I enjoyed reading this paper because the numerical simulations are of high quality, the experimental design is well devised, and the results yield interesting insight into the behavior of colliding nonlinear internal solitary waves with trapped cores. I have one suggestion for major revision although this won't require too much work, and some suggestions for clarification:

1) My only suggestion for revision is that the authors remove the three-dimensional results and discussion of the mixing, dissipation, and energetics. I would only trust discussion of these if the authors demonstrated that they are truly resolved through discussion of grid resolution requirements for DNS, i.e. grid resolution via Kolmogorov scale. It is hard to imagine that the mixing is resolved given that the molecular diffusion is so small. In fact, unresolved two-dimensional simulations can lead to more mixing because the inverse energy cascade in two dimensions stretches density filaments and leads to more numerical mixing, even if the dissipation is actually lower in two dimensions (see Fringer and Street 2003; doi:10.1017/S0022112003006189, Arthur and Fringer, 2014; doi:10.1017/jfm.2014.641).

   An additional problem with discussion of the energetics in the paper is that the Reynolds number varies significantly for different runs. Arthur and Fringer (2014) showed that not accounting for Reynolds number effects can give a very different picture of the dynamics of breaking internal solitary waves on slopes. Such may be the case for the results in Figure 12, for which it is difficult to determine whether the behavior of the energy loss is due to alpha effects or Reynolds number effects.

   It may be that the two-dimensional simulations represent the energetics to a reasonable degree, as in many studies of internal wave energetics, although I would not necessarily trust the arguments concerning the mixing. Either way, I suggest that the authors discuss the three-dimensional effects and associated energetics in a different paper.

2) Please discuss how you chose the grid resolution for the two-dimensional simulations.

3) The Richardson number should be defined as $Ri_m=g'h/U_m^2$ so that it is consistent with the way the other nondimensional parameters are defined, i.e. in terms of the independent parameters following the Buckingham Pi theorem.

4) It would be helpful if, on page 3, you discussed the general features of Series A-D, and included a brief description in another column in Table 1, i.e. a column indicated by "Comments" which, for series A would state, "No trapped cores". Also please indicate whether the waves were in regimes (i), (ii), or (iii) in Table 1.

5) What is the justification for choosing such a small molecular diffusion?

6) Page 4, Line 9: Please explain the meaning of and how you computed the phase shift $\Delta\theta$, and how it is normalized by τ_0.

7) Page 4, Line 10: Please explain how you expect $\Delta\alpha/\alpha$ and $\Delta\theta$ to behave for limiting cases ($\alpha \rightarrow 0$ and $\alpha \rightarrow$ infty). Why does $\Delta\theta \rightarrow \sim 4$ as $\alpha \rightarrow$ infty?

8) On Page 5, Line 5, you state that the colliding waves pass through each other. Theory suggests that nonlinear waves exchange momentum by bouncing off each other, just like billiard balls (e.g. Fringer and Holm 2001; doi:10.1016/S0167-2789(00)00215-3).

9) Please do not include the regressions on Page 5, line 13, unless you can justify the functional relationships through scaling or other means.

10) Minor:
    a. I don't understand the meaning of the sentence starting with "The waves of class (ii) …" on line 8 of page 1.
    b. Throughout: monotonous → monotonic

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September 19, 2017