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Interactive comment on "COSTRICE – three model online coupling using OASIS: problems and solutions" *by* H. T. M. Ho et al.

Anonymous Referee #2

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General comments

This paper describes the set-up of a 3-component regional coupled system assembling regional atmosphere, ocean and ice models. The paper details the component models used and the experiments realised (section 2), on the technical set-up of a coupled model using the OASIS3 coupler (section 3.1), the definition of suitable coupling frequencies (section 3.2), and on whether the coupling should be done by exchanging state variables or fluxes (section 3.3). The results of a first coupled simulation are only briefly described at the end of section 3.

This paper certainly needs major revisions before being considered for publication. I

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think that the current content which focuses on the choice of suitable coupling frequencies and quantities to exchange does not deserve publication per se. Firstly, many of the arguments and analysis proposed regarding these choices are not convincing or are not of great interest (see my detailed comments below). The most interesting part of the paper is the brief description (at the end of section 3) of the first results of a short coupled simulation (CPERAi) that show that these results are closer to the observations than results of forced simulations. It seems mandatory to me to focus the paper on the scientific results of a longer coupled simulation (as only mentioned in the conclusion). The presentation of these scientific results are interesting per se and would also validate the technical work described in the current paper.

Major detailed comments (in the order of the text, not in order of importance)

1- p.3266, L8: The authors should define more specifically what they call the "equilibrium time". I cannot really understand the logic of the whole paragraph. First they start by mentioning that "the equilibrium time of the atmosphere boundary" (the atmosphere boundary layer, I suppose?) "or the snow and surface ice layer" is 24 h and then describe the time scale of the changes in surface winds, of the mixing of upper ocean heat, SST, etc. Then they oppose the timescale of change of the ocean skin temperature and the equilibrium time of the ocean mixed layer. And they conclude that the "component models should be coupled more often than the equilibrium time of the climate components". I do not understand exactly what is the equilibrium time of the climate components. Is it the equilibrium of the ocean as a whole, or of the atmosphere component as a whole, what ever this means exactly? The whole argumentation about the fact that the coupling time should not be the equilibrium time is not convincing as there is no precise definition of this equilibrium time and it is not clear what equilibrium time is considered. Finally, I am not sure why they need to mention experiments with 1-3h air-sea coupling (Terray et al. 2012, Danabasoglu et al. 2006, and Ham et al. 2010) to conclude that "the diurnal variations of oceanic quantities are not simulated with the

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1-day air–sea coupling"; this conclusion seems quite obvious to me, even without any experiments! These papers were in fact probably analysing the impact of missing the representation of the diurnal cycle on the ocean modelling results.

2- p.3276, 2nd paragraph: From what is written, it looks like the one-month spin-up does not include two-way coupling. If there is no coupling, how can this be considered a spin-up of the coupled system? But more importantly, how do you justify a so short spin-up when you write yourself: "For a regional ocean model, Berni et al. (2008) set 5 yr for the spin-up time." I think you can justify this short spin-up only if you can show that some kind of equilibrium is reached (at least for the surface and the upper ocean levels). What you write at the end of the paragraph "For the longer runs, we will analyse the spin-up behaviour more in detail and choose the spin-up time accordingly" should be done for this study too.

3- p.3278, paragraph 3.2: I think the justification of the choice of 1 hour coupling for the atmosphere to ocean data exchange is not convincing. Firstly, nothing is written about the frequency of the forcings used for that "uncoupled version of CCLM from 1948 to 2010", which certainly influences the results onto which the analysis done in this paragraph is based. I think the distribution shown in Fig. 6 just shows that small changes in the wind magnitude (+-5%) occur on an hourly timescale and that the bigger changes happen on a longer timescale (3 or 6 hours). It would be important to have an hourly coupling frequency if large hourly changes (d1) were happening with high FC. It is obvious that as written at the bottom of p.3278, "if CCLM provides wind information every 3 or 6h, the hourly changes are missed" but from my point of view, Fig. 6 does not justify this choice. It would be better to draw a figure of the autocorrelation of the wind at different lead times and choose a coupling frequency of the order of, or slightly smaller than, the decorrelation timescale.

4- p.3279, 1st paragraph: The justification of the 3-hour coupling timestep invoking the density and the solidity (!) of the ice is simply wrong. It is well know that the heat capacity (thermal inertia) of ice is smaller than water. So the coupling frequency should

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be higher for atmosphere to ice than from atmosphere to water.

5- p.3279, L11-12: Again nothing is written about the frequency of the forcings used in the "28 yr simulation of TRIMNP from 1980 to 2007"; this can certainly influence the results onto which the analysis presented at page 3279 is based.

6- p.3280: I understand the analysis done on p. 3279 but I am not convinced that it justifies the conclusion "we conclude that a 3-h interval is recommended to pass SST from TRIMNP to CCLM and CICE". Why not 1h and why not 6h? The analysis just shows that a smaller coupling period will capture smaller changes, which seems obvious, but there is nothing to conclude on what are the important timescales to capture.

7- p.3280: the author concludes "This strategy helps to save computing time"; This may be true but before doing alternative experiments, the relative cost of the coupling time with respect to the model computing time should be analysed. In this case, it would not be surprising to me that the coupling time be very small with respect to the computing time anyway!

8- p.3281: To answer the question on whether state variables or fluxes should be exchanged, the author compares the quality of two uncoupled (i.e. ocean forced) simulations, STERva into which atmospheric state variables are used into bulk formula to calculate the heat fluxes and STERhf where the forcing is directly the heat fluxes as calculated by the atmosphere model. As the resulting fluxes are closer to ERA-interim reanalyses in STERhf than STERva, the author concludes that it is better in the coupled simulation to calculate the fluxes in the atmosphere and transfer them to the ocean. This might be the case in this coupled model but from my point of view this is simply linked to the fact that the ocean bulk formulae are not providing good results. What is presented here is not a physical justification of coupling through fluxes instead of coupling through state variables. It is well known that as the fluxes are not a linear function of the state variables, it is not physically justified to regrid the higher resolution variables (here the ocean SST) on the lower resolution grid (here the atmosphere) and

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calculate an average flux at the lower resolution. It is more physically consistent to calculate the fluxes at the highest possible resolution and then regrid the fluxes. This is certainly the physical reason why "Doescher et al. (2002) planned to calculate the flux on the finest grid of the ocean model for all variables for the next development step of RCAO". The problem of the non-matching sea-land masks between the ocean and the atmosphere detailed by the author is only another drawbacks of calculating the fluxes on the coarsest grid.

9- p.3282 and 3283: results of the coupled simulation (CPERAi) that show that the coupled results are closer to the observations (OISST) are very interesting and should be described in a section per se (currently, they are too briefly described at the end of section 3). The results from a longer simulation of CPERAi, just mentioned in the conclusion, should be analysed and should be the main focus of this paper.

10- The whole text should be revised and reformulated by a native English speaking person. As examples, I think that the following sentences are not written in proper English:

-p.3264, L6: To date, “coupling” has not been a new problem but is frequently applied ...

-p.3264, L20: ... while the climate of the Baltic Sea basin is merged by continental and maritime climates due to the geographical location, ...

-p.3264, L27: The North Sea was also focused in some studies such as

-p.3267, L1: "... some others studies ..." (should be "... some other studies...")

-p.3272, L13: "we spend 3 nodes with 64 tasks per node to run ..." (should be "we used 3 nodes with 64 tasks per node to run ...")

-p.3272, L15: "The distribution of task will change when CCLM runs with higher resolution" (should be "The distribution of task will change when CCLM will run with higher resolution")

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-p.3273, L16: "... compile OASIS3 in your computer ..." (should be "... compile OASIS3 on your computer ...")

-p.3267 L27, p.3272 L5, p.3275 L10, p.3280 L15, p.3284 L5, p.3302 Fig.5, : use of "amongst" (should be replaced by "between")

-p.3276, 1st parag: "they alter significantly timestep-by-timestep", or "alternative changes in wind direction may obscure wind speed values"

-p.3276, 2nd parag: "These unrealistic phenomena should not be accounted for to avoid a wrong feedback to the ..."

-p.3277, L11: "leading to rapid variations of SST are rapid on small spatial and temporal scales"

-p.3278, L12: "considering that alternative changes in wind direction do not obscure the mean value"

Other detailed comments

p.3265, L12: I am not sure about the definition of "online coupling": "online coupling is defined as a process in which two or more models run in parallel and exchange data more often to take interactions and feedbacks into account". More often than what? Why would the frequency of the exchanges define whether the coupling is online or off-line? I would say that it is online as soon as there is a feedback between the components, independently of the coupling frequency and whether or not the components run in concurrently or sequentially.

-p.3267, L22: I think the sentence "and hourly coupling may be due to the output writing time period." does not really mean anything. The hourly coupling is not linked to the output writing time period by magic. The sentence should probably be rewritten: ""and hourly coupling may have been chosen by convenience as it corresponded to the

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output writing time period and was therefore easy to implement technically speaking."

-p.3267, bottom and p.3268, top: The authors start by saying that "Another coupling problem is the adequate selection of variables that are exchanged amongst the component models". Then they mention that they will not consider gas exchange in the current study but do not justify why. Then they mention many studies about the dominant fluxes but without giving any conclusions or justifications about the precise coupling fields finally chosen or not in your study.

-p.3270, 1st paragraph: when describing the ocean model, I do not understand what the authors mean by "surface boundary conditions (e.g. pressure, wind, temperature, etc.) are taken from data of regional atmosphere models (such as CCLM, REMO, etc.)." In the current study, I suppose that the surface boundary conditions are the coupling fields provided by CCLM and not any data of regional atmosphere models such as REMO!

-p.3270, last sentence: I do not understand what the authors mean by "the lack of time calculation for leap years in the current version of CICE is supplemented to allow a time setup that corresponds to CCLM and TRIMNP." It s a bit awkward to mention this at the end of the paragraph describing the thermodynamic and dynamic characteristics of CICE.

-p.3271, L7 and 8: I do not understand why the authors write "The current OASIS3 version is compiled and run on NEC SX6, IBM Power4 and Linux PC cluster". They certainly did not perform your experiments on these 3 platforms. I think there is no need to provide all these general details on the coupler (that do not even correspond necessarily to the functions used in the current coupled system) but just a reference to Valcke 2006 (the reference on OASIS3 User Guide) or to the more up-to-date Valcke 2013 (S. Valcke, 2013: The OASIS3 coupler: a European climate modelling community software, Geosci. Model Dev., Dev., 6, 1–16, doi:10.5194/gmd-6-1-2013) should be enough.

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-p.3271, 1st line: why is the CICE domain so small with respect to the ocean TRIMNP domain? Any justification a priori?

-p.3272, 2nd parag: I suppose that STERva and STERhf are "ocean" standalone experiments? If so how are generated the air surface temperature, humidity, and wind speed (for STERva) and heat flux (for STERhf) from CCLM? Is CCLM run offline beforehand with some forcings as boundary conditions? This should be better explained.

-p.3272, L18-19: The authors write "during the time from May to September, the skin temperature of CCLM only comes from the sea surface temperature of TRIMNP". Is the period pre-defined and imposed independently of the results of your modelling system? If so, it should not be. Later in the paragraph, the authors write that "latest break-up of ice season is in June [...] But in general, the date of break-up is in April and May." Can't CICE pass on an appropriate surface temperature i.e. ice skin temperature when the surface is covered with ice and sea surface temperature when there is no ice? It should be better explained how the "switch" to a sea surface temperature of TRIMNP is managed.

-p.3273, L19-21: I do not see the relation between the second part of the sentence "and long waiting times for the communication between processors can be avoided" with the first part : " we use one processor for OASIS3 as there's not much gain using more than one processor.". Long waiting times can be avoided by doing what? What is the link with using OASIS3 on only one processor?

-p.3274, 1st parag: I think the description of the field sent by CICE to CLM (ice skin temperature) is missing, although it appears in Fig.3

-p.3274, L20-22: The sentence "Another library of OASIS3 "prism_def_partition_proto" is also used to define the transformation how the fields are exchanged by slave processors or by only the master processor" is not exact. "prism_def_partition_proto" is not a library, it is a routine of the library. And it is not used to define any tranformation. It is used by each process to describe the partition corresponding to the coupling field

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that will be sent or received by the process. In this case, only the master process will send/receive the field, so only the master process calls prism_def_partition to describe a partition covering the whole grid.

-p.3274, L26-27: The sentence "To compile each model component linked to the PRISM library one has to set the link of the library in its makefile." is so obvious that it should be removed from the text.

-p.3275, L5: I understand that "Some errors are caused by the interpolation" (even if I would write "Some imprecision is caused by the interpolation"); but I really do not understand what is meant by "hence the less interpolation is used the less difference between the source grid and the target grid occurs". One cannot really use "more" or "less" interpolation. Interpolation is needed because the grids are not the same, so the concept of "less interpolation" does not make any sense to me.

-p.3275, L7-9: "the total radiation energy flux or total precipitation are calculated in CCLM before being passed to TRIMNP or CICE even some or all of its components are transferred" does not make any sense to me; what is meant by "its components"? Components of what?

-p.3275, L10: "The temporal coupling process amongst the three component models ..." would be better phrased as "The temporal coupling algorithm between the three component models ..."

-p.3275, L13-14: The sentence "At every running time step, each model sends the required exchange variables to OASIS3." is not exact. At every running time step, the routine to send or receive the coupling fields are called but nothing is effectively sent or received between the model and OASIS3 if the time step does not correspond to a coupling timestep.

-p.3275, L18-23: I am not sure it is really useful to describe the debugging phase of the set-up of the coupled system during which different SEQ and LAG indices were tested:

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"For example, firstly, the state variables from CCLM are sent to TRIMNP (SEQ = 1), then the SST, salinity, etc. of TRIMNP are passed to CICE (SEQ = 2) and then sea ice skin and water temperature, etc. of CICE are passed to TRIMNP (SEQ = 3) and so on. Unfortunately, in our coupled system, the deadlock happens after running 3 h because at the same time all three models send and need data and that results in a loop of waiting." Furthermore, this is described using the present tense as if it was still occurring today. Mentioning only that in order to avoid deadlocks, "the three models are set to run simultaneously (SEQ = 1 for all exchanged fields) and LAG is set ..." seems enough to me.

-p.3275, L26: "Consequently, the exchange time is one running time step earlier than the coupling time step ..." does not make any sense to me. It should be ""Consequently, the sending time is one running time step earlier than the receiving time step ..."

-p.3278, L19: to help the user to make the link between the text and the figure, it would be useful to write: "... changes up to $\pm$5% with a total FC of 32–42% (adding the [0-5] and [-5,0] FC)" while ..."

-p.3278, L26: same remark as above: it is not straightforward to understand that the numbers "46 % and 64 % for d3 and d6" are obtained by adding all FC for [40,60], [-40,-60], [60,80], [-60,-80], [80,100], [-80,-100] (if I understood well).

-p.3279, L6-7: I do not see a direct relation between the relative density of water and air, their relative thermal conductivity, and the longer timescale of "considerable" changes of SST ("one to several hours"). More simply, the higher thermal inertia of water could be invoked.

-p.3285: It is funny to be thanked even before having made the above suggestions and comments!

Interactive comment on Geosci. Model Dev. Discuss., 5, 3261, 2012.