Reviewer#1.

The authors are most grateful for your comments. We have followed your suggestions and revised the manuscript accordingly in many places. Please, find our responses below.

GENERAL

This paper uses primarily 2-D simulations to study the collision of internal solitary waves with trapped cores of different amplitudes. The motivation is observed collisions of Morning Glory clouds in Australia. Results focus on the phase shift, amplitude change and kinematic mechanisms underlying the actual collision. I find this paper to be an interesting read which, nevertheless, leaves several questions. Numerous questions exist about how the simulations sweep parameter space, how the initial trapped core waves are set up and the physical mechanisms behind the actual collision. In terms of the latter, I am greatly concerned about the adequacy of the 2-D and 3-D resolution of the simulations, particularly in light of the use of a Schmidt number of $O(10^3)$?!? How well do these simulations resolve the finer features one expects, even in 2-D, due to the wind-up of the isopycnals by the K-H billows and how can we truly speak of turbulence and mixing at the resolutions used ? How much are the computed fields smeared at the finest-resolved scale by numerical diffusion ? Finally, there are a few points where the English needs polishing. One general grammatical comment: When describing the results, the authors often shift between past and present tense. Please keep the verb tenses consistent throughout the text. I list my specific comments below. If the authors address them I will gladly consider re-reading the paper to recommend it for publication.

Answer. See answers to specific comments.

SPECIFIC

Abstract

Line 12: Change "monotonous" to monotonic.

Answer. Done.

Introduction
Page 2, Line 2: The English feels awkward here. I would change to "… experiments and numerical solutions of both the DJL equation and the actual Navier-Stokes equations.

Answer. Done.

Section 2
1. Use of a Schmidt number of $Sc = v/\approx 1,000$ is highly perplexing. Such a value of Sc should allow the formation of very fine scale patterns in the density field: 2-D runs can support very sharp gradients, either due to the straining of the pycnocline during collision or due to the roll-up of isopycnal lines by K-H instabilities, which are most likely below grid resolution. In 3-D, one would expect a Batchelor scale (presuming the K-H billows can attain some level of turbulence) which is equal to 10001/2 times smaller than the Kolmogorov scale. Are the simulations resolving this scale ?

The authors need to clarify the following points:

a. *Have they conducted grid independence studies at least for their 2-D higher-amplitude ISW collision runs, where we expect the finest-scale patterns to form in the density field ?*

Answer. We carried out doubling-grid tests to verify that chosen grid adequately described flow fields. The comparison for wave A13 is shown in Figs. A1 and A2 (see answers to Comments 1b-1c). The text was added accordingly.

p. 4 l. 25 "Most of the runs were performed in a two-dimensional setting with a grid resolution of 3000×400 (length and height, respectively), whereas several runs for waves A9-A13 were also carried out with a grid resolution of 6000×800 (length and height, respectively) to verify effect of grid resolution on the wave interaction and to make the fine structure clearer. Comparison of the baseline and doubled grid resolution showed the equivalence of the calculated fields, with the exception of wave A13 for which 6000×800 resolution was used."

b. *How many grid points span the actual pycnocline ? My back-of-the-envelope calculations show that the pycnocline is very coarsely resolved. Upon wave collision, it'll even be further strained and less resolved. Numerical diffusion of the low-order method underlying the authors' model can artificially smooth out things.*

Answer. For the series A number of grid points span the pycnocline was 17 for grid 3000x400 and 35 for grid resolution 6000x800, for the series B the number of grid points span the pycnocline was 34 for grid 3000x400,whereas for the series C the number of grid points span the pycnocline was 68 for grid 3000x400.

c. *In a 2-D run, how many grid points does one have across a K-H billow associated with instabilities along the wave ? One would need at least 30 grid points to guarantee that the resultant transverse instabilities are properly resolved in 3-D.*

Answer. In our simulations about 45 grid points were placed across KH billow in the case (A13;A13) and Sc=1000 for grid resolution 3000x400 and more than 90 grid points covered KH billow for grid resolution 6000x800 as shown in Fig. A1-A2. For the rest of series of experiments this coverage was greater.
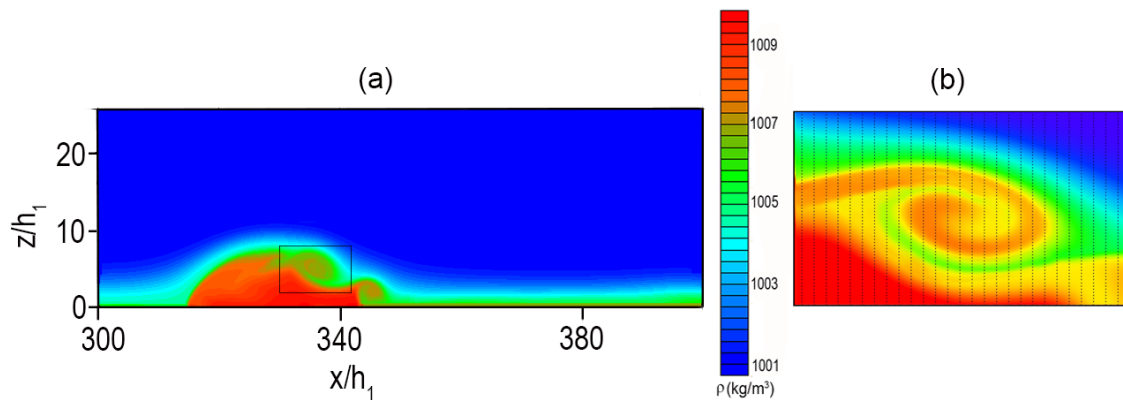

Fig. A1 Snapshot of the density field for case (A13;A13) at $\tau=175$ and Sc=1000 for grid resolution 3000x400 (a) and extended snapshot of KH billow with grid points (b).
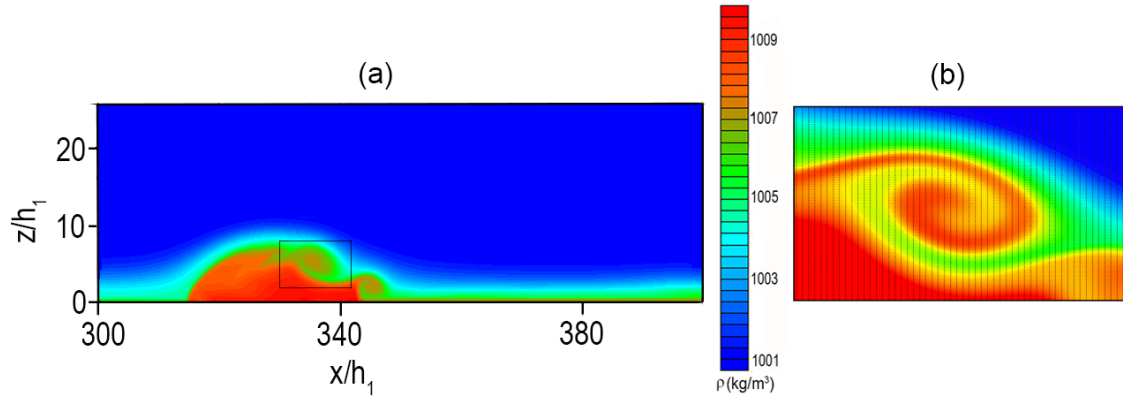
Fig. A2 Snapshot of the density field for case (A13;A13) at $\tau = 175$ and Sc=1000 for grid resolution 6000x800 (a) and extended snapshot of KH billow with grid points (b).

    d. *When 3-D runs are conducted, what is the local Reynolds number (based on local value of shear and B-V frequency along the wave-strained pycnocline) in the regions where K-H billows are observed, prior to K-H billow formation ? Is this Reynolds number high enough for actual turbulence to form within these billows or do they simply form, possibly pair and support some weak transverse instability ? How do we know that there are not scales smaller than the transverse instability that form ? Again, numerical diffusion can drive some very spurious results here.*

Answer. We excluded results of 3D simulation from this paper.

    e. *MOST IMPORTANTLY: In 2-D, the authors should conduct a comparison of one simulation of high amplitude ISW collision at Sc = 1 and 1000, where I would hope/assume Sc = 1 is well-resolved by the authors' choice of grid. How do the results compare ? The Sc=1 case is presumably more relevant to the atmospheric Morning Glory case which motivates this study.*

Answer. Text and figure were added to consider the impact of small diffusivity on the collision processes.

p. 7 l.8 "In the ocean and in the most of the laboratory experiments the Schmidt number is about 700-800. The used grid does not allow the whole range of inhomogeneities in salinity (density) to be resolved. Therefore, it is important to evaluate the effect of molecular diffusion of salinity on the dynamics of waves and to verify the possibility that diffusion can be neglected in the wave collision for large Sc. Two cases for large amplitude waves were considered (A9;A9) and (A13;A13). We performed runs for Sc=1; 10 and 1000. In the collision case (A9;A9) the behaviour of colliding waves are the same, whereas the difference between runs for Sc=1 and Sc=1000 was less than 1% of $\Delta\alpha/\alpha$ and $\Delta\theta$ values. The comparison of the density snapshots during collision in case (A13;A13) for different Schmidt numbers is shown in Fig. 9. Figure clearly depicts difference between structure of interacting waves for cases Sc=1 and Sc=10. The corresponding values of $\Delta\alpha/\alpha$ and $\Delta\theta$ differ by 5% and 0.6%, respectively. This was in agreement with the results by Deepwell and Stastna (2016), where it was shown essential effect of molecular diffusivity on the mass transport by mode-2 ISW in the range $1 \leq Sc < 20$. At the same time, the results of calculations at Sc=10 and Sc=1000 in Fig.9b and 9c

practically coincide, which indicates that molecular diffusion may not be taken into account when studying the global properties of colliding waves. This conclusion agrees with (Terez and Knio, 1998) as they estimate that the value of Sc=100 was "sufficiently high for density diffusion to be ignored during simulation period" and the results of the Deepwell and Stastna (2016) simulation, according to which the mass transfer is virtually independent of Sc already at Sc>20. However, diffusion can be important for small scale mixing processes in tiny density structures (see e.g. Galaktionov et al., 2001) forming in result of instability and turbulent cascade processes (Deepwell and Stastna, 2015) and persisting over time in a wake behind moving bulge of trapped fluid (Terez and Knio, 1998). These subgrid scale structures in our simulations were smashed by numerical diffusion which did not affect larger scale due to use of second order total variation diminishing (TVD) scheme for advective terms in transport equation. "
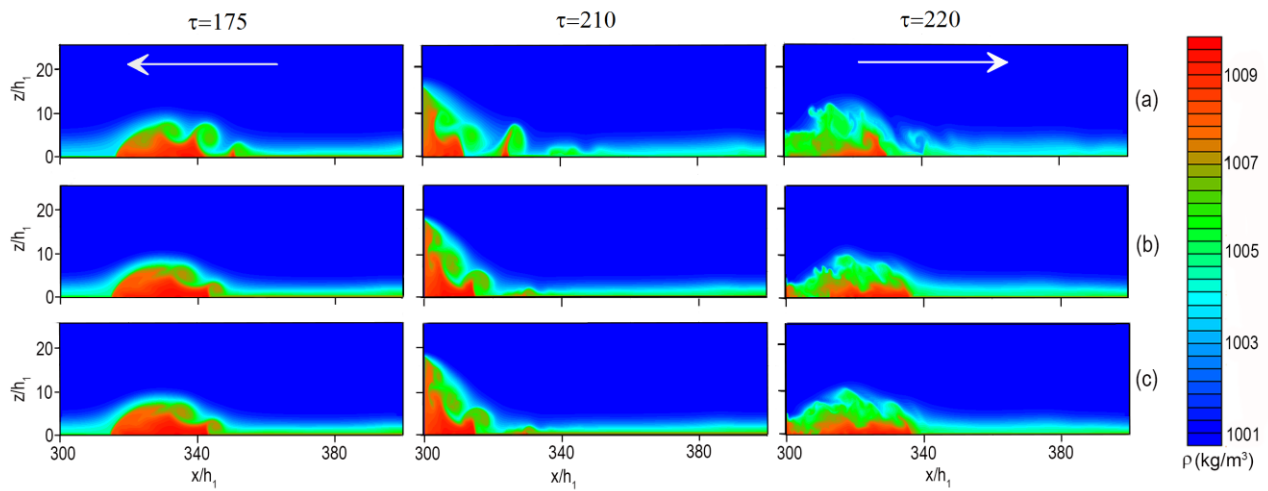

Figure 9. Comparison of the density snapshots during collision of ISWs in case (A13;A13) for different Schmidt numbers. (a) Sc=1. (b) Sc=10. (c) Sc=1000. The right half of the numerical flume is shown due to the symmetry of the interaction process.

The authors need to answer all the above questions. If they cannot they should at least be honest that their results are highly contingent on the degree of pycnocline resolution and the degree of numerical diffusion in their low-order numerical method.

2. Page 3, Line 10: The authors discuss at this point the various scaling parameters they use. Later on in the paper, in page 7, there's a discussion as to how such a scaling does not work for the Euler equations. To this end, it would help greatly if the scaled Navier-Stokes eqns. were written out explicitly hereand a warning was given to the reader about potential inapplicability of this finding to the Euler eqns.

Answer. We included dimensionless NS equations and clarified discussion on complete and incomplete similarity on non-dimensional parameters as you suggested.

p. 3 l.27 "Generally, however, the flow dependence on the viscosity, diffusivity and depth can retain at $Re \rightarrow \infty$, $Sc \rightarrow \infty$ and $\varepsilon \rightarrow \infty$ and scaling on them is called incomplete (Barenblatt,1996). In most cases it is impossible to determine the kind of self-similarity *a priori*, until the solution of the full problem. Like Maderich et al. (2015), we follow suggestion by Barenblatt (1996) "assuming in succession

complete similarity, incomplete similarity, lack of similarity - and then comparing the relations obtained under each assumption with data from numerical calculations, experiments, or the results of analytic investigations". The simulation results (Maderich et al., 2015), show that the flume depth in the range $23 \leq \varepsilon \leq 92$ does not affect the characteristics of the ISWs with trapped cores. The sensitivity of the wave dynamics to the values of ε was found by Carr et al., (2008) in the range $4 \leq \varepsilon \leq 11$. From these studies we conclude that results our simulations in the range $23 \leq \varepsilon \leq 92$ (Table 1) does not depend on ε. The possible effects of Schmidt and Reynolds numbers will be discussed in sections Sect. 3.4 and Sect.3.6."

p. 8   l. 28   "From dimensional arguments $\Delta E_{loss} = \Phi(\alpha, \mathrm{Re}_m, \mathrm{Sc})$, where Φ is function   of three arguments. Assuming complete similarity on the $\mathrm{Re}_m \to \infty$ and $\mathrm{Sc} \to \infty$ consider dependence ΔE_{loss} on α. As seen in Fig. 12, this dependence given for symmetric collisions ($\alpha = \alpha_L = \alpha_R$) is not monotonic and is not universal, changing depending on the series of calculations. "

p. 9 l. 8 "The absence of complete self-similarity on the Reynolds and Schmidt numbers also means that the Euler equations do not describe the wave interaction processes in deep water even for the range of stable waves. As shown in Table 1, the parameter Re_m varies in Series A-C several times for waves of the same dimensionless amplitude α. The incomplete similarity scaling following Barenblatt (1996) results in relation: $\Delta E_{loss} = \Psi(\alpha) \mathrm{Re}_m^m \mathrm{Sc}^n$, where Ψ is function of α, m and n are exponents. However, this rescaling also did not result in universal dependence. We conclude that it is due to the different mechanisms governing collision process in ranges I-III: nonlinear wave interaction, collapse of collided trapped masses and instability. Another factor influencing the interaction may be the diffusivity effect (Deepwell and Stastna, 2016), which is described by the Schmidt number. However, in these experiments, the Schmidt number was large and constant. "

3. Same page, line 19: Correct to "The simulations of interacting ISWs". Now, when one turns to table 1, there is an exhaustive list of simulations, organized in 4 groups, A through D. This is not an easy table to read. Please separate groups A, B, C and D by a space. Also, both in the text of page 3 but also in the figure caption, help the reader out by clearly stating what A, B, C and D represent. Finally, in the caption define what the first 5 parameters are so that the reader doesn't have to flip back and forth to the actual text.

Answer. We added text to explain difference between groups A-D, and added text to caption and also separated groups A-D in the Table 1.

p. 4 l. 14 "The waves are divided into four groups: (A,B,C) depending on the thickness of the stratified layer and D for simulation of ISW reflection from a vertical wall in the laboratory experiment (Stamp and Jacka, 1995)".
p. 14 Table 1. "Summary of parameters of interacting ISWs: pycnocline thickness parameter h, wave amplitude a, wavelength $\lambda_{0.5}$, ratio ε, dimensionless ISW amplitude α, Froude number Fr_{max}, minimum Richardson number Ri_{min}, Reynolds number Re_m and class of ISW."

4. Same page, line 23: Apparently, the authors are using these runs to double up for both simulations of mode-1 waves with trapped cores, for a near-surface stratification, and mode-2 waves in a two layer

stratification. The latter assumes perfectly symmetry of the solution around the middle of the pycnocline. Is this a realistic assumption and could it lead to misrepresentation of the actual physics ? How do the authors contrast this approach to that used by Stastna and Deepwell who examine the full domain.

Answer. We carried out simulations in deep flume ($\varepsilon \gg 1$) when depth of flume effects were weak that allowed using the results of simulation near the bottom as for surface layer as for mode-2 waves assuming symmetry. The simulations by Stastna and Deepwell (2016) for mode-2 waves corresponded value of $\varepsilon = 10$ whereas we carried out simulations in range $23 \leq \varepsilon \leq 92$. The text was reworked accordingly.

p.4 l. 21 "For large ε, these allow for the simulation of the interaction of mode-1 ISWs with trapped core, propagating in stratified layers near the surface, and the ISWs interaction near the bottom, as considered here, and the interaction of mode-2 ISWs, assuming symmetry in the Boussinesq approximation around the horizontal midplane (Maderich et al., 2015)."

5. Same page, line 26: Is the no-flux condition applied to salinity or density ? The authors should clarify what active scalar they actually examine and what type of equation of state they use, if it is salinity they are actually working with.

Answer. We used salinity stratified water. The text was added accordingly.

p. 2 l. 27 "A free-surface non-hydrostatic numerical model for variable-density flows using the Navier-Stokes equations in the Boussinesq approximation (Kanarska, Maderich, 2003; Maderich et al., 2012) was applied in the simulations of a numerical basin emulating a laboratory flume filled with salinity-stratified water.
p. 3 l. 3 "An equation of state $\rho = \rho(T, S)$ (Mellor, 1991) was used for constant temperature $T = 15^{\circ}C$."

6. How are the initial actual waves generated ? Are they produced by solving the DJL equation and then inserted into the Navier-Stokes solver to allow for the trapped core to actually evolve dynamically ? Alternatively, is some higher-density fluid released at the pycnocline as done by Stastna and Deepwell ?

Answer. The ISWs were generated at both ends of the flume by the collapse of the mixed regions (see p. 3 bl.5).

7. See Comment 1 above: How do we know that the resolution used by the authors is sufficient ? Have grid-independence tests been conducted ? What is the resolution of various critical lengthscales of the problem ? I seriously question the utility of the 3-D runs, at least until the authors are honest about their limitations.

Answer. See answer to comment 1.

Results
8. Page 4, Line 16: The reference to fluid having escaped both trapped cores and then subject to a buoyancy-driven collapse, countered by viscosity and diffusion of mass, raises the question: Are the trapped cores of the original waves subject to any leakage of mass in the first place ?

Answer. We refined description of the experiment, accordingly.

p. 5 l. 17 "The trapped fluid slowly leaks from rear of trapped bulge similarly to the laboratory experiments (e. g. Maderich et al., 2001; Brandt and Shipley, 2014). However, after collision, the waves lost all fluid trapped by the wave cores."

9. Page 4, line 29: What is a "small offset pycnocline" ?

Answer. The text was rewritten accordingly.

p. 6 l.1 "Some mass exchange that occurred in the mode-2 experiment (Stamp and Jacka, 1995) was, perhaps, the result of a slight displacement of the pycnocline in the vertical direction, which is often observed in laboratory experiments (Carr et al., 2015).

10. Page 5, Line 14 and onward: We suddenly are told that the numerical simulations include runs with internal waves with trapped cores reflecting off a side boundary. See my comment (3) above. Nowhere in section 2 are we told that reflecting internal waves are studied. Pre-dispose the reader please !

Answer. The run with reflection from side boundary was separated in text and table, accordingly.

p. 4 l. 14 *"*The parameters of interacting ISWs are given in Table 1. The waves are divided into four groups: (A,B,C) depending on the thickness of the stratified layer and D for simulation of ISW reflection from a vertical wall in the laboratory experiment (Stamp and Jacka, 1995)."

11. Same page, line 30: Beyond K-H instabilities, are the other mechanisms through which fluid can escape the trapped core ? Consulting Kevin Lamb's two JFM papers (2002 and 2003) might provide some useful insights in this regard.

Answer. The text was added accordingly.

p. 7 l. 1 "The waves carry out trapped fluid, but the cores gradually lose trapped fluid to the wake through KH billows shifting to the wave rear and through recirculation in trapped core (Terez and Knio, 1998; Maderich et al., 2001; Lamb, 2002). "

12. Same page, line 33: Can one truly speak of mixing in a 2-D context, when the actual process is turbulent but not resolved in 3-D ? At least qualify the statement by saying that "mixing, as represent in a 2-D context".

Answer. We eliminated the "mixing" in this sentence. The text was added also to another sentence.

p. 6 l. 10. "Then, the fluid in the cores is entrained by the outgoing waves with some mixing, as represent in a 2-D context, arising due to instability."

13. Figure 9 and relevant discussion in text: The top four panels need to be magnified by at least a factor of two. Any smaller-scale feature is barely visible and any transverse structure cannot be seen at all. This begs the question once again, how well-resolved are these transverse instabilities ? The authors use 45 spanwise grid-points and it seems that the domain is wide enough to capture about 4 (??) wavelengths thereof. Again, taking into account the numerical diffusion of their method, can we really

speak of resolving anything below the scale of the transverse instability ? Please see my comment (1). As such, any mention of turbulence and mixing in this section should be made with great caution.

Answer. We excluded results of 3D simulation from paper. Therefore, Figure 9 was substituted by figure with Sc impact analysis.

14. *Page 7, line 11: More detail is needed as to how ΔE dis is defined. Does one conduct a run of a single wave and measure the energy at the beginning at end of the run, with any losses driven by viscous decay (and apparently numerical diffusion) and shear instability ?*

Answer. We provided more detail on calculation of the energy loss due to the wave collision:

p.8 l.14 We defined the energy loss due to the wave collision (ΔE_{loss}) as the difference between the total loss of energy ΔE_{tot} due to the collision and the loss of energy by two single waves due to the viscous decay or instability ΔE_{dis}

$$\Delta E_{loss} = \Delta E_{tot} - \Delta E_{diss} \tag{9}$$

The relative loss of energy due to the collision of ISWs can be calculated as the normalized difference in energy of waves before and after collision

$$\Delta E_{tot} = \frac{PSE_L^{(bf)} + PSE_R^{(bf)} - PSE_L^{(af)} - PSE_R^{(af)}}{PSE_L^{(bf)} + PSE_R^{(bf)}} \tag{10}$$

$$\Delta E_{vis} = \frac{PSE_L^{(bf)} + PSE_R^{(bf)} - \overline{PSE_L^{(af)}} - \overline{PSE_R^{(af)}}}{PSE_L^{(bf)} + PSE_R^{(bf)}} \tag{11}$$

where $PSE_L^{(bf)}$ and $PSE_R^{(bf)}$ are the pseudo-energies of the waves before collision at the cross-sections x_L and x_R, respectively, and $PSE_L^{(af)}$ and $PSE_R^{(af)}$ are the pseudo-energies of the waves after collision at the cross-sections x_L and x_R, respectively, whereas $\overline{PSE_L^{(af)}}$ and $\overline{PSE_L^{(af)}}$ are the energies of the transmitted waves without interaction at cross sections x_L and x_R, respectively. The pseudo-energy is the sum of the kinetic and available potential energies (Shepherd-1993) of waves before and after collision. The method for estimation of the available potential energy and energy fluxes was given in (Scotti et al., 2006; Lamb, 2007). A detailed description of the procedure of the pseudo-energy calculation was presented by Maderich et al., (2010).

15. *Same page, line 22 and onward: This is a very interesting discussion. However, please see my comment (2) above. Including the actual scaled Navier-Stokes in the text would help the reader understand why this scaling won't apply to the Euler equations. Moreover, the remaining discussion is confusing. Please clarify what is meant by "complete" and "incomplete" similarity. As always, my concern of use of a Schmidt number close to 1,000 arises.*

Answer. See answers to comment 2.

Conclusions

16. Page 8, line 10: This study also examines mode-1 waves, simply with a near-surface stratification. Clarify that this contrast is made to mode-1 waves in a "two-layer stratification".

Answer. Done.
p. 9 l.25 "The dependence is similar to the interaction of the mode-1 waves in a two-layer stratification (Terletska et al., 2017), with the difference being that the phase shift continues to grow for the collision of interfacial waves of mode-1.".

17. Same page, line 15: Again, I doubt that this study resolves any turbulence. What we're seeing is the product of numerical diffusion. Also, correct "monotonous" to "monotonic".

Answer. The text was changed accordingly.
p.9 l. 29 "The collision of locally shear unstable waves of class (iii) was accompanied by the development of instability."

18. Trapped cores in internal solitary waves are efficient mechanisms for transporting particulate matter, not just mass (see the work of Lamb). Can the authors at least offer some comment here as to how much collision impacts the capacity for an ISW to transport mass ?

Answer. We added text accordingly.
p. 9 l. 30 "We conclude that this kind of interaction reduces the capacity for an ISW to transport mass."

19. It is clear to me that this study examines trapped core waves where the core forms due to near-surface stratification, i.e. one is looking at surface cores. However, the work of Lien et al. clearly observed subsurface cores in the South China Sea ; the localization of the cores in the subsurface orginates from the presence of a background current and the specifics of its vertical structure. Although I see an investigation of ISWs with subsurface cores to be outside of the scope of the particular study, it would help if the authors referenced such phenomena as a topic of future investigation.

Answer. We mentioned work by Lien et al. (2012) in Introduction. The text was added accordingly.
p.10 l. 9 "The obtained results can be applied to the interaction dynamics of subsurface trapped core formed within a shoaling large amplitude internal waves (Lien et al., 2012)."