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Comment

Interactive comment on “An approach for coupling higher and lower levels in marine ecosystem models and its application to the North Sea” by J. A. Beecham et al.

Anonymous Referee #1

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"An approach for coupling higher and lower levels in marine ecosystem models and its application to the North Sea" by Beecham et al.

Summary

The manuscript presents an "approach" for coupling marine models together from end-to-end. To demonstrate this, the manuscript takes two extant models - GOTM-ERSEM, a lower trophic level biogeochemistry model; and EwE, a higher trophic level ecosystem model - and combines these into a single coupled model that spans nutrient cycles to harvestable marine resources. The coupling is flexible enough to allow several operational configurations, including running on two separate, but networked, computers.

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The resulting end-to-end model is used in a series of historical and climate change simulations to illustrate performance. Along the way, the manuscript discusses levels of model integration, issues concerning model compatibility and problems encountered with the specific models used.

General comments

While the manuscript is well-written and relatively straightforward to read and follow, I have a number of major comments:

1. Perhaps I'm failing to see the wood for the trees, but the manuscript is unclear on whether a generic coupler has been developed for end-to-end work, or whether it is simply describing a specific coupler whose principles (but probably not the code) could be applied to other situations; if the former, a name, version number and code for the coupler / "model" needs to be provided; if the latter, the manuscript should much more clearly reflect on these principles (and probably still refer explicitly to the code)
2. While I appreciate that the manuscript is trying to focus on the coupling and not the details of the models, it is far too circumspect on what seems to me a critical detail: how exactly the ecological actors in the two models are related to one another; particularly so where there are direct overlaps between the models (i.e. does an actor in one model completely override its doppelganger in the other model?)
3. On a related point, the manuscript makes a point of referring to automated aspects of the coupling; as I read it, the manuscript presents these as removing some of the drudgery of coupling, but that presupposes that the automatic coupling is done well, something that would appear unlikely given the difficulty of aligning different models
4. More generally, such a coupling exercise inevitably involves a large number of operational decisions to get the models "playing nicely" together; most noticeably at the structural level, the rationale for these decisions is unclear, and little evidence is presented to support the choices that appear in the manuscript; given the fudging to

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parameterisation presented, it seems likely that such decisions would be / were important; a common approach would be to defend this rationale through, for instance, a presentation of the sensitivity of the model to these choices

5. While the authors present, as a strength, the diversity of coupling arrangements that CouplerLib supports, there's no technical evaluation of the efficiency – or otherwise – of these; for instance, whether or not all of the arrangements are equally computationally efficient; among other things, though it's pleasing that quite arcane arrangements can be built, knowing whether they would be absurdly computationally expensive would be helpful for readers engaged in the art of stringing individual models into end-to-end models

6. Finally, the manuscript's discussion goes into great detail on topics that the manuscript itself does not deal with (see comments below); principally, issues of science in a manuscript that is concerned with the technicalities of coupling; if the authors wish to engage with these topics, the manuscript's content should reflect them, otherwise it should be edited to not given them undue attention

In addition, I have a large number of specific comments on parts of the text, and these are presented below.

Overall, while the manuscript deals with an important topic (as it reminds us), its current form is less helpful on this topic than it could be, as described above. My recommendation is that the manuscript only be considered for publication after major revisions to address these aspects.

Specific comments

P1, Title: my understanding of GMD manuscript formatting is that their titles usually refer to the “developments” that they describe by name; here, and more generally, the manuscript makes no explicit reference to a named “approach”; is not Couplerlib a generic piece(s) of software that merits this?

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P1, Title: the term "approach" is rather vague; coupled to the lack of reference to a name for the specific methodology / software used by the manuscript, this leaves things rather indistinct

P2, L2-3: "... and rapidly developing approach to solve developing strategies ..." - very clunky; needs rewording

P2, L13: "can be coupled across networks" - I expect we'll come to some more on this later, but is this really necessary?; are not such coupled models run on the same machine?

P2, L24: "annual inputs" - of what?; one presumes food, but a clarifying noun would help

P2, L26-27: "... although the benthic linkage modelled was purely at the detritus level ..." - this sounds serious; hope it's discussed later

P2, L28: "... and showed the system to be nutrient limited AND relatively unaffected ..."; insert "AND"

P3, L2: "Ecosym" mentioned - should be "EwE" for clarity

P3, L6: "End to end modelling" - this should at least be defined so that readers are aware of which ends are being referred to

P3, L11-12: "... a highly cited paper ... personal communication" - first of all, why not just report the number of cites?; secondly, I make it 70 since 2010, which compares to 88 for the Fulton paper from the same year - perhaps remove this statement entirely

P3, L17: "Given the choice ..." - by "de novo" here does one include model codes that are formally combined (i.e. not just coupled) from extant codes?; the dichotomy presented here appears to preclude this - or leave it somewhere between the two cases

P4, L1-4: "sociological aspects" - this is a fair point, but we should also be able to do things differently should such aspects interfere with our ability to model things correctly

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P4, L17: "Process-oriented" - with GOTM-ERSEM this is more "biogeochemical process-oriented" because the physics is rather truncated to vertical processes only

P5, L9: "... impressively ..." - this is a bit gushing; why is it impressive?; expand slightly to make this clearer for the reader

P5, L26-27: "... at different time and space scales ..." - is this examined here at all?

P6, L7: what is this "data consistency"?; can it be made clearer, as I can imagine a number of things that would fall under this term

P6, L25: "We can define a series of degrees ..." - a clear statement at the end of this section indicating which of this series is being explored here could be helpful

P7, L7: "... of indirection" - is this the right term?; the text tries to clarify what's meant here, but it still sounds odd

P7, L8: "will not allow any form of data conversion" - this sounds unlikely; it's not at all clear why intimately coupled models, such as those being described here, should struggle with data conversion; I presume something more subtle is being meant

P7: in general, this section might benefit from a few more examples of models that fall into the different categories delineated by the manuscript; the "direct coupling" models would be well-represented by the likes of climate models, for instance

P7, L22: does "networked coupling" distinguish between models being run on separate (but networked) machines and models being run on different nodes of the same supercomputer?

P8, L3: offline coupling, as described here, is a fusion of genuine one-way coupling (i.e. model Y is fed by model X but cannot interact with it) and one-way-by-necessity coupling (i.e. infrastructure prevents two-way coupling); the latter is suggestive of using inappropriate technology rather than an active modelling choice

P8, L6: I'd reiterate that a statement saying which of these is examined here would be

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helpful

P8, L19-23: the principle described here sounds good, but it suggests a level of automation that, frankly, sounds implausible (i.e. the two models have a pre-run "chat" about what their inputs and outputs are, somehow work out which of these are aligned, and then line these up prior to running); my suspicion would be that this would only work if a human was involved as part of the process that brings the two models together; otherwise one might have the situation where the models automatically decide to couple themselves in an incorrect (but superficially plausible) way

P8, L24: "Windows only" - was EwE deliberately chosen to provide an example of the coupler operating across this computing divide?

P9, L7-17: this all sounds rather onerous; perhaps expand on why such a mode of working is required; it seems like a recipe for disaster to me, when the models could perhaps be more straight-forwardedly coupled

P9, L19: expand on "sockets"; this level of computing is likely to lie outside the experience even of many programmers

P11, L20: "... a registered correspondence between all functional groups ..." - this, I suspect, is a key part of the coupling process; I appreciate that, from a technical computing standpoint, it's not very interesting, but the ease with which this is facilitated by the "approach" used here will be important to potential adopters

P12, L18: "We consider this to be a reasonable approximate ..." - has this been tested?; in theory it sounds fine, but ...

P13, L3: is it worth including a screenshot of the GUIs when the end-to-end coupled model is in action?

P14, L1: while it's not a focus of this manuscript, a bit more information about the two models is in order as far as I'm concerned; for instance, food web diagrams of the two models; perhaps colour coordinated to show where the overlaps occur (Table 1 only

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partly fulfils this function)

P14, L17: were many / any variants of the "coupling" described here tried?; a large part of the successful performance of the model will derive from how this is done; and I'd have thought that the simplicity of the model would allow many alternatives at this stage to be quite thoroughly explored

P15, L1: "personal communication" - given the potential significance of this decision, I don't think that leaving it as an unexplored "pers. comm." (especially from 5 years ago) is good enough; if nothing else (and per my previous remark), performing variant simulations on this point would be useful for demonstrating the power of the coupler used here

P15, L8-20: has any of this been formally explored?; it reads like a lot of reasonable assumptions, but ones that haven't been tested

P15, L16-17: "... temporal variability ... immediate variability ..." - presumably these statements refer to the frequencies of coupling above?; i.e. "immediate" means daily

P15: I might just be missing it, but how are the phytoplankton classes in EwE and ERSEM squared off against one another?; does ERSEM overwrite the phytoplankton in EwE, or do they persist in some strange overlapping manner?; this seems a major overlap for the two models

P16, L3: "Couplerlib will check in its conversion dictionary ..." - again, this sounds automated to a degree that seems liable to fail without checks; are these done by a human?

P16, L6: section 2.7 should really come after section 2.8; it could even be classed as results; albeit "initial tuning / validation"

P16, L7-13: a few numbers to qualify the descriptions here would be helpful (e.g. mesozooplankton levels between the models; productivity changes; seasonality)

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P16, L14: "... very much reduced ..." - quantify

P16, L20: "... it was necessary to add a function to reduce predation ..." - this does seem something of a bodge; was this function the only modification attempted to correct problems, or is it the result of a spectrum of modifications?

P16, L24-25: "... metabolic costs were reduced to 0.3 for adults and 0.0 for juveniles ..." - zero metabolic cost?; this sounds ill-advised; again - were alternatives explored?; if so, how were decisions made?; or is this ad hoc, "expert" judgment?

P19, L24-26: a dinos vs. diatoms result is mentioned here, but I thought EwE only had a generic phytoplankton in it?

P20, L3: "... are indicated" – why not "shown"?; this sounds like you're not convinced by the results

P20, L7-L26: the results shown in Figure 5 appear to show strong spin-up artifacts in the coupled model; i.e. populations of some species seem to approach an equilibrium biomass after 10 years; is this interpretation (i.e. artifact) correct, or is the transient behaviour shown actually reflective of some key forcing (e.g. fishing pressure)?; if the former, my feeling is that the model should really have been initialised from more sensible conditions (e.g. those derived from a spin-up under climatological forcing), as it's difficult to take the ostensibly historical results seriously otherwise

P20, L25: "... we can produce ..." – did you?; this statement could be more clearly worded along the lines of "these results are preliminary, a range of tuning approaches is available to improve them, and this will form the basis of future work"; or some such

P21, L3: is there a reason for using older IPCC scenarios (A1B, B1) rather than the recent RCPs?

P21, L6: are these nutrient scenarios derived from future projections, or are they just idealised scenarios?; make it clearer

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P21, L21-25: this rather narrative explanation could be quantified somewhat; by positing two explanations, the manuscript immediately raises the question “which is more important?”

P22, L22: ERSEM is described as being optimised for a particular site, while EwE is more broadly tailored – is there not a more general parameter set for ERSEM that could have been used instead?; that would, one imagines, make the success of the model coupling more likely

P23, L25: “... of the Si dependency ...” – what “dependency” is being meant here?; what is dependent on what?

P24, L24: “Modified EwE parameterisations ...” – are these described elsewhere?; I only remember something about sandeels

P28, L28-29: “... which model the dynamics of plankton in response to changes in the geochemical model” – what does this mean?; “... which ground plankton dynamics in the biogeochemical cycles of major chemical species such as nutrients and oxygen”?

P22-30: a general comment I would make is that a lot of the material discussed here is not really explored with the model / coupling framework described here; e.g. issues around parameterisation, overlapping biological groups between the models, standard time periods of the models (days vs. years), geographical extents covered by the models, etc.; these points are valid when discussing how end-to-end models should be built (but often because end-to-end models are built using models with radically different underlying assumptions), but the manuscript does not investigate them at all; to my mind such material, while interesting, should probably be trimmed back; instead, attention should focus on the meat of the manuscript, the coupler itself

Table 1: this table should contain information about the sizes of the organisms compared; at present it’s unclear why some of the decisions have been made; also, a bit more commentary on the trophic overlaps would be useful - there are a few places

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where matches have been made despite functional differences between the models; e.g. herbivorous / omnivorous zooplankton in EwE are aligned with omnivores in ERSEM; epifaunal macrobenthos are grazers in EwE and predators in ERSEM; indistinct meiofauna in models are aligned; a single phytoplankton class in EwE is aligned with a whole suite of phytoplankton in ERSEM (some of whom might even be mixotrophs)

Table 2: where are these "scenarios" derived from?; they seem rather idealised

Figures: to reiterate, a diagram of the two models' components, specifically identifying their overlap would be helpful

Figures 1-3: these seem to overlap significantly; or it's not clear that they don't

Figure 3: seeing the coupled models this way, it occurs that the coupled framework could well impose a significant compute cost; is there a way of disentangling the cost from coupling this way from a more conventional "code meld"?

Figure 3: is there any way that this diagram could be reconfigured as a linked set of flow diagrams?; at present it's admirably detailed but practically impenetrable; also, please put names ("ewe.exe", "ersem.exe"?) on the grey boxes – everything else is already tagged up

Figure 4: while there is some vertical structure in these plots, it's very difficult to see; I would suggest converting the plots to simple line plots of averaged or integrated biomass; this would make the seasonal cycle much clearer (possibly with a log y-axis); alternatively, change the colour map (less colours, more gradients) and put the diatoms, flagellates and dinos on the same colour scale

Figure 5: looks like there are some spin-up issues here – or is this a function of real historical change in fishing, etc.; if the former, is it worth conditioning the initial state somewhat?; e.g. by spinning up an initial state based on climatological meteorological forcing?

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Figure 6: at first I didn't like this figure but, on reflection, it does a good job of illustrating how the inter-class changes differ between the high / low nutrient split, but is obviously less good across the two climate change scenarios

Interactive comment on Geosci. Model Dev. Discuss., 8, 5577, 2015.

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8, C1643–C1653, 2015

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