Review of the manuscript
"Observations of Shoaling Internal Wave Transformation Over a Gentle Slope in the South China Sea"
submitted to Nonlinear Processes in Geophysics by S.R. Ramp and co-authors

This paper analyzes and interprets temperature and velocity timeseries obtained over the course of two weeks from moorings positioned in a focused region over the slope of the South China Sea. It aims to provide a better understanding on generation of characteristically different types of nonlinear internal waves (NLIWs), a-waves and b-waves, in terms of phasing with respect to the background baroclinic internal tide. The paper then considers more detail how nonlinear transformations of the internal tide can lead to the appearance of each category of NLIWs and examines further structural changes these generated waves undergo as they shoal over the continental slope. Discussion is dedicated to exploring the potential for NLIW breaking (which appears to be a rather limited phenomenon at this site) and the energy transport associated with the particular NLIWs as they propagate into shallower waters.

The paper has been an interesting read, though I'll admit that I have not been able to read it to the degree of the depth that I'd normally want. It is also a bit of dense and longer write-up, with some of the figures requiring particularly close examination. This observation doesn't detract from the fact that the paper has some original contributions to the relevant literature.

My only comments have to do with clarifications and inquiries that relate to three recently published papers which, in my opinion, are relevant to this manuscript's technical scope but may have been overlooked by the authors:

Chang, M.-H. (2021). Marginal instability within internal solitary waves. Geophysical Research Letters, 48, e2021GL092616. https://doi.org/10.1029/2021GL092616

Chang, M.-H., Lien, R.-C., Lamb, K. G., & Diamessis, P. J. (2021). Long-term observations of shoaling internal solitary waves in the northern South China Sea. Journal of Geophysical Research: Oceans, 126, e2020JC017129. https://doi.org/10.1029/2020JC017129

Rivera-Rosario, G., Diamessis, P. J., Lien, R.-C., Lamb, K. G., & Thomsen, G. N. (2020). Formation of recirculating cores in convectively breaking internal solitary waves of depression shoaling over gentle slopes in the South China Sea. *Journal of Physical Oceanography*, *50*, 1137–1157. https://doi.org/10.1175/jpo-d-19-0036.1

I offer my specific comments below. Addressing these should involve minor revisions of the existing manuscript which I'd be happy to take one more look at.

Specific Comments

1. How strict are the definitions of "a" and "b"-waves, particularly in the former case, where these waves are assumed to be solitary-like? Is "solitary-like" used because these are observed as isolated waves or because the particular waveform matches fairly well the prediction offered by solution of the DJL equation ? I ask this because there are plenty A-waves in Fig. 4 (e.g. a2, A2, A3, A4) that have

at least one high-amplitude trailing wave behind them ; i.e., regarding these waves as isolated is somewhat inexact. Could the authors clarify ?

2. How may the findings of this study be contrasted to those of Chang et al. (JGR 2021) ? The particular study examines a much longer record (5-6 months) of temperature/velocity mooring data and also connects observed internal solitary waves to potential generation by the internal tide, given seasonal variations of background stratification. Where it might be different is the much larger number of convectively breaking waves observed in that dataset, most likely due to the different nature of the bathymetry and, potentially (as it was not measured), the role of background shear. Quite frankly, I'm intrigued by the fact that only one wave that showed features of convective breaking was observed in the current paper under-review.

3. My understanding is that the vertical (and potentially temporal) resolution of the measurement systems used here, particularly, temperature arrays is limited, such that the shear-instability signatures reported by Chang (GRL 2021) could not be observed. A reference to this should be made, nevertheless.

4. The discussion of wave-breaking mechanisms, via the Vlasenko & Hutter criterion, is definitely interesting. I would recommend that the authors dedicate some small additional discussion to the following:

    a. The breaking wave in Fig. 14a is distinctly asymmetric, more like what is described in Vlasenko & Hutter and the Vlasenko et al. (2005) textbook. This is most likely due to the enhanced relative proximity of the thermocline to the bottom. The discussion in the introduction of Chang et al. (JGR 2021) which points to a considerably different breaking mechanism for the waves they observed should be referenced: the waves in the site of Chang et al. are found to maintain a nearly-symmetric wave-form and, as such, their structural integrity over long propagation distances. This is confirmed by the recent 2-D modeling study of Rivera-Rosario et al. (2020).
    b. As discussed in both Chang et al. (2021) and Rivera-Rosario et al. (2020), background shear plays its own role in defining breaking criteria and the actual breaking mechanism for shoaling NLIWs. In particular, the criterion of Vlasenko and Hutter does not involve background shear and, as demonstrated by Rivera-Rosario et al., is not as efficient a predictor for the type of breaking identified in (a) above. Some short relevant discussion should be included in the paper by the authors.