in GEM, which is OK in my view (Occam's razor), and contain empirical parameterizations, which need tuning to each new case and may not well allow extrapolation.

Need for test cases and for quantitative validation (Y-Yhat plots) which includes estimates of uncertainty in simulation

-- for processes

-- for GEM as a whole, or major components (I appreciate that GEM is a system)

Although maps and time-series in Los et al. ms (good though as yet incomplete paper) demonstrate qualitative agreement between simulations and observations, there should be quantitative assessment of fit and uncertainty.

Physical models - *have read little about this so far* - is adjustment of K_h (mentioned by Los et al.) enough to fit the physical model? All my experience is that the hydrodynamics (and their modeling) are the most important determinant of the distribution of phytoplankton and nutrients in coastal waters of complex physical state. (And second are the boundary conditions.).

Following discussions with Rob Uittenbogaard about hydrodynamical models: given sufficiently fine grid and good boundary data, the 3D hydrodynamical can adequately simulate the main physical processes including salinity distributions and temporary stratification (thought to be the result of internal processes and response to wind rather than to spring-neap cycles) although there remain some fundamental issues about differences in the transfer of momentum, heat and salt (and Rob wants to validate simulations of coriolis effect on stratified flows using large rotating tank); the sub-grid-scale processes are parameterized by K_h , which need different values for tracers (10x) and momentum. I would still like to know if (a) the model correctly simulates total salt (or FW) in Dutch coastal waters, compared with observations (which are good) - this for nutrient budgeting; and (b) the frequency statistics for ROFI stratification correspond to those observed.

Optical model - EXTINCT_VL : account taken of all optically active components, but is the extinction model adequate (need to account separately for absorption and scattering - *consider further*); CALCRAD - Beer-Lambert decay might not be adequate (hyper exponential decay near surface; likely to be much scattering); need to calculate layer mean irradiance for thick layers/mixed water columns?

For computational efficiency, GEM is run in 2D; a vertical profile of irradiance is calculated assuming Beer-Lambert decay and this is multiplied by a p-I curve to get an average photosynthetic response (which I think is then used by way of a look-up table because the linear optimization routines of BLOOM can't deal with a curve). The extinction model (with $YS(f(sal))$, POC, and 2 iSPM components) was calibrated from K_d estimated from profiles of down welling PAR - hence might be based on total extinctions that were too large. Would any of this have an effect on predictions of light versus nutrient limitation? Should be investigated?

BLOOM - interesting! a creative solution to the problem of parameterizing the behavior of multi-species phytoplankton with several potentially limiting factors; includes some aspects of the microbial loop (and hence what I parameterize as 'microplankton', so respiration will include some microheterotroph contributions); I guess its biomass/productivity simulations might be more accurate than its predictions of species (=life form) composition; has it been tested against mesocosm data?

I gather that BLOOM has been fitted to Riegman mesocosm data - this should be written up. Also, predictions for Phaeocystis biomass have been compared with Ph abundance - this comparison should be made as a $Y=\hat{Y}$ scatter plot etc. Typical values of the respiration coefficient were taken from L&DBO2 studies, and are of order 10% of biomass/day, hence do include microheterotrophic respiration.

Pelagic, salinity-dependent, mortality term - originally to deal with mixtures of freshwater and marine phytoplankters, but can also provides an empirical parameterization of zooplankton or benthic grazing - how adjusted when grazing modules added? In absence of these, must be fitted to data for particular case to get accurate estimates of actual grazing loss - and what if grazing changes seasonally, or over years as a result of changing conditions?

Other processes - in general (*and taking into account some reading of the 'GEM documentation'*) the parameterization, often providing simple and progressively more complex options, seems OK; however, it is desirable for parameterizations to be derived from theory wherever possible, even if some parameter values estimated empirically (Stigebrandt approach), and for an analysis of terms to show that the simplest parameterizations capture the majority of the rate-determining processes on the space and time scales under consideration - i.e., adopt, more thoroughly, the Di Toro sediment modeling approach - or, pragmatically, show that the simple formulations are accurate when applied to process measurements under controlled (lab. etc) conditions - *I need to consider this further for some example processes! And to read the delwaq-G document.*

The Delwaq-G document describes more elaborate sediment mineralization, microphytobenthos, and grazer, sub models. Taking the sediment model, it should be able to provide more accurate simulation of sediment-water nutrient fluxes, including the effects of denitrification, sediment-P binding, and opal dissolution, based on papers in the scientific literature and the Di Toro (2001) book. However, I know sediment flux modeling to be a difficult problem in coastal waters, especially when multiyear as well as seasonal time scales need simulation; much current modeling is still at the research stage; and so there is a need to show validation of the fluxes simulated by this model in the circumstances in which it will be used.

3.2 Case studies

Maasvlakte 2 effects on the Wadden Sea and the North Sea coastal zone: *not easy to follow this report*, but it seems that the main modeling problem was to fit the observed distributions; the simulated changes resulting from the proposed works were (on this large spatial scale) small, and so this is the 'control+small perturbation' method and thus the modeling seems fit for purpose.

Nutrient reduction scenario report - a complex model is necessary only when there are substantial nonlinearities in the ecosystems potential response to nutrient loading or unloading - otherwise, the prediction of impact change could be made from observed distributions of salinity, given the Netherlands' good coastal monitoring program. If a model is to be used, the first requirement is that it correctly simulates salinity distributions (there are some reservations in the report about this, on the small scale) and that it has good boundary conditions for nutrients (the use of constant nutrient concentrations at the northern boundary should be tested). After this, the main question is, does GEM include the relevant

non-linearities (including internal feedback loops that might damp or magnify changes, either dynamically or in steady-state)? *Aspects to be further considered include:*

- 2D simplification of 3D physics - this has implications for light-nutrient limitation and ought to be examined, taking into account the short-term (tidal or spring-neap tidal etc) stratification that occurs in parts of ROFIs (see Wild et al, 2002)

- light-nutrient interactions within the BLOOM routines and the accurate calculation of mean PAR

- effect of changes in phytoplankton biomass on the grazing component of mortality

- BLOOM simulations of *Phaeocystis* abundance - how reliable is this (in contrast to simulation of total chlorophyll)?

- nutrient dynamics in the sediment: I agree that most mineralization likely occurs in the water column, but the longer-term response of the ecosystem may be most sensitive to sediment diagenetic processes - P-storage and release; denitrification, N-storage and release? *when I played with this in the 0.5 D model L3VMP (model, but not the multiyear simulations described in Tett & Walne, 1995), I found that spin-up periods of 40 years were needed to produce realistic sediment-water fluxes if the sediment was initialized with realistic C and N content. A validated delwaq_Q (improved mineralization) module could be added, with further thought given to long-term processes and the sediment pool of refractory nutrient.*

Delwaq-Q still under development and results not yet reliable. Wadden Sea difficult to model (because of tidal trying esp.) and its not clear if any available model will work for this case. For offshore, maybe a simple, bucket, approach will work best, provided it accurately parameterizes long-term processes.

A Gem review questionnaire Paul Tett

Paul Tett preliminary answers (Sunday, 3 December 2006)

2 Questions

A: Model set-up and process formulations

A.1: Are the processes that are included in GEM sufficient or are there processes missing or redundant?

I guess this is a question about the maximum set and the most complex process descriptions. I miss DOM and pelagic microheterotrophs (although these could be said to be parameterized within respiration and mortality), the explicit idea of ecosystem 'closure' at the highest trophic level represented (although this could be considered as parameterized within mortality), and, in the seabed, fluff-layer processes (might be considered to be within the SWITCH module) and physical irrigation of sediments

A.2: Are the process formulations, included in GEM, leaving aside individual process parameter values, adequate to draw conclusions on the effect of pressure variables on impact variables, as listed in Figure 1? For each of the processes present in the table presented on the next pages, please fill out the corresponding section under the column headings 'adequate?' and 'comments'. Under the column heading 'adequate?' a simple 'yes' or 'no' will do, but under 'comments' we would like you to substantiate your statement with arguments, particularly so, if you consider the process formulation inadequate.

A.3: Do you think the model-set-up of GEM is sufficiently generic, so it is suitable to construct other models with the same source code? Please elaborate?

I don't understand the question - does it refer to the ability to select different sets of subroutines, or to the potential to program different models, such as ERSEM or PROWQM?

A.4: Do you consider the level of detail of different processes in the GEM model sufficiently balanced? Please elaborate?

This depends on the purpose. I agree with using the simplest accurate parameterization of any process ('Occam's razor') - but where possible the relationship between empirical parameterization and underlying theory, and the estimation of parameter values and their confidence limits, should be documented. (This is different from tuning a model to fit a particular data set.) -- this also answers QA.5

A.5 Do you have any other comments on the model set-up and process formulations in GEM? Please elaborate?

Process	Approach in GEM	Reference to documentation	Included in attached model case studies?	Adequate?	Comments
Spatial schematization (general)	Can be equal to hydrodynamic schematization or coarser.	manual p. 2-1	schematization equal to hydrodynamic grid	Yes	for the scale of the processes of interest
Vertical schematization (water column)	Can be equal to hydrodynamic schematization or coarser.		The results of the 3D hydrodynamic model are aggregated to 2D	No	because of nonlinear effects of transient stratification on algae growth as function of light and nutrient in 'regions of freshwater influence'
Vertical schematization (sediment)	Several approaches:		yes		Answer depends on purpose; for some, S-1 is adequate; for others greater discretization and process description necessary, but SWITCH is FW-derived and Delwaq-G seems to me to still be a research tool
	<ul style="list-style-type: none"> no explicit sediment cells (S1) 	manual p. 4-1			
	<ul style="list-style-type: none"> steady-state (SWITCH) 	manual p. 4-63	no		
	<ul style="list-style-type: none"> explicit sediment cells (DELWAQ-G) 	report *	no		
Phytoplankton and primary production	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Yes	the optimization solution is interesting and creative
growth	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Yes	

respiration	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	No	needs to take account of pelagic microheterotoph processes
mortality	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	No	needs to distinguish salinity-mortality from 'closure'
nutrient limitation	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Yes	
light limitation	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Don't know	my experience is that parameterization of light-control of growth needs to reflect the nature of the physical-optical model
species composition	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Don't know	needs (demonstrable) validation against mesocosm experiments

	Several approaches: <ul style="list-style-type: none"> forcing function (CONSBL) (a dynamic version is in development) 	manual p. 6-8	no		Largely outside my expertise; I suspect there is a good empirical compromise to be had between the closure term and the level of complexity in models such as ERSEM mesozooplankton (and even more elaborate, age- or size structured models)
	<ul style="list-style-type: none"> zooplankton module from ERSEM 	Broekhuizen et al., 1995 ¹	no		
Sedimentation	included	manual p. 4-84, 5-86	yes		<i>need to consider further</i>
Resuspension	included	manual p. 8-4	no		
Burial	included	manual p. 4-88, 4-89, 8-6	yes		<i>need to consider further</i>
Under water light climate	according to Lambert-Beer formulation	manual p. 5-75	yes	No	Lambert-Beer law applies only to monochromatic light of uniform flux distribution, and it is not clear how mean light is calculated for a thick mixed layer: suggest using an approximation that links to light-growth routine

¹ N. Broekhuizen, M.R. Heath, S.J. Hay, W.S.C. Gurney, 1995. Modelling the dynamics of the North Sea's mesozooplankton. *Netherlands Journal of Sea Research* 33 (3/4), Special issue on ERSEM.

pH	not included		no		
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Nutrients	NO ₃ , NH ₄ , PO ₄ , Si	manual p.4-2	yes	Y ?	I guess this to be a question about the nutrient species. It is possible than PON should be included in long-term studies of de-eutrophication. Micronutrients - esp. Fe - currently fashionably interesting subjects of study, but unlikely to limit in near coastal temperate waters
Nitrification and denitrification, sulphide methane oxidation	processes NITRIF, DENITSED, DENITWAT. sulphide Methane oxidation	manual p.4-16, manual p.4-23 report*, § 1.2.5 report*, § 1.2.8	yes	?	are these included in the North Sea nutrient reduction study? I got the impression that only water column denitrification was there. Certainly, any long term study of nutrient reduction needs to include sediment denitrification

Adsorption	included	report*: §1.2.5	no		
decay of organic matter	Two approaches: <ul style="list-style-type: none"> • one fraction • 4 fractions 	manual p. 4-79 manual p. 4-51	one fraction in both water and sediment	Y/N	my experience is that (at least) a fast-decay and a slow-decay fraction need to be modeled in the case of long-term studies
oxygen	oxygen consumption during decay of organic matter, reaeration and production by phytoplankton.	manual p. 7-8	yes	Y	with the exception of reaeration (for which there compound relationships with wind speed have been published), the rest is no (and need not be) more than stoichiometry
Solid reactions (dissolution and precipitation reactions)	dissolution of opal silicate (Dissi)	report*, § 1.2.6	no		
Sediment-water exchange	<ul style="list-style-type: none"> • instant in S1 approach without explicit sediment cells • explicit in DELWAQ-G 	manual p. 4-59 report * § 1.2.9	no	?	water exchange is difficult to model because it may include bio-irrigation and physical irrigation - even the more elaborate Delwaq-G does not capture all processes, and simpler parameterizations may be better

Bioturbation	included in DELWAQ-G	report * § 1.2.9	no		
Macrophytes	in development		no		
Microphytobenthos	process: MICROPHYT	manual p. 5-58	no		
Birds, Fishes	not included		no		I have not seen any model that couples higher trophic levels well to spatially-resolved physical-biological models for lower trophic levels
Habitat suitability	not included		no		what does this mean?
Atmospheric input	user can specify additional nutrient input per cell as g/m ² /d		yes (except for 'Maasvlakte' study)	Y	increasingly important to take this into account
Integration scheme	20 options available	report **		Y	many options available - there is no perfect solution in 3D models

manual: GEM documentation and user manual
 report *: delwaq-G.pdf
 report **: numerical aspects.pdf

B: Model application for the Southern North Sea (please answer for both case studies)

B.1 Is the GEM application for the Southern North Sea adequate with respect to: model schematization in space and time.

YES

meteorological forcing

YES

suspended matter forcing

I think so - hard to see how it can be made more accurate without a complex 3D sediment dynamics model combined with remote sensing.

nutrient inputs

this is the critical bit. How do we know that the nutrient data that are used are accurate? They based on river loads (flows x concentrations). Does the GEM implementation take adequate account of estuarine processes such as denitrification or P de-adsorption?

boundary conditions

I think that the northern boundary conditions ought to be time-varying.

B.2 Has the model been sufficiently validated to judge its applicability to the subjects listed in Figure 1? If not, what type of validation exercise is missing?

I see a need for (a) process validation, insofar as this is possible, and (b) model validation, both by the use of Y-Yhat scatter plots and the calculation of r^2

B.4 Do you consider that the conclusions that are drawn based on model results in the example studies of application are justified by the model validity / performance? Please elaborate?

As I understand, in applications, GEM is fitted to an existing data set (drawn, in the examples provided, from the extensive Dutch coastal monitoring program) and then conditions varied for the test case. For the Maasvlakte 2 effects on the Wadden Sea and the North Sea coastal zone, the simulated changes resulting from the proposed works were (on this large spatial scale) small. Such small perturbations should not disturb the parameter estimations, and thus the modeling seems fit for purpose in this case. In the case of the Nutrient reduction scenario report, the simulation, set up to describe present conditions in the southern North Sea, must extrapolate the impact of substantial reductions in nutrients. In this case, the demands are more severe - does the model include accurately all important non-linear processes, especially those important for long-time (multi-year) scales, and can fitted parameter values be used reliably under changed conditions?

B.5 Do you have any other comments on the application of GEM for the two specific case studies dealing with the southern North Sea? Please elaborate?

C: Summary

Could you please give a summary of your comments on GEM below?

1. Netherlands coastal waters pose a challenge to modelers, both in physics (variability and gradients; optics) and biogeochemistry-ecology (high particle load, benthic-pelagic interactions, salinity effects, tidal drying areas). My comments about model adequacy relate in particular to the nutrient-reduction case study.

2. GEM seems to be a well-engineered model, allowing sets of process-descriptions to be linked together, forced with hydrodynamics and solved with numerics, appropriate to a given problem. I like the strategy of using the simplest description that gives accurate results for the problem, and the empirical approach, adjusting some parameters by fitting simulations to an observed data set, is good when only small perturbations are to be investigated. However, I have some doubts about the use this method to investigate large perturbations; in these cases it is important for the parameterizations (equations and values) to be derivable from good theory and to deal with all important processes on the relevant time-scales. We were told that the main parameter set is stable, but it may have 'adapted' to existing conditions in Netherlands coastal waters.
3. In general there is a need to present simulation-observation comparisons for each state variable as a scatter plot with an estimate of the % of explained variance (which will allow estimates of confidence limits of simulations)
4. The 3D hydrodynamic model seems good, especially at high resolution, but there are some questions about whether it can accurately simulate salinity distributions along sections at right-angles to the coast (maybe requires some large-tank testing, or maybe due to poorly-known input conditions, which could be improved by very high resolution simulation of the freshwater discharges). I would like to know if the model correctly simulates the freshwater (or salt) budget of each WFD water body - this is an important precondition for modeling nutrient reduction scenarios - and if it simulates the correct frequency of stratification/mixing alternation in the 'Regions of Freshwater Influence'.
5. GEM reduces the 3D physics to 2D processes and transports, and it needs to be demonstrated that the 2D parameterizations are adequate (both in theory and by observation-simulation comparisons). This is especially the case in relation to water-column optical conditions, mean light, and light-limited phytoplankton growth.
6. BLOOM is a creative solution to the problem of parameterizing the behavior of multi-species phytoplankton with several potentially limiting factors; there is a need to relate the fitted parameter values to theoretical values for each group and to assess the confidence level of predictions of biomass and 'species-group' composition.
7. BLOOM seems to include some aspects of the microbial loop (and hence what I parameterize as 'microplankton', so respiration will include some microheterotroph contributions) - this needs to be considered further in relation to parameter values including those for respiration.
8. The philosophy of GEM suggests that 'closure' terms should be as simple as possible; options for replacing closure by models for groups at higher trophic levels (e.g. mesozooplankton, benthic grazers) need further consideration. The present phytoplankton 'mortality' term is unsatisfactory because it combines salinity effects (e.g. on freshwater algae) with some 'closure' at the microplankton and higher trophic levels.
9. The accurate emulation of sediment mineralization on short, seasonal and multi-year timescales, in intertidal and shallow sub tidal waters, is difficult but of major importance in relation to nutrient reduction scenarios - no existing subroutines seem to describe this accurately for the Wadden Sea. Three options are available (S1, SWITCH (after Di Toro?) and Delwaq-Q) - it will be desirable to see comparisons of simulated and observed sediment-water flux for each. For the sub tidal, developing a simple model might have the greatest promise. I have found it necessary to deal accurately with 'fluff' to get short term mineralization correct - long term simulation (and the proper parameterization of the decay of refractory organics) is reliably modeled for deep-sea diagenetic models, but it is much harder in shallow, energetic waters, especially where there is physical as well as biological irrigation of the sediment.
10. Suspended sediment modeling is also difficult; I think that (so far as the light climate is concerned) the solution and extinction model is broadly adequate, but the estimated

coefficients of the K_d multiple regression equation should be examined in relationship to theory. Is there a biological-physical interaction process (particle aggregation?) to be included in the model?

11. Are nutrient boundary data adequate? - at the northern boundary, should there be a seasonal cycle, should the river discharges be corrected for estuarine processes (or does GEM do this)?

12. In general, if GEM is to have scientific credibility, each part of the model - its theory and validation - should be described in papers published in refereed scientific journals.

B GEM review questionnaire Alain Menesquen

1 Introduction

The objective of the review is to assess if the Generic Ecological Model (GEM) is fit for the purposes that it is currently used for. Figure 1 gives an overview of the kind of management issues that were examined by the use of GEM in consultancy projects.

The Generic Ecological Model contains a number of state variables. Variables can be divided into input variables and output variables. This is exemplified in the following figure. The figure also shows what variables lay outside the scope of current GEM applications.

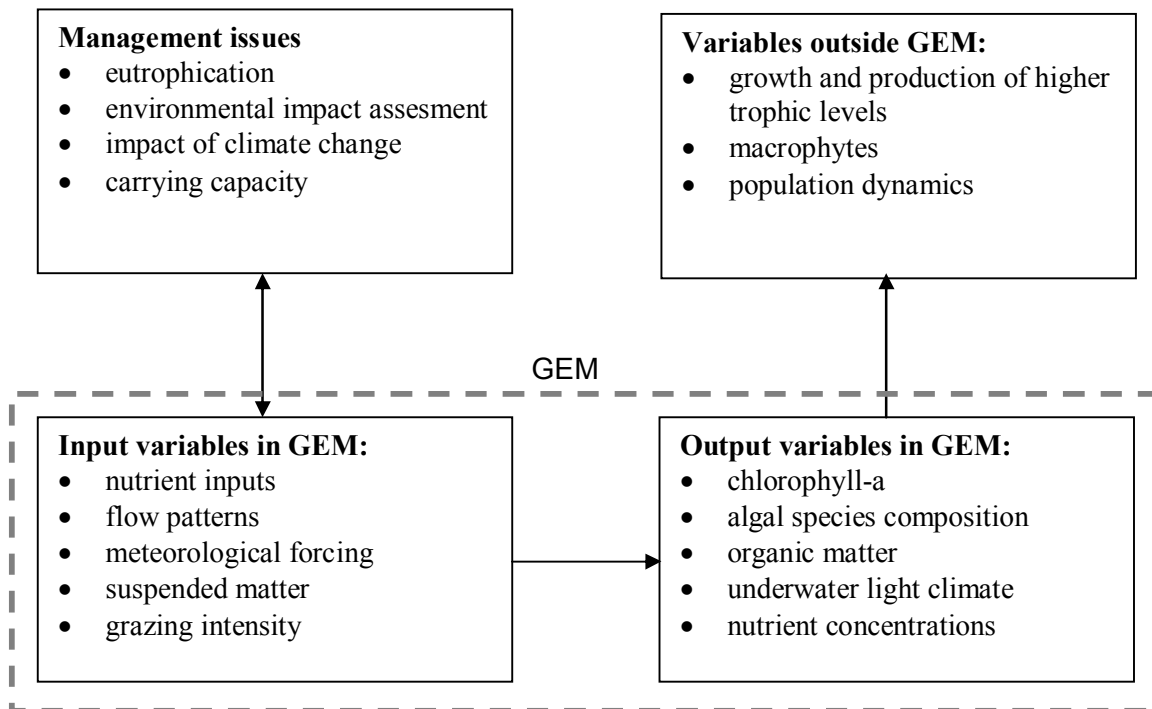


Figure 1: Overview of subjects for which the Generic Ecological Model is applied.

Figure 2 gives an overview of the basic processes in GEM. For some processes several options are available: from simple and straightforward equations to more detailed approaches. For consultancy studies often the relatively simple approach is taken.

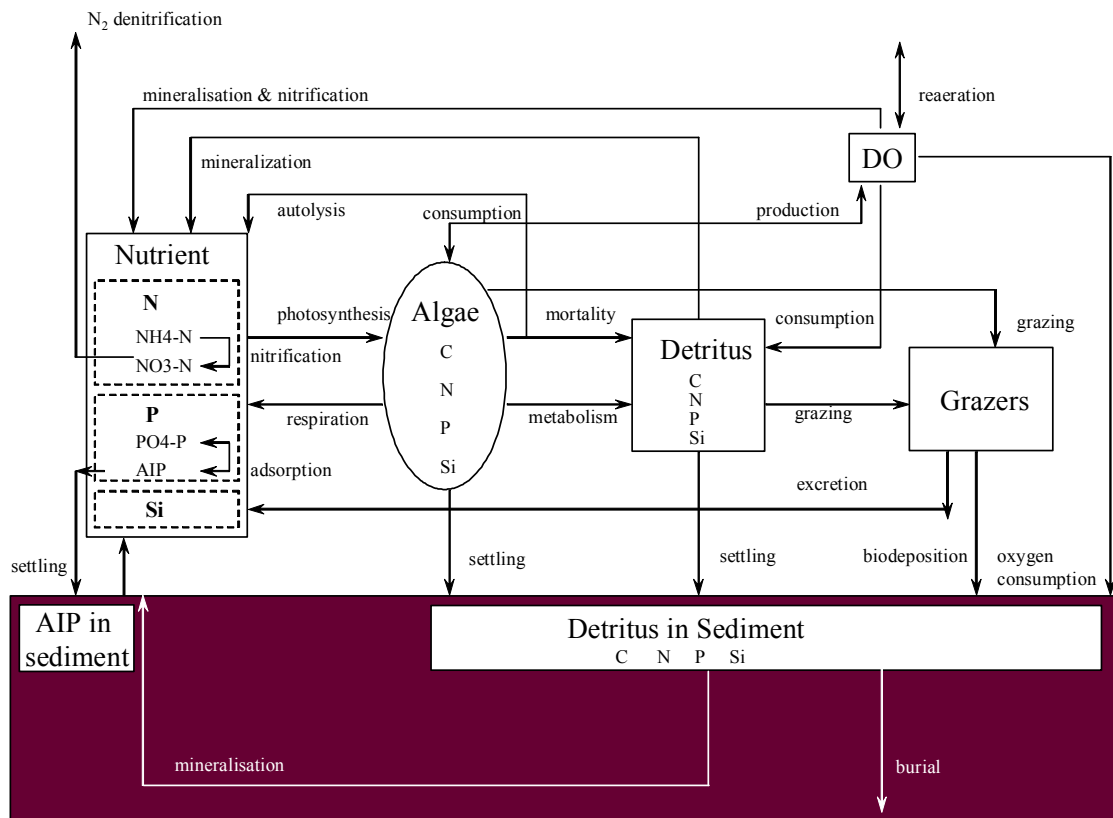


Figure 2: Schematic overview of the model set-up of GEM

The review basically concentrates on two aspects, those being (i) model set-up and (ii) model performance of the application of GEM within two specific modeling studies dealing with the Southern North Sea. The pertinent questions can thus be divided into two parts:

- A. Is the model set-up, in particular in regard to the selection of processes, concepts and applied process formulations, adequate to draw conclusions on the effect of input variables on output variables as contained in GEM for the type of management issues addressed (see Figure 1)? In other words, can you agree with the selection of processes, concepts and applied process formulations contained in the model vis-à-vis the use of the model in support of such consultancy studies?
- B. Is the model performance in two specific modeling studies adequate for drawing conclusions on the effect of input variables on output variables, as listed in Figure 1, in the way that has been done in the selected model studies? In other words: in the selected model applications, does the level of model performance achieved, permit the drawing of conclusions as has been done in those studies?

With respect to model performance we have selected two modeling studies dealing with the Southern North Sea that we would like you to comment on. These are:

- Maasvlakte: This concerns an environmental impact study of the impact of a large reclamation project in coastal waters outside the Port of Rotterdam. WL | Delft Hydraulics examined the impact on nutrient and silt dynamics.
- Application of the Generic Ecological Model for analysis of the response of phytoplankton indicators to nutrient reduction scenarios.

Along with this questionnaire we send you several documents:

Documentation on the model set-up and process formulations in GEM (as background information for part A of the questionnaire):

- a) A selection of text from a draft paper¹ on the model description of GEM. The model set-up described in this paper concerns the relatively simple model set-up as it is used in most consultancy studies. We advise you to read this first to get an overview of the structure of GEM. (paper_model_setup.pdf)
 - b) GEM documentation and user manual: This is the official documentation of GEM, but some of the more recently developed processes are not included yet. You do not need to read the whole manual. We just send this as reference material and we refer to it in some places of the questionnaire. (GEM documentation.pdf)
 - c) GEM application with explicit sediment cells (delwaq-G): This is a chapter from a report (in preparation) on a research application of GEM focusing on redox processes in the sediment of the Wadden Sea. This covers the description of the more recently developed processes. We refer to this document at some places in the questionnaire. (delwaq-G.pdf)
 - d) A chapter from a Delft3D-WAQ manual on numerical aspects of Delft3D water quality modeling, which applies to GEM as well. This document is referred to in the questionnaire. (numerical_aspects.pdf)
2. Documentation on the GEM application for the selected model case studies for the Southern North Sea (as background information for part B of the questionnaire):
- a) a draft paper¹ on the validation of the GEM application for the southern North Sea. This paper is under revision. Figures 17 and 18 and table 4 and 5 are new and are not referred to in the text yet. The cost functions are not described in the text yet either (validation_paper.pdf).
 - b) a report of a consultancy study on the effect of land reclamation near the Port of Rotterdam on the ecology of Dutch coastal waters and the Wadden Sea (Maasvlakte.pdf).
 - c) a report of a consultancy study on the effect of nutrient reduction scenarios on phytoplankton in Dutch coastal waters: "Analysis of the response of phytoplankton indicators in Dutch coastal waters to nutrient reduction scenarios". (response_curves_WFD.pdf)

¹ The GEM model is subject of two recently submitted papers that are still under revision. Although not yet published, we have deliberately chosen to include these draft papers in the set of background information because they contain, in the most concise form possible, relevant information for the present review process. Please note that we do not ask you to formally review these papers. They are included here as a mere reference.

2 Questions (Answers by A. Ménesguen)

A: Model set-up and process formulations

A.1: Are the processes that are included in GEM sufficient or are there processes missing or redundant?

GEM contains a lot of processes, but introducing also grazers (zooplankton as well as benthos) as state variables should clarify the food chain closure.

A.2: Are the process formulations, included in GEM, leaving aside individual process parameter values, adequate to draw conclusions on the effect of pressure variables on impact variables, as listed in Figure 1? For each of the processes present in the table presented on the next pages, please fill out the corresponding section under the column headings ‘adequate?’ and ‘comments’. Under the column heading ‘adequate?’ a simple ‘yes’ or ‘no’ will do, but under ‘comments’ we would like you to substantiate your statement with arguments, particularly so, if you consider the process formulation inadequate.

A.3: Do you think the model-set-up of GEM is sufficiently generic, so it is suitable to construct other models with the same source code? Please elaborate?

GEM has without any doubt a built-in generality. Is the code freely available, and are the entries and outputs fully described to facilitate coupling with every hydro dynamical code? Is it used actually in Delft Hydraulics through a dedicated interface software?

A.4: Do you consider the level of detail of different processes in the GEM model sufficiently balanced? Please elaborate?

Apart from the formulation of grazers, which are very important in shallow lagoons exploited by shellfish aquaculture, I think that GEM is already detailed enough, perhaps too much for simple ecosystems!

A.5 Do you have any other comments on the model set-up and process formulations in GEM? Please elaborate?

See the following table.

Process	Approach in GEM	Reference to documentation	Included in attached model case studies?	Adequate?	Comments by A.Ménesguen
Spatial schematization (general)	Can be equal to hydrodynamic schematization or coarser.	manual p. 2-1	schematization equal to hydrodynamic grid	YES	
Vertical schematization (water column)	Can be equal to hydrodynamic schematization or coarser.		The results of the 3D hydrodynamic model are aggregated to 2D	YES	
Vertical schematization (sediment)	Several approaches: <ul style="list-style-type: none"> no explicit sediment cells (S1) 	manual p. 4-1	yes	Yes for some problems	
	<ul style="list-style-type: none"> steady-state (SWITCH) 	manual p. 4-63	no	YES	
	<ul style="list-style-type: none"> explicit sediment cells (DELWAQ-G) 	report *	no	YES	
Phytoplankton and primary production	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Yes, but complex	Why the external inflows of nutrients are not taken into account in the constraints set?
growth	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes		Why do the dinoflagellates have the highest growth rates? See Table 3 in Blauw and Los (I suppose that the unit is h^{-1}, and not d^{-1})
respiration	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes		
mortality	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes		
nutrient limitation	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	Yes, but without quota	Is Ulva included?
light limitation	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	YES	

species composition	BLOOM phytoplankton model	manual p. 5-21 to 5-44	yes	<i>Interesting, but simulated composition never presented</i>	<i>Give more details on parameter determinations</i>
Secondary production (e.g. macro benthos)	Several approaches: • forcing function (CONSBL) (a dynamic version is in development)	manual p. 6-8	no	<i>NO</i>	<i>Compatibility between forced values and simulated potential grazer evolution leads to complicated modifications of the driving variable, which would become unnecessary if grazers would be treated as a simulated state variable. Problem of discontinuity between two formulae (Eq.31 &32 in Blauw and Los) Excretion using the same grz formula than ingestion??</i>
	• zooplankton module from ERSEM	Broekhuizen et al., 1995 ²	no	<i>??</i>	
Sedimentation	included	manual p. 4-84, 5-86	yes	<i>YES</i>	
Resuspension	included	manual p. 8-4	no	<i>YES</i>	<i>Is the sand advected as the silt?</i>
Burial	included	manual p. 4-88, 4-89, 8-6	yes	<i>YES</i>	
Under water light climate	according to Lambert-Beer formulation	manual p. 5-75	yes	<i>YES</i>	
pH	not included		no		
Nutrients	NO ₃ , NH ₄ , PO ₄ , Si	manual p.4-2	yes	<i>YES</i>	
Nitrification and	processes NITRIF,	manual p.4-16,	yes	<i>?</i>	

² N. Broekhuizen, M.R. Heath, S.J. Hay, W.S.C. Gurney, 1995. Modelling the dynamics of the North Sea's mesozooplankton. Netherlands Journal of Sea Research 33 (3/4), Special issue on ERSEM.

denitrification, sulphide methane oxidation	DENITSED, DENITWAT. sulphide Methane oxidation	manual p.4-23 report*, § 1.2.5 report*, § 1.2.8			
Adsorption	included	report*: §1.2.5	no	YES	
decay of organic matter	Two approaches: <ul style="list-style-type: none"> • one fraction • 4 fractions 	manual p. 4-79 manual p. 4-51	one fraction in both water and sediment	Yes, but not for detrital Silicon	<i>Frustule's dissolution is a physico-chemical process, which is not mediated by bacteria. Remove carbon stoichiometry in detrital Si equation.</i>
oxygen	oxygen consumption during decay of organic matter, reaeration and production by phytoplankton.	manual p. 7-8	yes	To be completed	<i>Introduce grazer's respiration</i>
Solid reactions (dissolution and precipitation reactions)	dissolution of opal silicate (Dissi)	report*, § 1.2.6	no	YES	
Sediment-water exchange	<ul style="list-style-type: none"> • instant in S1 approach without explicit sediment cells • explicit in DELWAQ-G 	manual p. 4-59 report * § 1.2.9	no	YES	
Bioturbation	included in DELWAQ-G	report * § 1.2.9	no	YES	
Macrophytes	in development		no		
Microphytobenthos	process: MICROPHYT	manual p. 5-58	no	Probably Yes	<i>Is a diatom able to creep 1 m up in the sediment? Verify the unit in Fig.2. page17 in the "Structure of GEM"</i>
Birds, Fishes	not included		no		
Habitat suitability	not included		no		

Atmospheric input	user can specify additional nutrient input per cell as g/m ² /d		yes (except for 'Maasvlakte' study)		
Integration scheme	20 options available	report **		<i>Too many possibilities!</i>	<i>Select only 2 or 3 schemes!</i>

manual: GEM documentation and user manual
 report *: delwaq-G.pdf
 report **: numerical aspects.pdf

B: Model application for the Southern North Sea (please answer for both case studies)

B.1 Is the GEM application for the Southern North Sea adequate with respect to:

- model schematization in space and time
Yes, the curvilinear grid with refined coastal domain is one of the best techniques for coastal hydrodynamic modeling. Surprisingly however, this refined model, at least in its 2D version (Maasvlakte impact study), seems to be too diffusive near the coast, and not succeed in confining the plumes along the coast: Noordwijk 20 and 30km have too low salinities in winter, whereas Noordwijk 2km is too salty in summer.
- meteorological forcing
It seems correct. But I did not understand the technique used for simulating the temperature of emerged sediment layers.
- suspended matter forcing
Not very satisfactory! The random cosine forcing around a steady state baseline appears as denying the reliability of the GEM's module simulating deterministically erosion/re-suspension of silt and sand. If deterministic modeling is not yet realistic enough, why doesn't GEM use satellite images climatology?
- nutrient inputs
It seems correct.
- boundary conditions
Difficult to have a definite opinion without looking carefully at the data used. Why are the northwest oceanic boundary conditions constant all over the year?

B.2 Has the model been sufficiently validated to judge its applicability to the subjects listed in Figure 1? If not, what type of validation exercise is missing?

A great part of the comparisons shown between measurements and simulations seem pretty good, but some questions must be re-examined:

** the interaction between pelagos and benthos in very shallow lagoons as Wadden Sea seems totally inadequate in the actual GEM model. The grazers should be treated as state variables, but the phosphate strong underestimation in the water column raises questions about the sediment-water interaction module.*

** why are nutrient simulations so bad (especially silicate and nitrate) in the Wadden Sea GEM Delwaq-G run (see Figures 9 and 10 , p.C-18 in Maasvlakte impact study)?*

** nowhere are shown the time-course of the numerous phenotypes of algal groups calculated by the linear optimization algorithm. Is it rather smooth, or very stochastic-like?*

I also suggest making more quantitative assessment between measurements and simulation, beginning with a simulated vs. measured plot.

B.4 Do you consider that the conclusions that are drawn based on model results in the example studies of application are justified by the model validity / performance? Please elaborate?

Main conclusions in the coastal strip seem to be reliable, but nothing can be said for the Wadden Sea. The conclusion that N-reduction would enhance Phaeocystis production requires some explanation (see Fig. C4a in the "Nutrient reduction scenarios" study).

B.5 Do you have any other comments on the application of GEM for the two specific case studies dealing with the southern North Sea? Please elaborate?

Reports could be easier to read, if they could begin with a concise, but precise description of the tools used (state variables and corresponding equations, parameter values, grid used and numerical scheme).

Summary

Could you please give a summary of your comments on GEM below?

- *GEM is a very comprehensive ecological model, with a great number of biogeochemical processes dealing with inorganic, living algal and detrital forms of N, P, Si, in the water column and in the sediment.*
- *GEM includes a very original treatment of phytoplankton diversity and physiological adaptation, the so-called “BLOOM” sub model. The obtained phenotype succession however has to be shown and discussed, and the corresponding parameter values have to be justified (maximum growth rates are one order of magnitude too low, and dinoflagellates have the highest growth rates!).*
- *The sediment multi-layer approach with numerous biogeochemical processes seems very detailed, but the very bad results obtained in the Wadden Sea rise questions about its efficiency in its actual status. Scientific papers with more details about the results would be helpful.*
- *The grazers should be included as fully state variables, instead of being considered as “modifiable” forcing variables, especially in shallow ecosystems supporting heavy aquaculture.*



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