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Ocean Science
Discussions

Interactive comment on "Eddy length scales and the Rossby radius in the Arctic Ocean" *by* A. J. G. Nurser and S. Bacon

Anonymous Referee #1

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The idea of providing an estimate of Rossby radii for the Arctic Ocean as announced in the title is excellent. The idea is even better since de facto the authors give estimates for the whole Arctic Mediterranean. The task is addressed in a straight way: they use a high resolution model, check its performance by comparison with observations in all basins up to the shelf edge and then compute the Rossby radii up to order 2. However, I recommend publication of this ms. only after very major revision. Actually only after this revision I would be able to review it.

The slight discrepancy between title and real content of the paper points to the overall weakness of the paper: It is written in a very sloppy and imprecise style, suitable for informal lab seminars but certainly not for a scientific publication.

I stopped reading at page 7, because I don't consider the task of a reviewer solving

riddles posed by authors in an immature draft.(Note: Page 7 refers to a former submission to another journal which I reviewed already. Since the authors simply recycled their initial ms. without considering the reviewers comments I just recycle my review as well. Easy job for both sides.)

Here is where I have questions: (The numbers refer to lines of the earlier submission.)

49ff: also z and g should be explained.

75: What is atmospheric output from a US center? Do they smoke there?

86ff: "output a few years from the start ..."?? After some guessing and checking back with native English speakers (I am not) I concluded the authors might want to say: "output from after 1992 which is a few years after the start of. . ."

88: I wonder how unstable kinetic energy looks like.

Figure 3 and elsewhere: It seems to me that both observations and model simulations produce data that can be compared; and then from these Rossby radii can be calculated and compared. But to compare "model with data" (352) leaves the reader to guess work.

101: What is a data result?

102ff: I wonder why the exactness is emphasized. Are other calculations in the paper not exact?

Why do the authors "allow scatter"? I guess scatter might be there or not regardless if they allow it or not. If this remark has any meaning it should be explained.

114ff: Why "would"? Maybe 0.1K would make little difference but since the inaccuracies are rather 1K, they probably do make a difference. To illustrate if this is the case it would help plotting the respective temp and sal contributions to density anomalies. Also I see the model surface mixed-layer being rather 200 m than 100 m too deep. And what is a fresh salinity?

125: Why does the title mention exact and approximate fields? What is an approximate model field anyway?

126: Skip “Winter and summer” or justify why these months should be representative (e.g. sea ice minimum is in September)

128: “increase” from winter to summer or vice versa, or increase with what?

129, Fig. 1 and tab 1 and Fig. 4: I suggest the authors make up their mind which geographical names they want to use in the paper and note them in a map in Fig. 1. Kara Sea is also a Siberian shelf sea.

135: “less pronounced” than what? Who is pronouncing the variation anyway and why?

140: I have no clue what the authors talk about: What is (or rather “would be” predicted here? Why do I need to read chapter 4 first before understanding predictions made in Chapter 3? What amplitudes are talked about here? What is a “transpolar increasing tendency”?

Fig. 3 ff: Axes lables are not readable.

I stop reading here, and I am curious to see a readable ms sometime.

Interactive comment on Ocean Sci. Discuss., 10, 1807, 2013.