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Interactive comment on "Internal hydraulic control in the Little Belt, Denmark. Observations of flow configurations and water mass formation." *by* Morten Holtegaard Nielsen et al.

Anonymous Referee #1

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General comments:

This is a presentation of an interesting data set in Little Belt that shows a transition from internally subcritical to supercritical flow as the water enters a narrow and deep part of the strait. Further downstream, the water is much less stratified, indicative of large mixing taking place in the supercritical flow, and fluerescence data also indicate enhanced primary productivity due to mixing of nutrients into the euphotic zone. The data clearly shows situations where the lower layer accelerates down the slope and becomes the active layer. There are also indications of a situation where the upper layer accelerates, thins, and becomes active due to the decreasing width of the channel. I think this is an interesting work that deserves publication. The presentation is

brief, well structured, and mostly clear. However, I have some issues which I would like adressed before publication:

Specific comments:

1. The discussion is rather qualitative, except for the estimate of Froude numbers. Particularly when it comes to mixing, I would like a more quantitative discussion of the claimed water transformations, and the reasonability of those. One way to help the reader would be to show the TS diagrams both upstream and downstream of the transformation region. Another would be to estimate the volume fluxes of incoming and transformed waters. For example, in Figure 2, the downstream water is of nearly the same density as the upper layer upstream. Is it reasonable that the upstream volume fluxes combine into this light water mass, or has the dense water simply not reached the downstream section yet? The same is the case in Fig. 4, and there I would like some further arguments that the sharp front is a stationary control rather than a propagating bore. An energy argumentation would also help. Is the loss of kinetic energy sufficient to explain the claimed rise in potential energy due to mixing? I think it is worth putting some effort into this, since this is one of the most extreme cases I have seen regarding mixing and removal of stratification downstream of a jump. That should also be one of the main key points of the manuscript.

2. I miss a discussion about the importance of the curvature of the strait. The strait is dramatically meandering, and transects upstream of the most curving sections show strong interface tilting. How much can the interface be expected to tilt in the curving parts, and how does that influence the along-strait transects? May it also influence the hydraulics, the mixing, and the friction in the strait?

3. In the claimed scarcity of published observations of controlled flows the authors miss a number of high-quality observations over fjord sills that show the controlls and associated mixing in such flow configurations in much more detail than what is presented here, e.g. Farmer and Armi (1999), Klymak and Gregg (2004), Inall et al. (2004),

Staalstrom et al. (2015).

Detailed comments:

Line 22: “that” should be “than”.

Paragraph starting at line 48: Include references to high resolution fjord sill studies.

Lines 91-93: How much larger is the mixing efficiency in supercritical flow? I find no such conclusion in the referred paper. I would also like some references where the stratification breaks down totally after a control. Most of those I know about maintain a stratification, although modified, downstream of the control.

Line 113: “reducing” should be “changing” since reducing is only applicable in one end.

Line 126: “refraction” is the change of wave propagation direction due to change in propagation velocity, which is not what happens here.

Line 142: “vertical profile” is misleading since the probe does not move vertically.

Line 168: “less and somewhat linearly stratified”. Unclear.

Figures 2 and 4: Show TS data both upstream and downstream. Also indicate in the transect where the TS diagrams have been taken.

Figure 3: Please indicate the timing of the various transects.

Lines 208-210: How short are the length scales and how short should they be for the flow to be quasi-steady?

Lines 221-222: Please add a quantitative discussion about the reasonability of this. Is it sure that the front is not moving?

Lines 229-230: What happens downstream when the curvature is much more important?

Lines 261-263: Strictly speaking what is shown is that the flow goes from subcritical to

supercritical. You do not show where the critical cross-section is, and that it is steady in time. I.e. you do not show that the flow is controlled.

Lines 292-295: Again, argue more quantitatively. The evidence of a control is quite weak.

Lines 314-320: How does the increased primary production in the upper layer fit with the earlier conclusion that entrainment is mainly into the bottom layer?

Lines 326-327: Again this is somewhat confusing. Entrainment is mainly into the upper layer, but downstream the upper layer is at the bottom. Some more explanation needed to make the text clear.

Line 340: In estuarine flows the exchange is often determined by internal hydraulics rather than sea level differences.

Lines 371-372: Again a quantitative discussion is needed. How quickly is Little Belt flushed out?

Lines 381-383: How can information about the critical point be used to quantify mixing?

Line 412: Since such phenomena have not been observed, one could include “the possibility” to avoid stating that they exist in reality without evidence.

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