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Ocean Science Discussions

Interactive comment on "High resolution modelling of the North Icelandic Irminger Current(NIIC)" *by* K. Logemann and I. H. Harms

Anonymous Referee #1

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General comments

This is an interesting paper about the flow of Atlantic water into the Iceland Sea west of Iceland in the North Icelandic Irminger Current (NIIC). Although this is a minor branch of the main warm salty inflow across the Greenland/Scotland ridge from the Atlantic, it deserves attention and is important for juvenile codfish on the Icelandic shelf. The authors use a very high resolution numerical model to explore the (volume and heat) transport variability and pathways of the NIIC in a single numerical experiment which is presented in detail.

The main findings are to do with: the fidelity of the model (its NIIC is too weak compared to data), the origin of model NIIC variability at several periods (the meridional wind

north of Iceland is very important), and the origin of the NIIC (it consists of a roughly equal split between East Greenland Current water and water from south of Iceland). These are worthwhile contributions and the paper is nearly ready for publication. I have many minor technical comments and suggested corrections. I also have two main specific comments that are more important. They are to do with: understanding why the model NIIC is too weak compared to data and the consequences for the main findings of the paper (currently, there are only inconclusive speculations), and presenting an explicit comparison with the available observations, especially at the critical Hornbanki transect north of Iceland where there are quarterly hydrographic transects. I think that dealing with these two comments will require some significant (but not substantial) changes to the article. They should make the paper more complete and give the reader a better guide to the reliability of the model results.

Specific comments

1. Table 1 shows the model has 0.3 Sv Atlantic Water flowing north between Greenland and Iceland, but Hansen & Østerhus (2000) have 1 Sv (sections 3.1, 4, 5). Furthermore, the NIIC transport at section 7 (0.6 Sv; Table 2) is about half the value of Jónsson and Valdimarsson (1.1 Sv; 2005), because of excessive entrainment of PW (section 4). This is worrisome because the main point of the paper is to explore the pathways of Atlantic water inflow through the NIIC. I agree that these differences are probably within the formal error bars (2nd paragraph, section 3.1), but something more substantial should be said here. For example, a discussion in the context of other literature estimates is appropriate (in section 3.1). What can be said about the sensitivity of these model transport numbers to variations in model parameters? (for example, grid resolution, forcing frequency, eddy diffusivities, and relaxation timescale are all obvious choices here). How does the model transport variability compare to observations? In particular, is this diagnostic more or less accurate than the mean transports? (see also comments below).

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The discussion in Sections 4 and 5 is OK but inconclusive. The main problem is that the reader doesn't know which aspects of the results to trust and which to doubt. I realise that a comprehensive answer to this question will require more numerical experiments, but without some guidance here the paper is incomplete. This is the difficulty with presenting just a single numerical solution that disagrees with the data in an important way. The danger is that a skeptic may simply dismiss the model as unrealistic. On the other hand, a fully comprehensive answer is not really needed. Some carefully-justified argument that persuades the reader which results are more or less reliable would be sufficient. One good way to give a more complete picture is to add and discuss the Icelandic observations to the plots of model results wherever possible (see below).

2. What is the origin of the strong high-frequency variability seen in section 7 transports? (Figure 7, section 3.3.1). In particular, is the wind responsible as it is for the seasonal variations shown in 3.3.2?

3. Section 3.3.1: The 2nd paragraph on origins of heat flux variations through section 7 is obscure. The symbols in the 2 formulas need to be defined and "variability of the mean temperature" seems to be an oxymoron. I think I understand what the authors are trying to say here, but it needs to be carefully corrected to make sense to a novice.

4. The results on transport/stress correlation in Figure 8 (section 3.3.2) are very interesting and an important finding of this paper. If a few more details were provided it would be even stronger: For example, why is 67^o 40' N, 22^o 32' W chosen? Does this location maximise the correlation with transport? Also, is there good correlation with the zonal stress too? Naively, the meridional stress drives a zonal Ekman transport across section 7 such that northerly winds produce a westward flowing Ekman transport, in qualitative agreement with Fig. 9. The zonal stress produces an Ekman transport perpendicular to the zonal coast

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however (if the stress is taken near the end of section 7; it's hard to tell from the figures). This modulates the sea-surface slope at the coast, and, presumably, the NIIC transport through geostrophy. A correlation with zonal stress therefore also seems likely. But the dynamics may be more subtle than this simple picture and deserve more discussion. Finally, is there any evidence from the *in-situ* Hornbanki data of either the transport/stress correlation or the secular trend? Adding these data to Figure 9 and discussing the agreement would be very interesting indeed.

Some of these issues appear to be covered in the Discussion (section 4, Fig. 17), but this analysis is disconnected from that in section 3.3.2. Perhaps some rearrangement of the material here would help clarify the main messages?

5. Similarly, the results on the anomalous 2003/2004 (shouldn't it be 2002/2003?) winter in section 3.3.3 (Figure 8) are also very interesting. Again, a more explicit and detailed comparison with the Icelandic Marine Research Institute hydrographic data is called for. Plotting their data on Figs. 10 and 11 where possible would deepen the discussion and give a clearer impression of both the model strengths and possible explanations for the variability in the real system. In the Conclusions it is claimed that the high winter heat fluxes in 2002/2003 "are responsible for the observed temperature anomaly" but there is no evidence to justify this assertion. Finally, as the authors point out, the temperature variability beyond periods of 30days is damped by the model relaxation. The low-pass filtered temperature results in Figures 10 and 11 therefore probably underestimate the real variability. Can this effect be quantified somehow? Are there some model results available with a different damping timescale to compare to? Again, this will broaden and deepen the importance of the authors' results.

6. Section 3.4: The analysis of NIIC origin and pathways is innovative and revealing. I have a suggestion that might strengthen the discussion in an interesting way: What are the hydrographic properties of the NIIC source waters identified

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through the tracer diagnostics in Figs. 12-14 and, hence, how much interior mixing, air/sea buoyancy exchange, and T/S relaxation is involved in the formation of the NIIC? Focusing on the polar water components and the deep Faroe-Shetland components would be most interesting as the results on these waters are the most surprising.

Technical corrections

1. Abstract: Are the numbers in the 2nd paragraph averages?
2. Introduction: The overall goals of the paper are a little unclear from the short introduction. The first two paragraphs motivating the study are fine, but the 3rd paragraph is a little disconnected. Also, the connection to cod eggs is a little unclear; the discussion in Sections 4/5 is superficial compared to the physical analysis. I suggest that the paper's objectives are stated clearly in the Introduction and the discussion for cod egg dispersal is expanded a little (or cut).
3. Model description: Is the hydrostatic assumption reasonable at vertical/horizontal scales of 10/1000 m? I guess yes, but there might be important vertical accelerations at this resolution. Can you comment?
4. I don't understand the explanation of the gridding shown in Fig. 2 (2nd half of 2nd parag. of Section 2, and right hand panel in the figure). Also, where are the physical fields defined on this grid? Is this a Cartesian grid, or some other type? (the maps look curvilinear, but then what is x?).
5. There is a sign error in (1) and in the inline equation that follows it: the lower limit of the integral should be $-z$ if z is depth as stated (positive down).
6. Model Description: I know the "ECMWF" and "NCEP/NCAR" acronyms, but not "PHC". What does it mean? Also what is "depth-independently"? Finally, what conditions are specified at the open boundaries?

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7. Model Results: It appears from the 3rd paragraph that “Atlantic Water” (AW) means water with salinity greater than 35.00. This is a reasonable definition, but it should be stated more clearly earlier in the article (e.g. Introduction), for clarity. Similarly, please explicitly define polar water, “PW”.

8. Section 3.2: “despite of this recover”; “last row in Table 2” should be the last column.

9. Section 3.3: “high frequent”; “for at these sections”; the 2nd sentence of 3.3.1 does not make sense to me.

10. Section 3.3.2: “in phase ... with a time lag of 4 months” is self-contradictory. Reword.

11. Section 3.4: “flows in opposite direction”; does “climatological flow fields (1997-2003)” mean the 1997-2003 *average*, or is it seasonally varying? The integral formula needs to be explained a bit better: what are $3a$ and D? Also, this quantity is not what is conventionally called a “flux”.

12. Section 4: “breakdown and recover”; “it even recover slightly”; “which would be consistent” is confusing; “rather comprehensive view” is unclear; “enforce the EGC” is unclear.

13. Section 4: “suggests a kind of hydraulic control” is vague and superficial. Please be more explicit and explain this claim in detail.

14. Section 4, 3rd paragraph from end: I don’t understand the sentences starting “This rather good...” and “Obviously the NIIC...”. Reword to explain these ideas better?

15. Section 5: “resolve sufficiently good”.

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16. Table 1: Across which sections are these transports defined? Are they net volume fluxes?

17. Fig. 3 caption: “homogenous”

18. Figure 5 (and similar maps): What is the grayscale shading?

19. Figure 9: Mark the wind stress location on Figure 6? “3.2 10^{-6}”.

20. Figure 15 caption: “consist of”

21. Figure 17: Shouldn’t the ordinate label be *correlation density*, not “correlation”?

Interactive comment on Ocean Sci. Discuss., 3, 1149, 2006.