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Ocean Science Discussions

Interactive comment on "The role of turbulence and internal waves in the structure and evolution of a near-field river plume" *by* Rebecca A. McPherson et al.

Anonymous Referee #2

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This manuscript presents the results of a momentum balance analyses in the near field region of an energetic fresh water plume entering into a fjord in New Zealand. Components of the momentum balance are derived directly from upward transiting microstructure profiles, and a modified version of the MacDonald and Geyer control volume analysis. Failure of the momentum balance to close, particularly within the interface region (2-4 m depth), is used as an opportunity to explore other mechanisms of energy dissipation, including forcing of internal waves, and energy dissipated through a hydraulic jump. These two missing mechanisms are identified as the missing elements in the mismatched momentum budget.

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Discussion paper

The authors present an interesting take on near field dynamics, and their suggested mechanisms for closing the momentum budget appear plausible. However, I have several concerns related to the data set, which lead me to some skepticism. The microstructure data set is discussed in more detail in McPherson et al (2019), particularly in Appendix A, where concerns about tilt angle and rate of change of the tilt angle of profile are disregarded. Given the significant ramifications of these extremely high dissipation measurements, both in this paper, and in McPherson et al (2019), I do not believe that the validity of these measurements has been thoroughly vetted. In McPherson et al (2019) these measurements are used to argue that Thorpe scale to Ozmidov scale ratios are several orders of magnitude below unity, in stark contrast to decades of observations in oceanic shear layers. In this paper, the measured dissipation values lead to dramatic conclusions about hydraulic jumps and internal waves, which, while plausible, are significant and groundbreaking. Given the potential significance of these two sets of conclusions and the fact that both are tied directly to dissipation measurements from microstructure profilers at extreme angles, I do not believe that the justification provided in the JGR paper is convincing. As one comparison, I suggest that the authors use the momentum balance in the interfacial layer to determine the stress divergence required to close the momentum budget, and compare that to the microstructure derived values, the Thorpe scale values of McPherson et al (2019) and other river plume environments. What dissipation rates would be required to close the budget? Without that context, it is difficult to gauge the relevance of the alternative mechanisms suggested by the authors. There is no doubt that these mechanisms may play a role to some degree, but their magnitude is at issue.

Additional comments (some minor) are as noted:

Line 112: The ADCP is also important in constraining F_z, correct?

Line 173: The referenced figure should be 5d

Line 179: Fr_i is not defined, and a discussion should be included of exactly how the quantity is calculated from the data. Fr_i is notoriously difficult to quantify in river plume environments due to the difficulty in constraining internal wave speed in a stratified (rather than two layer) flow. In using a classic two layer approximation, it is difficult (if not impossible) to accurately calculate a layer depth. As such, calculations of Fr_i in river plume environments typically have significant relative value (i.e., Fr is increasing or decreasing) but it is very difficult to constrain the crossing of supercritical to subcritical transitions. This becomes extremely important in later discussions regarding hydraulic jumps, so it is critical to back up these calculations here.

Figure(9): Panel (d) is extremely confusing, and does not appear to be consistent with panels (a), (b), and (c). For example, at 0.5 km, I would expect the pink bars (which are the sum of components in panel (a) to be of order 1 m/s2, the light blue bars to be of order 5 m/s2, and the blue bars to be of order 1-2 m/s2 (these are approximations by eye). The bars shown are not consistent with these approximations. Please clarify the intent of panel (d) or correct the plot if necessary.

Line 322: An increase in plume thickness from 3.3 to 5 m is suggested in Figure 6(b). While this jump does exist for two adjacent data points, a better interpretation (given the variability in h in 6b) might be a gradual increase in h from 4 to 4.3 m over approximately 1 – 2 km.

Line 330: The authors claim that hydraulic jumps are responsible for contributing up to 30% of the energy dissipation, based on entire water column calculations, which two lines earlier they suggest is unlikely. The 2% estimate is probably more realistic.

In summary, I strongly recommend that the authors further investigate the nature of the dissipation measurements from the microstructure profiler by comparing their measurements to budget derived estimates, and revisiting the extreme angle analysis and justification. This will provide essential context for further evaluation of the profiler data taken at extreme angles, and may have ramifications not only for the present manuscript but for the manuscript recently accepted by JGR-Oceans.

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