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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.

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COMMENTS FROM REFEREE: 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 chan- nel. 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.

AUTHORS' RESPONSE: We agree with the referee that the discussion - and the manuscript in general - is rather qualitative. Given the nature and the extent of the data material we are not able to provide a much more quantitative analysis without becoming too speculative. At this stage in our research we have focused on showing the presence of internal hydraulic control in the Little Belt and documenting the conditions under which it occurs and the influence that it has on the lowest trophic levels of the ecosystem. However, the referee has made several suggestions as to how to make the manuscript more quantitative and informative, most of which we are able to follow. What we are unable to pursue is the energy argumentation, including the change of kinetic energy and the increase in potential energy due to mixing. A direct assessment of this would require concurrent, along-strait observations of flow velocity, which we don't have. The referee has a very good point though, which is indeed worth putting effort into, we agree. When we have better, more detailed observations available we are going to pursue this idea.

AUTHORS' CHANGES IN MANUSCRIPT: We will revise Figs. 2 and 4 to show in detail how the mixing is taking place. This will be done by adding three TS diagrams that show the water masses in different locations (upstream, downstream and somewhere in between) along the two transects. This will allow us to determine the mixing ratios and to speculate on the mixing rates. We will revise the text accordingly. In addition, we will use the TS diagrams shown in Figs. 7 and 8 to discuss and compare the mixing ratios found in the two contrasting situations in which the upper or the lower layer become the active, accelerating one. In case of Fig. 2 this will also allow us to answer the referee's question concerning the fate of the mixed water masses on the downstream side.

COMMENT FROM REFEREE: 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? AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: The referee has a good point here. Curvature and secondary flows certainly play a big role in the narrow part of the Little Belt, but are of much lesser importance for the internal hydraulic control as such, which occur some distance upstream of the narrow part. We will revise the text to include a discussion and references on the possible effects of curvature and secondary flows.

COMMENT FROM REFEREE: 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 asso-ciated 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). AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: We cannot explain why we have missed these references. We will carefully assess the information contained in these papers and will revise the text accordingly.

COMMENT FROM REFEREE: Detailed comments: Line 22: "that" should be "than". AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: Yes. We will fix it.

COMMENT FROM REFEREE: Paragraph starting at line 48: Include references to high resolution fjord sill studies. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: Yes, cf. our reply above.

COMMENT FROM REFEREE: 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. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: Prastowo et al. (2009) report mixing efficiencies of around 0.11, which they compare with values of up to 0.20 reported in the literature. We don't find it necessary to revise the text, but will consider doing so to help the reader. We have no knowledge of observations of similar situations in which stratification breaks down completely. We will consider adding this information also.

COMMENT FROM REFEREE: Line 113: "reducing" should be "changing" since reducing is only applicable in one end. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: We don't understand what the referee means, but will try to word differently.

COMMENT FROM REFEREE: Line 126: "refraction" is the change of wave propagation direction due to change in propagation velocity, which is not what happens here. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: We agree. We will find a correct way of expressing what's taking place.

COMMENT FROM REFEREE: Line 142: “vertical profile” is misleading since the probe does not move vertically. AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: We agree and will modify accordingly.

COMMENT FROM REFEREE: Line 168: “less and somewhat linearly stratified”. Unclear. AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: Yes. We will be more specific.

COMMENT FROM REFEREE: Figures 2 and 4: Show TS data both upstream and downstream. Also indicate in the transect where the TS diagrams have been taken. AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: This follows from the referee’s general comments. As we have explained above, we will modify Figs. 2 and 4.

COMMENT FROM REFEREE: Figure 3: Please indicate the timing of the various transects. AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: We will add this information to Fig. 3.

COMMENT FROM REFEREE: Lines 208-210: How short are the length scales and how short should they be for the flow to be quasi-steady? AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: The length scale to be considered is the distance over which the width changes, i.e., about 2 km. For the flow to be quasi-steady this should be well below with the distance travelled by a baroclinic wave (at a speed of about 1 m s-1) during a period equal to the time scale (about 12 hours), i.e. more than 10 km. We will try to clarify this in the text.

COMMENT FROM REFEREE: Lines 221-222: Please add a quantitative discussion about the reasonability of this. Is it sure that the front is not moving? AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: A revision of Figure 4 should be able to help clarify this. In principal we cannot be sure that the front is not moving because we did not observe the front specifically. However, we did observe the same transect about 6 hours earlier, reported in Lund-Hansen et al. (2008), and found roughly the

same conditions. We also note that the salinity and temperature time series from the narrow part show that the stratification is often broken down, showing that the front isn’t simply advected through the Little Belt. We will try to modify the text to clarify further.

COMMENT FROM REFEREE: Lines 229-230: What happens downstream when the curvature is much more impor-tant? AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: This we will try to explain with reference to observations from Øresund, another Danish strait, reported in Nielsen (2001).

COMMENT FROM REFEREE: 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. AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: We believe that we have shown that the flow can be considered quasi-steady and so that the point of control must be located somewhere between the two stations. Under different flow conditions the point of control could be located elsewhere though. We will put more emphasis on discussing the variability of the flow conditions and the effects thereof with respect to hydraulic control.

COMMENT FROM REFEREE: Lines 292-295: Again, argue more quantitatively. The evidence of a control is quite weak. AUTHORS’ RESPONSE AND CHANGES IN MANUSCRIPT: We admit that the argumentation could be improved here. We will argue more quantitatively using the mixing ratios, as described above, and we will reverse the order of the arguments roughly as follows. The flow speed observed in the narrow part (around or above 1 m s-1) along with the weakened stratification shows that the flow is supercritical here. Since the conditions on the upstream side are certainly subcritical, a point of control must exist and be located somewhere in the contracting part to the north. The structure of the water column observed on the upstream side shows that the upper layer is the active, accelerating one, which is entraining water from below. This is supported by the water masses observed in the narrow part of the Little Belt.

COMMENT FROM REFEREE: 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? AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: When the intensely mixed water masses are advected away from the narrow part of the Little Belt, they are brought upward in the water column and into the photic zone and are spreading sideways. This is simply due to the shallow depth on the downstream side. Fig. 2 should be able to show this. We will modify the text to clarify this.

COMMENT FROM REFEREE: 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. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: The density of the mixed water masses and the stratification found on the downstream side determine where in the water column the mixed water masses end up. Even if this is relatively deep in the water column, increased phytoplankton concentration may be the result still. We shall explain better.

COMMENT FROM REFEREE: Line 340: In estuarine flows the exchange is often determined by internal hydraulics rather than sea level differences. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: Yes, but a great deal of estuarine examples exist in which there is a substantial barotropic pressure gradient that may drive both layers in the same direction. We will try to be more specific about the situations that we are considering.

COMMENT FROM REFEREE: Lines 371-372: Again a quantitative discussion is needed. How quickly is Little Belt flushed out? AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: Yes, this piece of information is needed. We will add it both here and in the section on the Physical Setting.

COMMENT FROM REFEREE: Lines 381-383: How can information about the critical point be used to quantify mixing? AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: Given a 'weir formula', cf. Pratt (2004), the production of kinetic en-

ergy and a mixing efficiency it should ideally be possibly to quantify mixing. We will revise the text to explain this.

COMMENT FROM REFEREE: Line 412: Since such phenomena have not been observed, one could include "the possibility" to avoid stating that they exist in reality without evidence. AUTHORS' RESPONSE AND CHANGES IN MANUSCRIPT: That is true. We will follow the referee's recommendation.

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