Response to Reviewer 2 comments on McPherson et al., 'The role of turbulence and internal waves in the structure and evolution of a near-field river plume'– original comments and responses are headed in bold and italics respectively.

Author Comment:
We thank the Reviewer for their insightful and helpful comments and suggestions. Their dedicated time and thoroughness have improved the quality of the manuscript. Below, we have responded to all their comments.

Reviewer Comment:
1. Concerns regarding tilt angle of VMP:
The authors have responded by pointing to the Appendix of the JGR paper to support their use of the VMP data set. I have conducted a careful review of the response as well as the earlier JGR paper, and the Lueck et al 2013 technical note. To a large degree, my concerns about the data set were motivated by concerns over the conclusion drawn in the JGR paper regarding the L_T/L_O ratio, as best illustrated in Figure 12 of the JGR paper. Although this is not directly relevant to the paper currently under review, I am compelled to address these concerns here as they relate to the dissipation data set. Figure 12 (JGR) shows extremely high values of L_O, ranging from 1 m to 1000 km (10^6 m)! In my initial reading of the JGR paper I was not as focused on the range of values, but only that they were large, and thus attributed this to an over estimation of dissipation rates, which were measured at relatively high angles. Upon further inspection, there appears to be some other calculation error in the JGR manuscript affecting these values of Lo. Given the definition of Lo, and values of epsilon ranging from 10^{-6} to 10^{-2}, appropriate values of N2 necessary to generate Lo values of 1000 km would be 10^{-12} to 10^{-9}. Actual values of N^2 range from 10^{-3} to 10^{-1}. Alternatively, note that the data shown in Figure 9(JGR) is associated with L_O values falling between 1 cm and 1 m, using the axis values of N^2 and epsilon to calculate L_O. These values are very consistent with the range of values of L_T shown in Figure 12 (JGR). Thus, my initial concerns regarding the dissipation data set may have been overstated, but I strongly encourage the authors to revisit the L_O calculations relevant to the JGR paper and issue a correction as necessary. With regards to the shear probe data, I still have concerns about the highest dissipation values observed O(10^-2 W/kg), which appear to be associated with attack angles exceeding 15 degrees. However, I recommend that these concerns be dealt with in the manuscript in comparison to, for example, the residual divergence estimates discussed further below, or that a cutoff angle of 10 or 15 degrees also be used and compared with the present averages. In closing on this topic, I should also mention that I agree entirely with the authors' comment that the measured dissipation rates should be much higher than those found in ocean shear layers. In fact, the observed values are largely consistent with values seen in the Columbia, the Merrimack,

and other plumes. My initial comment was not focused on the magnitude of the dissipation rates themselves in comparison to ocean shear layers, but only on the ratio of L_T/L_O. Given the points made above regarding L_O, this discrepancy may now be resolved.

Response:
The authors thank the Reviewer for their comments and the thoroughness of their response concerning the VMP data set in both papers. With regards to the shear probe data, a standard limit of $\theta = 20°$ was applied to the VMP data in both analyses as recommended by Osborn & Crawford (1980), and Lueck et al. (2013), amongst others. Reducing that limit to 10 or 15° would rule out the high dissipation rates that are representative of the strong velocity shear that actually drives the tilt. As concluded in the JGR Appendix, the high tilt of the profiler is a result of the strong shear which is accurately represented by the high ϵ; the high ϵ are not erroneous signals due to the tilt. The issue of VMP tilt and the impact on ϵ is now addressed when examining the residual stress divergence in the revised manuscript in the new sub-section (Assessing the control volume accuracy). The relevant part of the section reads, with reference to figures in the manuscript, is below. The point about L_O is worth further examination and highlights the nature of scaling. L_O is not a "real" quantity in the sense of being something tangibly measurable, instead it is inferred. The range of L_O in the JGR paper does suggest to us some further sampling is required, perhaps with a different platform. A moored profiler would be a suitable way of capturing the variability across this interface.

The discrepancy in residual and observed ϵ in the interfacial layer (Fig. 11), thus internal stress divergence, was not due to the inability of the control volume method to resolve high dissipation rates. As ϵ was generally greatest within the surface layer and not the interfacial layer (Fig. 5d), the good agreement between the observed and residual stress divergence terms within the plume (Fig. 10, pink) illustrates that the control method can produce reasonable estimates of internal stress in plume systems in which dissipation rates are enhanced. Control volume calculations of high turbulence stress have compared well to observed values in the near-field of the Columbia River, where $\epsilon > 10^{-4}$ W kg^{-1} (Kilcher et al., 2012).

Furthermore, measurement error in the turbulence observations is also unlikely to be a major source of error to the control volume calculations. A thorough evaluation of the microstructure profiler sampling technique and the validity of measured ϵ in shear-stratified flows is conducted in the Appendix of McPherson et al. (2019). The angle of the microstructure profiler relative the mean axial velocity increases as the VMP rises through the water column and meets the strong velocity gradients between the plume and ambient. However, the angle of the profiler did not generate erroneous ϵ when the tilt of the profiler was $< 20°$ C; the enhanced ϵ were instead representative of the intense shear-driven mixing in the surface layer. As the residual and observed dissipation rates within the interface compared well downstream of 1 km (Fig. 11), where the profiler con-

tinued to tilt due to the enhanced velocity shear throughout the whole near-field region, the angle of the profiler through the interface, for $\theta_x < 20°$ C, was not responsible for the discrepancy between measured and residual ϵ estimates near the river mouth.

Editor Comment:
The reviewer 2 detailed comments relevant to this manuscript are provided below. These provide some advise to consider in the final revision of the manuscript.

Reviewer Comment:
2. Internal waves and Hydraulic Jump:
The authors present strong evidence of internal waves observed at or below the interface, along with a reasonable estimate of their energy as a percentage of the total measured momentum. These calculations suggest that IWs may play a minor, but not negligible, role in the local dynamics. Evidence of a hydraulic jump is less clear from the available data. Changes in velocity and layer thickness are very difficult to discern from Figure 6, and may more realistically represent gradual changes as the plume evolves. Distinct and well defined hydraulic jumps are notoriously difficult to identify in these environments, and have remained elusive to many plume researchers. In the text added on hydraulic jumps, the authors point to the sharp difference in layer thickness and velocity at 1 km. Unfortunately, for both variables, the point at 1 km is an anomaly, and should not be considered in isolation. Consistent with this interpretation, the authors point to high dissipation rates in figure 6(e). These rates do not spike at the 1 km location, but are high throughout the early stages of the plume, consistent with rapid acceleration of the upper layer in the liftoff zone, followed by a gradual decrease beyond 1 km as the plume widens and decelerates. Thus, I believe the discussion about hydraulic jumps is not consistent with the data set and should be revised.

Response:
While hydraulic jumps are still discussed in the manuscript as the results do suggest their existence, the conclusions drawn from the data about the presence of jumps in Deep Cove have been revised. The conclusion has been been edited and the section which addresses hydraulic jumps and their existence in Deep Cove (Section 4.4) now reads:

As well as allowing for the release of internal waves, the transition from a supercritical to sub-critical flow regime can also indicate the presence of an internal hydraulic jump (Cummins et al., 2006). When flow dominated by kinetic energy (a supercritical flow) transitions into a flow dominated by its potential energy (a sub-critical flow), mechanical energy is released and is either radiated away by internal waves or dissipated locally (Nash and Moum, 2001; Klymak *et al., 2004; Osadchiev, 2018). These jumps can alter the vertical structure of the stratified flow by intensifying density gradients, accelerating the flow and modifying vertical shear (Nash and Moum, 2001). Hydraulic jumps have previously been observed in Deep Cove, caused by variable discharge rates, as the fast surface plume discharged into the deep, stationary ambient presents an ideal environment for their generation (OCallaghan and Stevens, 2015).*

Observations of the evolving plume structure in the near-field region are suggestive of the presence of a hydraulic jump. Typically, jump occurence is corroborated by distinct differences in the vertical thermohaline structure and the transition in flow regime from super- to subcritical. While both of these features were identified in the along-channel plume transect, a distinct and well-defined jump is not clearly evidenced in this data set. The clear decrease from $Fr_i > 1$ *to* $Fr_i < 1$ *observed at approximately 1 km downstream of the discharge point (Fig. 6g) indicates a transition in flow regime. However, care should be taken when identifying a threshold value (*$Fr_i = 1$*) using the two-layer definition of* Fr_i*. This approximation in river plume systems can accurately indicate changes in* Fr_i *however, given the thickness of the shear interfacial layer (Fig. 5b), it is difficult to constrain the transition from supercritical to sub-critical flow, thus define a specific jump location.*

A change in plume structure, characteristic of a hydraulic jump, also occured where the transition in Fr_i *was identified. The abrupt deceleration in flow speed from 1.2 to 0.8 m* s^{-1} *(Fig. 6c) and increase in plume thickness from 3.2 m to 5 m (Fig. 6b) is consistent with the thin, fast near-surface supercritical flow matched to the thicker, slower sub-critical layer. However, the high-frequency variability in the along-channel plume structure throughout the near-field region makes clearly identifying a potential hydraulic jump difficult. Such variability may instead represent changes in behaviour as the plume evolves. There exist limitations in observations with respect to hydraulic jumps due to the difficulties of resolving the sharp horizontal density and flow gradients, as well as their temporal evolution. Recently, a number of near-field plume studies have identified the formation of a hydraulic jump near river mouths using an extensive suite of remote sensing and in-situ observations (Honegger et al., 2017; Osadchiev, 2018). Thus such hydraulic mechanisms can exist in near-field plume systems under certain conditions however, the clear identification and constraining of such hydraulic processes with observational data remains challenging.*

While characteristics of a hydraulic jump can be identified in the near-field plume, though not distinct and fully resolved, the potential influence of a hydraulic jump on the distribution of near-field momentum can still be speculated about. The power dissipated across a hydraulic jump can be estimated using $E = \rho g' Q \Delta H$*, where* Q *is the tailrace discharge rate and* $\Delta H = (y_2 - y_1)^3/4y_2y_1$ *(Weber, 2001). The depths for the surface plume and Deep Cove are* $y_1 = 10$ *m and* $y_2 = 110$ *m respectively, reflecting the large difference between the depth of the surface layer and the inner fjord, thus* $\Delta H = 225$ *m. When* $Q = 530$ m^3 s^{-1}*, a total of* $E \sim 28.7$ *MW is dissipated across the jump. This is the equivalent to over 30 % of the total energy within the interface. A more conservative depth estimate for the supercritical and sub-critical layers of* $y_1 = 2$

m and $y_2 = 10$ *m respectively, as it is unlikely for the hydraulic jump to fill the entire water column, results in a total energy loss of 2 % across the hydraulic jump. Therefore, the hydraulic jump could contribute up to one third of the total dissipation of momentum within the interfacial layer and is thus a crucial, yet generally unconsidered, process in the balance of plume momentum.*

Reviewer Comment:
3. Stress divergence derived from the momentum balance:
This is important and useful information, and I appreciate the authors undertaking this analysis. I would strongly suggest that elements of the discussion included in the review be incorporated into the manuscript, including possibly Response Figure 1. However, the discrepancy between residual stress and measured stress in the interfacial layer (where tilts are presumably highest) is still troubling. The authors suggest that this demonstrates inadequacies in the residual method but do not explain why or how, except for pointing to internal waves and hydraulic jumps as possible energy sinks, but the manuscript and data are far from conclusive. Thus, it is unclear to me whether there are discrepancies in the residual method, or the measurements, or both. I suggest that this issue be tackled head on in the manuscript, even if there is no clear resolution.

Response:
A new section with this analysis and further elaboration about the potential sources for discrepancy, as the Reviewer outlines, is also included here (Section 4.2.1 Assessing the control volume accuracy), with new figures to better illustrate the analyses and determine sources of error (Figure 10 & 11). In this section, further discussion about the microstructure profiler tilt in the interface and its effects on measured ϵ are undertaken, as well as a deeper investigation of the control volume method and its assumptions. The aim is to better understand the potential inadequacies within the method and their extent, before suggesting alternate mechanisms for energy transfer. The section is also included here, with the new figures included in the revised manuscript:

4.2.1. Assessing the control volume accuracy

There are a number of possible sources for the discrepancy in budget terms in the shear-stratified interfacial layer (Fig. 9b,d). Firstly, that the budget components were not fully resolved by the observations, and measurement errors therein. Secondly, errors in the control volume technique arose from invalid assumptions. Thirdly, other processes, which were not accounted for in Eqn. 3, impacted the momentum balance within the interface of the Deep Cove system.

Potential under-sampling is a concern, particularly for the intermittent and heterogeneous turbulent field (MacDonald et al., 2013) where peak ϵ *occured within the upper 3 metres of the water column (Fig. 5d). However, as turbulence data was resolved right to the surface by the upwards-profiling microstructure profiler, stress was measured throughout the full vertical extent of the interfacial* *layer. Furthermore, as a balance of the momentum components was achieved within the surface layer (Fig. 9a,d), in which measurements are generally more difficult to resolve, insufficient observations seems an unlikely source of the discrepancy in the interface. Therefore, under-sampling should not greatly affect the stress divergence term in the interfacial layer.*

An alternate method of assessing the accuracy of the control-volume method and its estimate of the budget components is to compare the residual and directly measured internal stress divergence terms. When direct estimates of internal stress are unavailable, τ *is defined as the residual in the momentum budget, i.e., the force required to balance the control volume estimate of total plume acceleration, pressure gradient and Coriolis force (MacDonald and Geyer, 2004). The residual internal stress divergence was calculated within the plume, interface and ambient, and compared to the equivalent observed stress divergence in each layer. BBoth terms were averaged over each kilometer downstream of the tailrace discharge point to examine the along-channel difference between the two components, reflecting the spatial evolution of the other budget terms (Fig. 9). Turbulence dissipation rates were then derived from the residual stress divergence to determine the* ϵ *required to balance the momentum budget in the layer, and compared to the directly measured* ϵ*. Within the surface plume and ambient below, there was generally good agreement between the observed and residual internal stress divergence over the length of Deep Cove (Fig. 10). Both terms agreed within a factor of 2 which suggests that the control volume method produces a reasonable estimate of plume deceleration in these layers.*

Within the shear-stratified interfacial layer, the internal stress divergence over the initial 1 km was overestimated by the control volume method (Fig. 10). While the observations show a strongly negative forcing (-10^{-3} m s^{-2})*, the residual indicates a weakly positive value* (10^{-4} m s^{-2}) *required to balance the momentum budget. The difference between the magnitude of stress divergence terms is a result of the underestimation of turbulence dissipation rates within the interface by the control volume method. The observed* ϵ *from the microstructure profiler in the initial 1 km are approximately one order of magnitude greater than the residual-derived* $\epsilon = 10^{-4}$ W kg^{-1} *estimates (Fig. 11), which leads to the weaker residual stress divergence. Downstream, both observed and residual* ϵ *compared well which was reflected in the good agreement between internal stress divergence terms over 1 - 2 km and 2 - 3 km from the discharge point (Fig. 10).*

The discrepancy in residual and observed ϵ *in the interfacial layer (Fig. 11), thus internal stress divergence, was not due to the inability of the control volume method to resolve high dissipation rates. As* ϵ *was generally greatest within the surface layer and not the interfacial layer (Fig. 5d), the good agreement between the observed and residual stress divergence terms within the plume (Fig. 10, pink) illustrates that the control method can produce reasonable estimates of internal stress in plume systems in which dissipation rates are enhanced. Control volume calculations of high turbulence stress have compared well to observed values in the near-field of the Columbia River, where* $\epsilon > 10^{-4}$ W kg^{-1} *(Kilcher et al,. 2012).*

Furthermore, measurement error in the turbulence observations is also un-

likely to be a major source of error in the control volume calculations. A thorough evaluation of the microstructure profiler sampling technique and the validity of measured ϵ is conducted in the Appendix of McPherson et al. (2019). The angle of the microstructure profiler relative the mean axial velocity increases as the VMP rises through the water column and meets the strong velocity gradients between the plume and ambient. However, the angle of the profiler did not generate erroneous ϵ when the tilt of the profiler was $< 20^\circ$ C; the enhanced ϵ were instead representative of the intense shear-driven mixing in the surface layer. As the residual and observed dissipation rates within the interface compared well downstream of 1 km (Fig. 11), where the profiler continued to tilt due to the enhanced velocity shear throughout the whole near-field region, the angle of the profiler through the interface, for $\theta_x < 20^\circ$ C, was not responsible for the discrepancy between measured and residual ϵ estimates near the river mouth.

The analysis above suggests that neither failing to fully resolve the budget components from observations, nor errors in the control volume method for estimating turbulence stress, are likely to be the main source of discrepancy between momentum budget components in the shear-stratified interfacial layer. Other potential errors in the control volume result from assumptions in estimates of lateral spreading, and assuming that the v-component of velocity was equal to zero along the plume axis. The plume width is estimated from Eqn. 7 which provides a good estimate of lateral fluxes within the plume layer where u and the salinity gradient are high (Kilcher et al., 2012), which is the case in the interfacial layer (Fig. 5). Furthermore, the control volume b matched well with estimates of plume width derived from the GPS drifter experiment described in Section 2.2, and with measurements of b from observed across-channel velocity structure (Fig. 1c). Thus error in the estimates of plume spreading should be minimal. The second assumption is that the plume is aligned with the along-channel transect (i.e., $v = 0$). The sampling transects (Fig. 1c) suggests that the vessel was aligned with a plume streamline however, as the Coriolis term was non-zero (Fig. 7, 9), this indicates that sampling was not directly aligned with the core of the plume. While this could give an error in the control volume estimate of plume deceleration hence residual stress divergence, calculations in the surface layer are relatively unaffected by lateral effects due to the alignment of the streamwise direction with the mean plume flow direction. The general agreement between stress divergence terms within the surface plume (Fig. 10) indicates that any error introduced by this assumption remains small. A more detailed consideration of these assumptions and their potential as sources of error to control volume calculations are discussued in Kilcher et al. (2012) and MacDonald and Geyer (2004), and future studies should more accurately resolve these terms to determine a more reasonable estimate of the control volume-derived stress component.

The influence of other riverine physical processes, not resolved by the traditional budget, on the momentum of the system is now considered. To balance the strongly negative $\partial\tau/\partial z$ (Fig. 9b), a positive Du/Dt is required. Return flows are intrinsic to estuarine circulation and propagate in the opposite direction of the plume between the surface layer and ambient below (Pritchard, 1952). The *up-fjord directed current would transport momentum back into the system along the pycnocline. In order to balance the observed* $\partial\tau/\partial z = -10^{-3}$ *m* s^{-2} *within the interface, the return flow would have to increase by approximately 0.9 m* s^{-1} *from the seaward end of Deep Cove to the tailrace discharge point. This is more than four times greater than the difference of* ~ 0.2 *m* s^{-1} *between return flow velocities at either end of Deep Cove measured by OCallaghan and Stevens (2015). Therefore, a return flow would not be sufficient to contribute to the additional up-stream momentum required to balance the budget components in the interface.*

Reviewer Comment:
4. Additional comment regarding Fr_i:
The authors provide a clearer response regarding their definition of Fr_i, elements of which should be included in the manuscript. Note however, that $G^2 = F1^2 + F2^2$, which should put G much more in line with $F1$, as is typical in plume environments. That said, the two layer approximation can be a good indicator of changes in Froude number, but given the thickness of the shear layer (and lack of two well defined layers), it should not be used to identify thresholds such as $Fr = 1$. Small changes in layer thickness can result in significant changes in Fr, and the profiles should only be taken to demonstrate changes in Fr (e.g., sharp or gradual decreases after ~ 0.5 km).

Response:
This is an interesting point about the Froude number and how it can and should be used outside the laboratory where such two-layers systems are not always so distinct. The authors have amended the discussion to reflect this point and the conclusions drawn from the Froude number. The text is included in the above section (4.2.1). The definition of Fr_i from the previous response has also been included in the methodology and now reads:

The internal Froude number, $Fr_i = u_f/c$ *is defined using the vessel-based instrumentation, where* u_f *is the near-surface flow velocity estimated from the ADCP, and* $c = \sqrt{g'H}$*, where* H *is the thickness of the surface layer defined by the depth of maximum stratification. This definition of* Fr_i *has been used in previous river plume studies to determine flow regimes (Hetland, 2010; O'Callaghan and Stevens, 2015; Osadchiev, 2018).*

Reviewer Comment:
5. Additional comment regarding plume thickness:
As discussed above, the new text still focuses primarily on the drop between two adjacent points, and is misleading.

Response:
The text has been amended to reflect the change in the focus of the discussion

surrounding hydraulic jumps, included in Section 4.2.1 above.

Reviewer Comment:
6. Additional comment regarding hydraulic jump contribution to the energy budget:
As discussed at length above, the data does not provide convincing evidence of a hydraulic jump. Researchers have been speculating for decades about the existence and/or importance of hydraulic jumps in plume dynamics, but no one has been able to successfully identify and constrain with data, the existence of these jumps. This study is no exception, as the data presented may be suggestive of a jump, but is full of ambiguities. A major driver in the lack of well defined hydraulic jumps in plume regions is likely the result of width expansion and stratified shear mixing driving deceleration of the upper layer, resulting in a "softer" transition to subcritical flow that occurs over several km, rather than a sharp and well defined hydraulic jump. Thus, I believe that the language regarding hydraulic jumps in the manuscript should be softened, and the manuscript should be revised to reflect the reality of, and limitations of, the observations with respect to hydraulic jumps.

Response:
The authors response to the topic of hydraulic jumps have been discussed in detail above. Pertaining to this point, the authors have amended sections of the manuscript when connecting internal waves, hydraulic jumps and the energy budget. The relevant part of the conclusion now reads:

Internal waves were observed propagating along the base of the surface layer, visible in both near and far-field plume regions (Fig. 4, 13), and were capable of transporting almost 15 % of the total energy out beyond the plume's boundaries. The generation of internal waves by river plumes and their transport of energy and momentum along the pycnocline has been previously observed in both large and small river systems (Nash and Moum, 2005; Pan and Jay, 2009; Osadchiev, 2018). Evidence of an internal hydraulic jump was suggested by a transition from a supercritical to sub-critical flow regime in the initial 1 km (Fig. 6f) and a modification of plume flow speeds and vertical structure, characteristic of a hydraulic jump. However, the observations were unable to clearly resolve the sharp gradients and temporal evolution of a jump, thus the existence of such a hydraulic feature can only be speculated about. Thus the momentum within the system which was not resolved by Eqn. 3 could be accounted for by considering the redistribution and dissipation of momentum by these processes. The consideration of internal hydraulics and wave radiation when evaluating a momentum budget in a shear-stratified environment is therefore necessary to fully understand the impact of governing dynamics on plume behaviour and evolution.

Reviewer Comment:

Overall, I still believe that the study contains an interesting data set, and that many of the calculations and analyses may be valuable to the community. However, the authors should be cautious in reaching too far beyond the data in drawing their conclusions.

Response:
The authors thank the Reviewer for their suggestions and recommendations, and recognition of the value of this analyses. We hope that our response has sufficiently addressed any concerns.