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Ocean Science
Discussions

Interactive comment on "A comparison between gradient descent and stochastic approaches for parameter optimization of a coupled ocean–sea ice model" *by* H. Sumata et al.

Anonymous Referee #1

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General Comments:

This is an informative paper on exploring the parameter space in a sea ice model. The concepts are broadly applicable to other sea ice models, although the exact methods may only be useful for a limited class of model. The scope of the paper is well explained and justified, and it represents a useful addition to sea ice modeling literature. Data and methods appear to be rigorous, and descriptions are sufficiently detailed so as to make the work repeatable by others in the field. I suggest this should be accepted with minor edits, with comments needing to be added about three aspects of this methods and results, a possible minor edit to the title, and a limited number of grammatical

corrections. Suggested changes to some figures may make the work clearer to the uninitiated reader.

Specific Comments:

1) Choice of sea ice model and cost function:

The sea ice model in NAOSIM has a parameter space that is especially subject to tuning. This makes it an excellent choice for this study.

However, for modeled sea ice mechanics, the value P* is not dependent on the potential energy of floating sea ice, and it is interrelated with the somewhat arbitrary h0 lead closing parameter. For sea ice thermodynamics, the fixed broadband albedos and albedo ratios have limited applicability to other sea ice models. Therefore, a brief comment should be made within the cost function description, or perhaps in the conclusions, that the choice of J is particular to this study and other Hibler (1979) class models, and less to some more recent sea ice codes, such as CICE.

Within the cost function description and in Table 2, it should be made clear that cdwin, cdlat and cdsens are neutral coefficients, assuming that no surface stability is being calculated in a thin surface layer beneath the NCEP reference levels of 2m and 10m. If neutral conditions were not assumed, a more detailed description is required under the model section of the boundary layer calculations. If neutral atmospheric buoyancy was assumed, a brief comment (one sentence) should be made on the limitations this places on the applicability of the cost function to more comprehensive ice-ocean or fully coupled models, without detracting from the comparison of the two optimization methods provided in this study.

2) Physical meaning of parameter space obtained:

The paper is focused on two methods of parameter space exploration, and perhaps more consideration should be attributed to the physical meaning of the final parameter space obtained by the methods.

The end desire of all parameter space exploration is to obtain a physically realistic model. Therefore, at least some discussion should be given to the effectiveness of the methods, combined with the cost function, in achieving a physically realistic solution using the optimized parameter space. It is insufficient to conclude that because the parameter space was consistent between experiments, the parameter exploration was productive. In particular, the optimized albedo values obtained, exceeding 0.97 for frozen snow, seem excessive. These results may be a comment on the particular model and cost function chosen rather than the optimization methods. However, the physically realism (or not) of the final parameter space should be discussed, perhaps accompanied by analysis attributing reasons for any unphysical values obtained. Note that by optimizing the model for only one year (2003), results presented in 12 (b), (c) and (d) may not indicate more general physical realism for those particular variables, since the spinup time for the model exceeds the experimental window. The paper limits the scope of the investigation to this short window early in the manuscript, and the reasons given are legitimate. However, a comment should be made about the broader physical meaning of the optimized parameter space.

3) Comment on physical consistency of models:

It should be noted under the model description that the smoothed model, while expedient for testing optimization procedures, may, in fact, contravene physics assumptions or laws in the underlying model on some occasions, especially true for sea ice mechanics.

4) A minor point on the title:

Although the title of the paper refers to an “optimization of a coupled ocean-sea ice model”, the optimizations performed are focused exclusively on the sea ice model, and on the ice-ocean and ice-atmosphere coupling parameter space. Since performing this research without at least a coupled ice-ocean model would have been challenging, it was a worthwhile tool for the task. However, the optimization you may like to consider changing the title to “A comparison between gradient descent and stochastic

approaches for parameter optimization of a sea ice model”.

Technical Corrections:

The following minor grammatical or typographical corrections need to be made:

1) P. 3595, Line 24: “. . ., they had to perform more than a hundred experiments. . .” 2) P. 3598, Line 13: “. . ., we provide a useful parameter optimization. . .” 3) P. 3605, Line 16: “. . .without a special programming effort” 4) P. 3613, Line 17: “It should be kept in mind”

Figures:

Presentation of results from the Micro Genetic Algorithm could be made clearer in Figure 12. First, I would like to suggest that the standard deviations from both the FD and GA methods are shown in two rows of panels in Figure 10, thus removing the bottom line of panels from Figure 12 and aid intercomparison. Next, make Figure 12(a) a stand-alone figure. Finally, I would like to suggest that the remaining panels in Figure 12 are presented in the same format as in Figure 8, so that easy comparison can be made with observations, and that the optimized FD values from figure 8 are repeated in a first column in Figure 12, make figure 12 a 3x3 panel figure. It is assumed that the GA figures in 12 are for optimized values, but this is not explicitly stated in the panel titles in the same way that Fig 8(a), (e) and (h) all abbreviate ‘opt’ to indicate this. Consistency between figures 8 and 12 would make this clearer.

Interactive comment on Ocean Sci. Discuss., 9, 3593, 2012.