Nonlin. Processes Geophys. Discuss., 2, C169–C175, 2015
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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "The evolution of Mode-2 nonlinear internal waves over the northern Heng-Chun ridge south of Taiwan" *by* S. R. Ramp et al.

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TO: Editors, Nonlinear Processes in Geophysics

FROM: Steven R. Ramp, Soliton Ocean Services, Inc.

RE: Response to reviewers for our paper entitled "The Evolution of Mode-2 Nonlinear Internal Waves Over the Northern Heng-Chun Ridge South of Taiwan"

Many thanks to the referees for their thoughtful and high-quality reviews which have improved the quality of the manuscript. Our point-by-point response to their comments

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here follows.

Referee #1:

Page 244, Abstract. Some of the detail which the reviewer found distracting has been removed. The overall quality and clarity of the abstract has been improved.

Page 246. NLIWI has been defined.

Page 246. 120.5 should be 20.5 fixed, good catch!

Page 247. We added a little about what a convex wave is referencing the Yang et al. papers. “Convex waves arise in a three-layer system when the middle layer is relatively thin (less than one-half the water depth) and consist of an upward displacement of the thermal structure in the upper water column and the opposite below. This results in a “bulge” in the thermocline with velocity in the same direction that the wave is traveling [Yang et al., 2009; 2010].”

Not noted by reviewers: Sun Yat-Sen should be Sun Yat-sen.

Figure 1. This was a very helpful idea. After acquiring better data sets, we added the requested figure to create a four-panel figure with zoom, fixed up the captions, references to the figure in the text, etc. The improved view adds insight as to where the waves are coming from which has been added into the discussion (new page 14) of the satellite image (Figure 11, now Figure 8). We also added the positions for the UCTD stations to the new Figure 1b which is very helpful later on.

Page 250 “change” is ok I think

Page 250 referencing Figures 2 and 3. We added individual letters to each panel and updated the references to them in the text accordingly.

Page 255. I think what the reviewer means here is to state clearly that it is the convex mode-2 wave that has a surface signature similar to a mode-1 elevation wave. Done.

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Page 257. “westward” should be “eastward” yes, fixed.

Page 258. Note to the editors: In my version, the table has the shading, which is essential for identifying the three different groups of wave arrivals. It somehow disappeared during type-setting. Can we please bring the shading back?

Equation 1. The = sign changed to a + sign. Fixed, not sure how that got in there. Some “word” to “pdf” conversion thing.

Page 259 (and also Reviewer #2). This should be Nash et al., [2006]. Fixed. Also, the Winters and Armi [2012] reference has been added to the reference list.

Page 265 Line 2 (I caught this on my own, not a reviewer comment) AC appears twice. The first one should be AW. Fixed.

Page 266. This wasn’t clear, agreed. We checked the calculation and changed the comment to “72% of the energy loss between moorings could be accounted for.”

Page 267. Added a sentence to make this clear. Done

Page 269. I think “from station data” would be the best way to say it. Done.

Page 270 lines 13-15. The stratification is favorable. The words describing what a favorable stratification is were inserted in the introduction (see comments under page 247 above). To check we made “waterfall plots” of density profiles vs. time and the middle layer with weak stratification is clearly seen. This is due to primarily to the salinity maximum occupying the 100-200m depth range as stated in the conclusions. I wouldn’t add the waterfall plot to the paper though. Of course this 3-layer structure is more obvious once the wave is there which expands the region of weak stratification and sharpens the density gradients above and below. This was more of a reviewer “comment” so hopefully it is ok now.

Page 270 lines 20-21. We can make a stronger statement here. The sentence was replaced by “This was because the westward tide, which peaked at -65 cm s-1 in both

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model and observations, was too weak to arrest the mode-2 waves."

Page 270, line 24. We consulted co-author Buijsman. No, the Kuroshio is not in the model. The stratification is uniform in x and y and only a function of z. We changed a sentence in the model description to reflect this. "The stratification is uniform in (x, y) and is a linear function of temperature in (z)."

Page 272 line 2-6. (Note similar wording also appeared earlier on page 264). Our model output does not extend far enough south to include the Alford et al., [2011] moorings. We can make an improved statement however: "Farther south along the Heng-Chun ridge near 20° 30âĂšN in water 1800 m deep, the maximum dissipation was order 10-5 W kg-1 [Alford et al., 2011]. These were near-bottom values observed close to the spring tide." The words in the conclusion can be more general so we left it as is.

Figure 5 caption. "latitude" should be "longitude" yes, fixed.

Figure 15 (now Figure 12) caption. I added the explanation of the vertical black lines to Figures 2 and 3 as well.

Figure 16 (now Figure 13). Added white arrows to indicate the relative magnitude and direction of the bottom flow. The spelling of "snapshots" is fixed.

Referee #2 (J. Moum)

"It's a bit tedious" We shortened the number of case studies from 5 to three, focusing on the ones that were observed also in the UCTD transects. We created a new figure (now Figure 7) that matches the waves in the backscatter image, also in labeling and how we refer to the wave packets in the case studies. This eliminated almost two pages of text and three figures. I don't think anything essential was lost. I agree it reads better now.

Emphasis: Changed the wording in the abstract and elsewhere to support the idea that the energy budget, and contribution or lack thereof to the bigger SCS energy

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budget, is the important thing. How each term was computed is clearly stated and well referenced.

Confidence limits on Figure 13 (now Figure 10). This is a bit problematic. Since no averaging went into this figure, it is not obvious how to add confidence limits. Other authors [Alford et al., 2011; Nash et al., 2012; yes even Moum et al., 2007!] have shown this same figure without confidence limits. Propagating instrumental error is insignificant [Nash et al., 2005]. To average horizontally would be to throw out what we are trying to show, that is, the different contributions from tides vs. NLIW. We could try limited averaging to produce a bar graph with confidence limits, but I don't think this adds anything. (The waves are not step functions.) There is value in showing the temporal evolution and how long the maximum values last. We would prefer to leave this figure as it is.

Fixed references as already mentioned under Referee #1

L 465. We state in a couple of places that the apparently slow observed wave propagation speeds (in some cases) relative to the theoretical mode-2 speed are due to the waves competing with background flows (tides and mean currents). The records are too short to filter: I'm not sure there is much more we can do with this. We could make a general statement like 29 cm/s (wave) + 60 cm/s (opposing tide) = 89 cm/s (mode-2 speed) but this seems pretty soft to me. We'd rather not do that. Better to just report the facts which is the observed wave speed after properly accounting for the ship's speed, which we have done.

Line 776. These energy comparisons are pretty simple. I don't think it merits adding another figure or table. We are really trying to shorten the manuscript.

Line 776. The units are the same but we are fine with W m2 so we just changed it.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 243, 2015.

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Interactive Comment

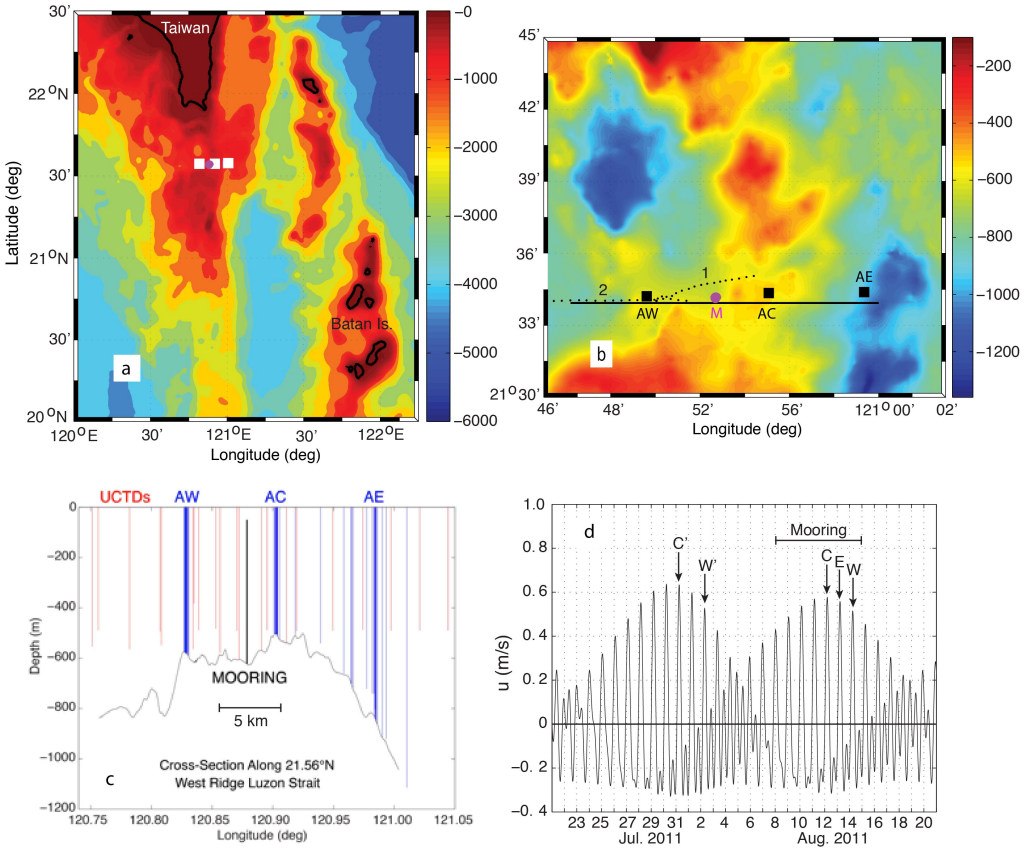

Fig. 1. New four-panel Figure 1

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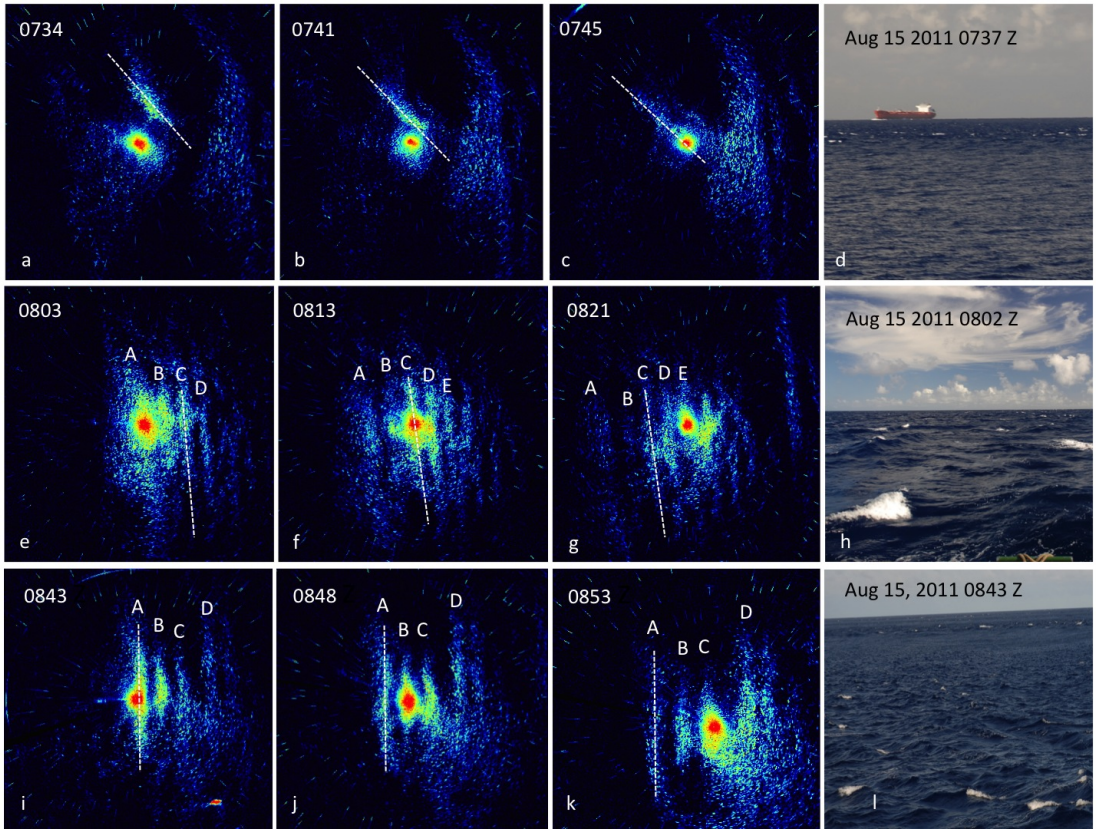

Fig. 2. New Figure 7 to match Figures 5 and 6

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