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Ocean Science Discussions

Interactive comment on "On the assimilation of ice velocity and concentration data into large-scale sea ice models" *by* V. Dulière and T. Fichefet

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General Comments:

This paper discusses the impact of assimilation of sea ice motion and concentration into a global ice-ocean model. Data assimilation holds a lot of promise in improving simulations of sea ice and polar climate, yet assimilation has been limited in sea ice models. The approach here is to conduct identical twin experiments to test the model sensitivity to the assimilated data. This approach has not been employed for sea ice. Thus, this paper represents an important step towards a fully-assimilative model. The results demonstrate that, while assimilation can improve model results, the assimilation methods must be implemented with care or results can be degraded. The paper highlights difficult issues (such as mass conservation, etc.) inherent in assimilating sea

ice data that are unique from assimilation within atmospheric and ocean models.

Specific Comments:

1. The introduction is a good discussion and summary of the state of Arctic sea ice concentration and thickness. In the thickness discussion (pg. 267, lines 10-30), Winsor (2001) and Holloway and Sou (2002) are cited to discuss whether the Rothrock et al. (1999) results are indicative of a displacement of ice and/or interannual variability instead of a thinning trend. However, there are several papers since then that reconfirm the thinning and I think the authors can make a more definitive statement on the thinning ice cover. For example:

Rothrock, D.A., J. Zhang, and Y. Yu, 2003. The arctic ice thickness anomaly of the 1990s: A consistent view from observations and models, J. Geophys. Res., 108(C3), 3083, doi: 10.1029/2001JC001208.

Yu, Y., G.A. Maykut, and D.A. Rothrock, 2004. Changes in the thickness distribution of Arctic sea ice between 1958-1970 and 1993-1997, J. Geophys. Res., 109, C08004, doi: 10.1029/2003JC001982.

These papers analyze the submarine data in more detail and conclude that thinning has indeed occurred and the sub observations cannot be attributed (at least not completely) to shifts in the location of thick ice. Other results also show agreement with overall thinning, e.g.:

Perovich, D.K., T.C. Grenfell, J.A. Richter-Menge, B. Light, W.B. Tucker III, and H. Eicken, 2003. Thin and thinner: Sea ice mass balance measurements during SHEBA, J. Geophys. Res., 108(C3), 8050, doi: 10.1029/2001JC001079.

Also, there are substantial indications of thinning from proxy data, e.g., of ice age, that indicate an overall thinning (i.e., younger ice and a reduced area of older, thicker ice):

Rigor, I.G., and J.M. Wallace, 2004. Variations in the age of Arctic sea-ice and summer sea-ice extent, Geophys. Res. Lett., 31, L09401, doi: 10.1029/2004GL019492.

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Fowler, C., W.J. Emery, and J. Maslanik, 2004. Satellite-derived evolution of Arctic sea ice age: October 1978 to March 2003, IEEE Geosci. and Remote Sens. Lett., 1(2), 71-74.

Comiso J.C., 2002. A rapidly declining perennial sea ice cover in the Arctic, Geophys. Res. Lett., 29 (20), 1956, doi:10.1029/2002GL015650.

I would suggest that at least some of these papers be added to the references.

2. In the twin experiment, why was 1992-1993 forcing used to replace the 1995-1996 for the assimilation experiments? This isn't unreasonable, but a more straightforward, and perhaps more useful approach would be to simply perturb the original 1995-1996 (e.g., add in a bias and random noise to data). This would allow one to know and to control the actual error, allowing one to examine the relationship between the "model" error and the final assimilation error. Also, 1992-1993 may have no relation to 1995-1996 (e.g., wind patterns completely different). By perturbing the same year, it seems like you're more likely to simulate real data assimilation (i.e., there will be errors, but the general conditions should be reasonably close to reality).

I wouldn't suggest redoing any experiments for this paper, but it might be worth adding a short discussion on the rationale of using different years data for the test assimilation and what reason (if any) 1992-1993 was chosen (over any other two years). And perhaps the perturbation test could be done in a follow-up test.

3. On the assimilation scheme, if I'm reading things correctly, basically, the OI was implemented only at single grid points. In other words, the "data" (i.e., model result from the twin experiment) from a given grid point is assimilated into the model at that grid point, and only at that grid point. However, OI can take advantage of the fact that there is spatial correlation between observations at a certain length, depending on the character of the observations. For example, ice motion is well correlated out to at least 800 km. Thus, even at 100 km grid spacing, potentially several "observations" could be assimilated at each grid cell. Increasing the number observations should lead to

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improved results. Is there a reason why only one “observation” is used for each grid (perhaps just simplicity? or perhaps it makes it easier to assess the assimilation?)

4. In the comparison with observed concentrations (from Comiso) (pg. 276, Fig. 4), it should be noted that the passive microwave observations also have biases in concentration, particularly during summer where surface melt is seen by the algorithms as reduced concentration.

5. As mentioned above, this paper is particularly valuable in illuminating the issues involved in developing assimilation strategies for sea ice and the unique problems sea ice poses for assimilation compared to the atmosphere and ocean. For example, the discussion of the impacts of selection of weighting coefficients and the trade-offs that must be made between thickness vs. volume conservation are very relevant to the development of sea ice data assimilation methods. The authors may wish to look at a just-published paper that discusses similar issues:

Dai, M., T.E. Arbetter, and W.N. Meier, 2006. Data assimilation of sea-ice motion vectors: Sensitivity to the parameterization of sea-ice strength, Annals of Glaciology, vol. 44, 357-360. Available at: http://www.igsoc.org/annals/

Figure Comments:

Figure 2: The dashed line is very thin and faint and didn’t print completely on my copy. Suggest making this thicker like the solid arrows.

Figure 3a: It would be nice if the grid marks from where there isn’t ice (and thus no ice motion) could be removed.

Figure 4: Just for clarity, suggest that the caption explains that the pole hole in the observations is due to lack of satellite coverage in that area.

Technical Corrections:

1. Pg. 268, Line 29: "does not allow to correct" - awkward, rephrase

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2. Pg. 270, Line 17: "abovementioned" –> "aforementioned", or "The studies mentioned aboveĚ"

3. Pg. 271, Line 3: "Sect. 2." –> "Sect. 3", also suggest spelling out "Section" (also on Line 4)

4. Pg. 272, Line 16: "Un upstream" –> "An upstream"

5. Pg. 273, Line 6: "Ice is not allowed is grid" –> "Ice is not allowed in grid"

6. Pg. 275, Line 19-20: "runoff and motionless" –> "runoff and a motionless"

7. Pg. 275, Line 26: "flagrant", suggest another word, e.g., "substantial", "significant", etc.

8. Pg. 276, Line 1: "too thin an" –> "too thin of an"

9. Pg. 279, Line21: "supposed", I believe the authors mean "assumed"

10. Pg. 279, Line28: "deteriorates a bit the ice thickness", awkward - rephrase

11. Pg. 280, Line 13: "To be total", assume authors mean "To be complete"

12. Pg. 281, Line 3: "when time times goes by", extra word

13. Pg. 284, Line 8: "pretty good analysis" - colloquial and unspecific, suggest rewording

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Interactive comment on Ocean Sci. Discuss., 4, 265, 2007.