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Interactive comment on "Water mass structure and the effect of subglacial discharge in Bowdoin Fjord, northwestern Greenland" *by* Yoshihiko Ohashi et al.

Anonymous Referee #2

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General Comments:

The manuscript by Ohashi et al. 2019 presents observations from Bowdoin Fjord in northwest Greenland from 2014 and 2016 and supports these observations with interpretations from a 3D nonhydrostatic model. The study is mostly a straightforward report of the measurements between these two years in terms of the stratification and turbidity, which seems to be competent. The observational portion of the work is clear with a topic and discussion that is appropriate for this journal. A scientifically-interesting part is the comparison between the two summers, from which the authors draw conclusions about the influence of variations in the subglacial discharge and stratification. However,

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the validity of such a comparison must be addressed with greater care (a few suggestions for this are provided in the specific comments). For instance, might we observe similar differences in turbidity if we simply made the measurements in two different weeks within the same melt season (due to intrinsic variability in the discharge rate)? This also goes hand-in-hand with how the short 5-day integration times in the numerical experiments may be enough to stabilize plumes, but not necessarily enough to set up a steady-state fjord stratification (discussed in specific comments). In addition to integration time, there are also additional issues/caveats that must be addressed with respect to the model configuration (resolution and boundary conditions) use to interpret the observational results. The connection between the model and the observations is also rather tenuous - a direct comparison is not well-established, leaving the authors to draw inferences from a few model experiments to help explain the differences they observe between 2014 and 2016. It is unclear whether the model experiments actually yield any new understanding (of the roles of discharge and stratification) not available from previous studies. If not, it's not clear why the model is needed at all; if so, then why aren't these findings reported as part of the manuscript's results section? Finally, in general, the manuscript is not particularly well written (some technical comments are provided but is not exhaustive) - it could benefit from editing by a native English speaker to improve clarity.

Specific Comments:

Page 1, Line 16: What is the significance of the 60-80 m depth range? What does "temperature profiles were distinctively different" mean specifically? Be more specific about what "a larger fraction" means. In general, this result seems contradictory/unclear: there is more discharge in 2016, but higher discharge fraction at 15-40m in 2014, yet there is also higher turbidity near the surface in 2016. The authors attribute all this to different stratification/discharge flux combinations but it's not at all clear why this is the case from the abstract.

Page 1, Line 29-32 and other lines which use similar citations: This is a subtle point in the framing of the paper in an atmospheric/ice sheet perspective vs. a growing body of literature on the ocean-driven variability of the ice sheet. While these lines provide an accurate summary of some recent literature on Greenland mass budgets (but also see King et al., 2018: https://doi.org/10.5194/tc-12-3813-2018, Mankoff et al., 2019: https://doi.org/10.5194/essd-11-769-2019), perhaps rewording or additional clarification is necessary to leave the reader with the right impression of the role of ocean in this process. The mass budgets cited use a “flux gate” further up-glacier where surface speed and ice thickness is used to estimate solid ice passing through this gate which is then assumed to be eventually calved into the ocean. This however does not account for surface thinning downstream or submarine melt at the terminus. A short discussion on the ocean’s role in undercutting glaciers (through submarine melt) is presented in Rignot et al., 2015: https://doi.org/10.1002/2015GL064236 and Straneo et al., 2015: https://doi.org/10.1146/annurev-marine-010213-135133. This perspective is more relevant for this study than many of the atmosphere/ice references that are presently cited and a discussion focusing on the ocean’s role would help set the scene for this study.

Page 1, Line 37-41: While this is an accurate account of recent observations on exiting plume water masses, it should be noted that plume meltwater fractions (cited as 7-10 percent) depends strongly on the discharge strength and depth of the plume source (which together prescribe the degree of entrainment and neutral buoyancy of the exiting plume water mass). Bendtsen and Mankoff’s measurements both focus on fjords that are shallow and have a plume undergoing weak overall entrainment i.e. they exit at the surface instead of at mid-depth. In general, the meltwater fractions should be much lower for deeper plume sources or plumes that undergo greater entrainment. Here, you are focusing on a shallow discharge plume that rises to the surface, but it is important to point out that many plumes do not fall into this category and why, as well as why shallow plumes have such a high plume meltwater fraction (see Straneo et al. 2015 for a relevant discussion on plumes: https://doi.org/10.1146/annurev-marine-010213-135133).

Page 2, Line 8: “The subglacial discharge distribution into a fjord” is better described as “vertical distribution of outflowing plume water” since the subglacial discharge only exists as the terminus depth and is not “distributed,” but rather the plume outflow can be vertically distributed. There are a few other cases throughout the paper where this distinction could be made clearer. Also, note that the plume outflow distribution cited is more relevant for plumes that reach neutral buoyancy at mid-depths (instead of a concentrated outflow at the surface, as in this study). Consider rewording other instances of this including Page 3, Line 8, and others.

Page 2, Line 11: What is the “realistic influence?” In what way were previous evaluations of its influence not realistic?

Page 5, Line 9: Considering renaming the title to Section 4 as “Observational Results” or similar, since it can be confusing that Sect. 3.3 discusses the numerical experiments, which is not discussed again until Sect. 5.2. An alternative, which may be preferable is restructuring the sections so that all the observational discussion precedes the numerical simulations e.g. Sect. 3: Observational Data and Methods, Sect. 4: Observational Results, Sect. 5: Numerical Experiments.

Page 4, Sect. 3.3 general comments: In general, the numerical experiments would benefit from higher resolution (which would also improve the quality of the numerical results and many of the figures including Fig. 11, B1, and B2) as well as caveats/justifications for certain choices such as the seafloor no-slip conditions and the integration time.

(a) Resolution Issue: There is a strong concern that the results here are strongly dependent on the horizontal and vertical resolution. For instance, the plume (assuming a point plume with entrainment coefficient of alpha = .1, although a similar case can be made for a line plume of finite width) radius at the surface for such cases would be approx. 20m, which is already subgridscale. Many current numerical studies including plumes implement some version of a subgridscale plume parameterization (Xu et al.

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2013, Cowton et al. 2015) unless they are extremely high resolution (< 1 m horizontal). Also, not discussed here is why the plume source (choice of outflow dimensions) is chosen to be 200 m wide and 50 m tall. Is there an observational justification for these dimensions or was it chosen such that the source was multiple grid points high and wide? A source that is 50 m tall seems especially large given that the depth of the plume is 210m and much of the change in density of the plume occurs near the source (i.e. within the first 50m). Therefore, it is strongly recommended to run these runs at higher resolution (and to reduce the height of the plume source) to observe how much the results would vary, or demonstrate the convergence of the relevant metrics with respect to resolution.

(b) No-slip conditions: Since the access of the AW is controlled by a small layer (only a few gridpoints tall at 1-2km from the icefront), this choice in bottom boundary condition is likely to dampen the ability of AW to fuel the entrainment from the plume. The choice of no-slip boundary conditions would only be justified if the viscous sublayer were resolved. Consider quadratic drag (which is more defensible) bottom layer or demonstrate insensitivity to/justify the choice of no-slip conditions. What is prescribed at horizontal boundaries? Perhaps an increase in vertical resolution in (a) would help as well.

(c) Integration time: Please consider justifying the integration time of five days (or stating the implications or caveats associated with such a short integration time) or running the numerical simulations for longer. Although the plume rise time is much less than five days, the residence time within the fjord (timescale associated with volume of the fjord divided by rate of overturning circulation) is likely much longer and the response of the background stratification to the water mass transformation due to plume entrainment should be on the timescale of months (see Carroll et al., 2015).

Page 7, Line 42: A caveat which may be worth noting is that turbidity at the fjord surface may only be a reasonable proxy if the plume is able to reach the surface, which depends on the degree of entrainment.

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Page 8 (throughout): Consider an additional run ST16Q600, which uses the stratification observed in 2016, with a discharge that is 20 percent higher than the CTRL run, since you state the PDD is 20 percent higher in 2016 compared to 2014, so a more realistic representation of the 2016 state would take into account the increase in discharge as well.

Page 8 (sect. 5.2) : Why are the model results presented in the paper's discussion section - aren't these also results?

Page 22 (also, see other relevant figures and discussions): Each of these measurements represents a snapshot of the turbidity, temp, salinity etc., but are these snapshots representative of the entire 2014 and 2016 melt seasons? How much variability in the discharge rate, turbidity etc. would we expect within a single melt season? I'm not sure whether it's possible to even distinguish differences between the 2014 and 2016 melt seasons from a single sample from each season. If not, then attempting to explain them using the model is not a valid approach.

Technical Corrections:

Page 1, Line 1 and many others. When "structures" is used to denote the properties of vertical water mass profiles i.e. "water structures", this should instead be "vertical density profiles" or equivalent for clarity. For instance, consider changing the title to: "The effect of subglacial discharge on vertical density profiles in Bowdoin Fjord, northwestern Greenland."

Page 1, Line 23: Consider rewording the last sentence of the abstract for clarity e.g.: "Fjord stratification is an important factor controlling the vertical distribution of freshwater outflow due to subglacial discharge strength and entrainment. The fjord stratification does not influence the magnitude of subglacial discharge 'amount', as is implied in the original statement.

Page 2, Line 34: "...of a proglacial fjord." Page 2, Line 42: Correct to present tense:

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“This study focuses on BF . . ..”

Page 3, Line 1: Correct to: “In June and July, the sea ice melts rapidly exposing the open ocean surface.”

Page 3, Line 6. If the depth of PW/AW interface is between 50m and 150m, then 210m should always be below the AW and PW interface. In Fig. 12, the schematic shows a PW/AW interface that is between 175m and 225m.

Page 3, Line 24: “0.01” psu? Page 3, Line 36: “. . .which subsequently spreads due to entrainment.”

Page 4, Line 9 and others: Consider another word instead of “endmember” which is unclear when it is used. . . perhaps “source?”

Page 5, Line 27 and others: Consider a more compact notation $\Theta_{\text{max}}^{2014}$, etc.

Page 9, Line 29: As a point of clarification, the subglacial discharge that exits near the surface as in this study would lighten the surface layers and increase stratification, but if it preferentially lightens intermediate/deeper layers, it would act to decrease stratification as is observed in other studies (see Jackson et al., 2017: https://doi.org/10.1002/2017GL073602).

Page 9, Line 43: “In turn, a prominent local . . . was detected around 60 m in 2016, which was significantly warmer than the local maximum in 2014.”

Page 16, Line 4: Correct to: “. . . (blue in 2014, red in 2016, both inside Bowdoin Fjord, and green in 2016, outside Bowdoin Fjord).”

Fig.12 and to a lesser extent, Fig. 13: Consider esthetic improvements that would improve this figure including fewers arrows, clearer color/font contrast, labels, etc. Is the PW layer here meant to only represent the PW core? It may be clearer to differentiate between Θ_{min} and the full PW layer. It is not clear exactly which portion of the vertical column has a stronger vs. weaker stratification.

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