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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 #2

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General comments: This paper estimates the first and second internal deformation radii in the Arctic Ocean (and adjacent seas) based on hydrography from a numerical model. The internal deformation radius is a very useful metric in ocean modelling, and presenting and discussing estimates of this for the Arctic should be of interest to many. Still, I find that the substance of this article is too thin for publication; it falls, in my opinion, below the 'smallest publishable unit'. It is also written in too casual a style and gives the impression of rushed work.

Specific comments: Certainly the title is misleading; with the article anywhere near to its current form a descriptive title should be "The internal Rossby radius of deformation in the Arctic Ocean".

It's fine to calculate scales from a well-tested and validated model. But why not also

calculate it from the climatology, particularly when comparing your model results with observations? Your synoptic observations are also fine, but we don′t know how representative they are (and did you also interpolate your model fields in time to the station data?). So personally, when I read something like "…the model estimate is too high…" I don′t know how concerned I should be (about the model fidelity) when you′re essentially comparing it to hydrographic 'snapshots' only. The gridded hydrography is coarse, yes, but this would then also give you something more to write about, namely the added information you get from the higher-resolution model.

Your plots of the phase speeds are, in my opinion, utterly useless. Since f varies so little up here the plots of c1 and R1 are practically the same. You also hardly discuss the c figures, so I suggest you drop them.

Also, I suggest you plot the annual mean R1 (and R2) and then summer-minus-winter values. As you state yourself, it's hard to spot differences between summer and winter. Difference plots highlight any such difference.

Your section 3.3 (Rossby radius and stratification) adds very little useful information to this article. So you put in average quantities (vertical hydrographic gradients) and get some numbers out that are close to those of the full calculations. The agreement is no surprise. I get the feeling you added this section to add some pages to the article. Please work harder at teaching us something new here, or consider dropping this entire section.

Also, your zonal mean calculation seems to be 'added fluff'. You yourself state that the zonal mean is a metric of questionable value up here.

The relationship between the deformation radius and observed (or modelled) eddy scales is of course of huge interest (Is there a substantial inverse energy cascade in the Arctic Ocean?), but here it is treated way too superficially. This could become a good paper—one reaching above a 'least publishable unit'—if the authors had put some serious work into this issue.

Minor and technical details: Introduction: I think very few would agree with your definitions of "eddy resolving" and "eddy permitting". At two grid points per def. radius your model explicit or implicit diffusion will kill growth at the def. scale. Please substantiate your claim (with references!) or modify. Also, check your sentence logic here: "…while the typical best resolution…is" (compared to what?).

Introduction: You discuss the def. radius of the Arctic before presenting your own results. This doesn't seem justified. Or are you quoting other people′s estimates? If so, please give references.

Methods and data: Equation (1) is linearized "around a state of rest". Also, there is a sign error in your equation. Equation (2) is only approximately correct since you have ignored higher S and T (or theta) terms in your (potential) density equation.

Methods and data: "…and the scale of maximum eddy growth becomes less clear…" At this stage you haven′t mentioned any detail about relationship between def. radius and eddy growth (presumably you′re referring to the predictions of the Eady problem). So your sentence here is hanging in mid-air.

Are your model plots all from 1992? Also the comparison with observations? Please clarify. Since you have various years to work with (after spin up), you could consider also plotting/discussing inter-annual variability of the def. radius.

Sec. 3.1: "While the model vertical salinity gradients are realistic…." It is of course precisely the vertical gradients that need be correct to get a good estimate of the def. radius. I think I see what you're trying to say here, but please try to rephrase.

Your last paragraph of sec. 3.1 doesn't hold water. You first write that your comparison with obs. has pointed to some systematic errors in the model. Then you write that earlier studies have shown the model to be good, "so the model patterns of the Rossby radius are realistic, and ….". This is too wishy-washy, hand-wavy, whatever you want to call it. Please tighten up this.

Sec 3.2: "The model also resolves the second mode Rossby radius Surely your 1/12 deg model doesn't resolve much of R2, but somehow I think this is not what you're trying to say here. Rephrase.

Please tell us WHY you are also showing us the WKB version of the def. radius. Because much analysis is based on this simplified estimate? Also, I would prefer if your plot of the differences showed R_wkb – R_full (i.e. the other way around), to better judge a sentence of the type "…WKB estimate is too high…".

Sec. 3.3 (which you should consider removing entirely): rho_ref = 1000 (add a _ref)

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