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Ocean Science Discussions

Interactive comment on "Arctic Ocean circulation and variability – advection and external forcing encounter constraints and local processes" *by* B. Rudels

Anonymous Referee #3

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This is quite a unique article if viewed either as a review or a research article. I've thoroughly admired and enjoyed it and recommend its acceptance for publication.

The article is a rather specific, focussed review largely following the author's own substantial contributions to Arctic Ocean understanding. The article is best summarized by the author himself (pg 2344): "history of the exploration and study of the Arctic Ocean, and some basic themes in Arctic Ocean oceanography have been examined, following ideas and using methods introduced by oceanographers in the early 20th century".

Making insightful use of descriptive hydrography (largely T-S plots), the author illustrates from his previous publications some of the kinds of calculations that would have

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felt familiar to pioneers in the early 20th century. This is not a comprehensive review of up-to-date understanding, much of which has been following numerical modeling, including attention to wind-forced as well as thermohaline circulation. The author is entirely forthright recognizing the limited and speculative nature of his own inferences.

I feel this is important. The author's way, indeed a classical way, of drawing insights and making simplified calculations can be overlooked by a community increasingly attuned to numerical modeling. Publication of this article should provide a convenient statement of a classical approach. I wonder if there is an opportunity in the modern world of modeling to apply the author's methods to model output, testing validity of classical assumptions and possibly extending those methods (all with caution about veracity of model outputs).

What follow are a number of minor comments.

p 2320. I would limit a remark abut Arctic freshwater (FW) forcing to comparison of ocean area to area of drainage basin, omitting mention of the relative size of coastline. Coastline size compared to what?

p. 2320. The reader deserves clear caution that FW transported by idealized rotationally controlled buoyant bndry current can be quite misleading when substantial barotropic flow also is present (as seems often the case).

p. 2327. The author does a very good job arguing how to constrain the fraction of heat loss from Atlantic layer (AL) that goes to melting ice and the fraction transferred to the atmosphere and to space. This is an important antidote when one sees remarks about heat from AL sufficient to melt the entire sea ice cover several times over.

Use of T-S diagrams is great. However, when T-S traces are identified by symbols keyed to locations of profiles, the locator maps should be improved. Especially when comparing traces from a limited region, viz., Fig 7 or Fig 13, e.g.,please show locator symbols on a blow up of the relevant region rather than a pan-arctic map. Note that Fig

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18 is good in this regards. Please reconsider all figures.

At Fig. 10 I miss a locator map.

At Fig. 11 let me please express personal skepticism about depicting a distinct westward flow along the southern flank of the Amundsen Basin. I believe the westward flow along the Lomonosov and along the northern portion of the Nansen Basin are valid, and that shallow flows may be generally westward across the interiors of Amundsen and Nansen basins. But I am doubtful about a distinct core of westward flow (at AL and intermediate depths) along the southern Amundsen. At greater depth I would speculate about an eastward-flowing core along the southern Amundsen.

p. 2335-36. Discussion about when, or if, and how, FSB water crosses over the Lomonosov seems excellent, as well as frankly speculative. This may be a crucial subject with Arctic-wide implications.

p. 2338. I'm confused about how dyn height at 2000m provides a reference for pressure gradients at 2000m. How is this dyn height obtained or interpreted?

p. 2339-43. Discussion of two-ways transports at Fram Strait are certanly thought-provoking. But I feel troubled and doubtful when we impose a rule like no geostrophic transport up a pressure gradient (seemingly true by definition?) yet we go about estimating geostrophic transports down pressure gradients.

Significantly, the author argues that the two-ways FS transports cannot be explained from water mass / baroclinic forcing hence must depend upon wind-forced barotropic circulation. Here the author overlooks another forcing that was not part of early 20th century (nor even most of late 20th century) thinking; that is, ubiquitous eddies will spontaneously organize a mean Arctic circulation very much like the author's Fig. 11 (except in the southern Amundsen). In FS one may estimate (albeit crudely) eddy-forced of two-ways flows near fLˆ2H with L a characteristic eddy scale of a few to several km and H the depth of FS, this yielding at least a few to several Sv – quite

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apart from mean wind- or baroclinically- forced flow. N.b., this is not about eddies transporting / dispersing properies; it is eddy-forcing of mean flow. However, in the context of the present paper based upon classical ideas, eddy-forcing on mean flow may be considered post-classical hence outside the author's present scope.

My feeling is the present paper, within minor mods, does an excellent job of gathering and displaying a kind of classical understanding. It is important this be set out.

Interactive comment on Ocean Sci. Discuss., 8, 2313, 2011.