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Interactive comment on "Ocean properties, ice–ocean interactions, and calving front morphology at two major west Greenland glaciers" *by* N. Chauché et al.

Anonymous Referee #3

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The manuscript presents new hydrographic and turbidity data obtained during short surveys from two consecutive years in nearby fjords. The data sets are interesting, but their interpretation appears somewhat speculative (or problematic). The authors would do well by grounding their interpretation into existing work, or provide specific evidence where their data provides alternative processes at play.

A number of major issues:

1. Abbreviations and water masses: While this is not a major scientific problem, the manuscript is somewhat hard to read, given the flood of abbreviations, many of which are uncommon in the literature or non-intuitive. At a minimum, a table that summarizes

abbreviations is warranted. You might also consider renaming, for example, simply refer to subglacial discharge (instead of GRW; and you seem to be using "runoff water - RW" as well, e.g. Table 3), Atlantic(-origin) Water (AW, instead of SPMW), and Polar(-origin) Water (PW, instead of BEW). This would also simplify placement within existing literature. Unless you have a strong case for the choice of SPNW (a more appropriate reference of which would be McCartney and Talley, 1892, and with a narrow formation process through winter deep convection)? I am also somewhat doubtful with regard to BEW. I've seen it used, e.g. in highly specialized context of dense water in shallow Arctic continental shelves, e.g. Schauer, 1995, where it is termed "brine-enriched shelf water (BSW)". In the context of this manuscript it is not clear to me whether you imply that your "BEW" is mainly locally generated (sea ice production within the fjord), or remotely advected. If not remotely advected, what about polar-origin surface waters from the West Greenland current? Are they not found in the fjord?

2. "Interannual variability". For example, p.87, l.8-13: It seems highly problematic to discuss interannual variability from two snapshot measurements taken one year apart. I acknowledge that this is all the data you have, but at a minimum be careful how you interpret them. Or p.90, l.10,11: Do I have to think of this as a depth level maintained throughout the summer? If so, is the corresponding momentum or buoyancy source that steady? It is somewhat hard to believe. Again, I am wondering whether you confound snapshot and mean state.

p.82, l.25: I am somewhat doubtful regarding the "BER", see comment #1. At a minimum, a reference here would be in order (BER is not a common water mass label), and some explanation regarding the origin of such waters seems warranted. I would prefer the labeling of Polar(-origin) Waters.

p.90, section 4.2.3: In addition of doubting the BER story, I don't understand what is meant by "insulating effect". Insulating what from what?

p.91, sections 5.1 and 5.2: The main problems with the manuscript are in my view with some of the interpretations. Many of the modal attributions are in my view artificial. In particular, there is a smooth continuum between “Mode 2” and “Mode 3” with the only distinction that “Mode 3” waters happen to achieve neutral buoyancy at depth levels where you occasionally find BEW. Similarly, I would expect a smooth transition between “Mode 1” and “Mode 4”. I fail to be able to distinguish between the two in your Figures 4 and 5. p.92, l.19ff: As already alluded to above, I fail to follow your description of an isolating effect of the “BER”. At a minimum, the data at hand are too far removed from the ice front to corroborate this view. I don’t see how this would work dynamically, and your schematic, Fig. 7 flatly contradicts this view. p.92, 1st paragraph: The entire paragraph contains rather hand-waving arguments. You refer vaguely to “dilution” or “vigorous mixing”, where various previous studies have tried to provide quantitative estimates of entrainment rates based on plume theory. It would help to frame your discussion within those quantitative concepts, or alternatively, demonstrate how and where they fail. Your framing in terms of kinetic and potential energy is interesting, but again completely hand-waving. Please provide some rough quantitative estimates to support your claims. It’s also somewhat confusing. Not clear to me what “potential thermal energy” is and how it can be invoked to infer melt rates.

p. 93, section 5.2, 1st paragraph: The review of the existing work is rather simplistic. Indeed, a homogeneous discharge along the ice front is “assumed”, but it has been clearly acknowledged (e.g., Jenkins 2011; Salcedo-Castro et al. 2011; Xu et al. 2012; Sciascia et al. 2013), that in the absence of knowledge of the subglacial discharge spatial distribution it is a reasonable, or the simplest initial assumption to make. The implied model simplifications are also deemed reasonable to provide rapid 2D baseline estimates. All papers acknowledge the need to better understand - and simulate - the spatial discharge source distribution.

Minor comments:

p.80, l.9: “2.8degC SPMW”: that seems extremely accurate. Uncertainty bounds of some kind seem warranted. Are you claiming that the buoyant melt water plume (e.g.,

Jenkins 2011), somehow gets shielded from the ice-ocean interface by the BER water masses? If so, what dynamics would be at play? Your schematic, Fig. 7 would contradict that view. Or maybe I misunderstand?

p.81, l.7: “The subpolar gyre . . . water around the coast of Greenland driving”. Certainly not around Greenland as a whole. Please be more specific.

p.82, l.3, and p.91, l.9-12: “modes” sounds elegant, but why call them “modes”? Is this some sort of modal decomposition? Is it evident that these is a clear/clean separation? It seems somewhat artificial to me. In particular the distinction at the beginning of section 5.1 between (1) and (4) seems rather arbitrary. Isn’t this rather a continuum (as a function of increased discharge)? Also, the modes “(2)” and “(3)” seems somewhat arbitrary too. Following e.g. Sciascia et al. (2013), the more general level of outflow is more tightly connected to the level of neutral stability of the plume, irrespective of presence/absence of “BER”.

p.82, l.6: See above, appropriate reference to SPNW (if you prefer to keep it) goes back to at least McCartney & Talley (1982). I am not sure whether most of the references given are appropriate in this context. In turn, referencing Jenkins (2011) earlier in the sentence (in the context of modeling) seems very appropriate (also in other places in the manuscript).

p.83, l.14: What are the calving rate uncertainties based on? Seasonal variability? Uncertainties in inferences from time lapse (or otherwise)?

p.86, l.2,3: Sorry, I don’t understand what you do (sentence: “The spatial distribution of temperature. . .”).

p. 86, l.12: Can you briefly comment on the sources and magnitudes of uncertainties associated with those discharge estimates. It seems to me that they are much more uncertain than your Table 3 would seem to suggest (e.g., digits of accuracy).

p.97: Please check following references for correctness/completeness: * Christoffersen et al * Fofonoff et al * Straneo et al 1011 * Weidick * Xu et al

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Interactive comment on The Cryosphere Discuss., 7, 5579, 2013.