We very much appreciate the overall positive attitude of the referee to our manuscript and thank him for particularly useful comments. The comments, questions and suggestions of the referee are presented in italics.

1. *In the Introduction it is stated that "These powerful disturbances are usually excited by interactions of barotropic tidal waves with the Kuroshio Current..." (Minor comment: 'excited' is probably better replaced with 'affected'). This statement seems to be in a direct contradiction with "The field of large-scale currents was ignored" on p. 7. Indeed, the modal equations on p. 4 and 5, and the coefficients of the Gardner equation on p.5 are calculated under the assumption that there is no background shear flow. However, the presence of the flow will change the very parameters calculated and analysed in the paper. Thus, the authors are asked to justify ignoring the currents.*

Thank you for highlighting this issue. The wording is changed as follows: (page 1, 25)
"These powerful disturbances are usually excited by tide-topography interaction in the Luzon Strait where Kuroshio serves as a background current that may greatly modify the generating conditions. The resulting internal waves are further modified by numerous islands, seamounts and other bathymetric features in the Luzon Strait (Liu et al., 1998, 2004, 2006; Cai et al., 2002; Rump et al., 2004, 2015)."

"These powerful disturbances are usually excited by tide-topography interaction in the Luzon Strait, and Kuroshio can serve as a background current. They are further modified by numerous islands, seamounts and other bathymetric features in the Luzon Strait (Liu et al., 1998, 2004, 2006; Cai et al., 2002; Rump et al., 2004, 2015)."
We do agree that the Kuroshio Current may affect the wave generation, but it does not explicitly affect the coefficients of the model in the South China Sea. We have added the relevant comment into the text and commented this issue also at the end of the body text.

2. In the discussion of the applicability of the Gardner equation for long internal waves in the South China Sea (section 3.3) the authors provide estimates for the terms in the bracket of the Gardner equation (1). This discussion seems to be incomplete. It would be useful to add estimates (or at least a discussion) for (a) the nonlinear and dispersive terms in (1), (b) the fifth and nonlinear dispersive terms which appear in the derivation of the higher-order KdV equation, but are neglected in this study. The authors are asked to clarify these points.
Gardner's equation is derived under the assumption that the coefficient at its quadratic term may vanish and change sign. When this coefficient $\alpha = 0$, the cubic nonlinear term is the main nonlinear term and the equation, formally, transforms into the mKdV equation. When this coefficient tends to zero but does not vanish yet, one can write $\alpha = \varepsilon\chi$, where ε is a small parameter. The transformation $X = \sqrt{\varepsilon}x \; T = \varepsilon\sqrt{\varepsilon}t$ converts then the second order asymptotic equation

$$\frac{\partial\eta}{\partial t} + \alpha\eta\frac{\partial\eta}{\partial x} + \beta\frac{\partial^3\eta}{\partial x^3} + \varepsilon\left(\alpha_1\eta^2\frac{\partial\eta}{\partial x} + \gamma_1\eta\frac{\partial^3\eta}{\partial x^3} + \gamma_2\frac{\partial\eta}{\partial x}\frac{\partial^2\eta}{\partial x^2} + \beta_1\frac{\partial^5\eta}{\partial x^5}\right) = 0,$$ where the cubic term is in the second order, into the equation

$$\frac{\partial\eta}{\partial T} + \left(\chi\eta + \alpha_1\eta^2\right)\frac{\partial\eta}{\partial X} + \beta\frac{\partial^3\eta}{\partial X^3} + \varepsilon\left(\gamma_1\eta\frac{\partial^3\eta}{\partial X^3} + \gamma_2\frac{\partial\eta}{\partial X}\frac{\partial^2\eta}{\partial X^2}\right) + \varepsilon^2\beta_1\frac{\partial^5\eta}{\partial X^5} = 0.$$

The quadratic and cubic terms are in the same order in this framework and the other terms are in the next order.

As this procedure is widely used in the theory of weakly nonlinear waves, we do not feel necessary to comment it in the manuscript. A detailed discussion of this procedure and implications can be found in (Pelinovsky E.N., Slunyaev A.V., Polukhina O.E., Talipova T.G. Internal Solitary Waves. In: *Solitary Waves in Fluids* (ed. by R.Grimshaw), WIT Press, Southampton, Boston, 2007, 85–110). We included a reference to this source and tell now: "The case when both nonlinear terms are small is discussed in detail by Pelinovsky et al. (2007)."

3. On p. 3 it is stated that "This feature makes it possible to use these models to isolate and identify principally new features of the dynamics of internal waves even if some details of the system are not reproduced..." The authors are asked to expand this discussion and briefly describe the main advantages and disadvantages of using the weakly nonlinear models of this type, rather than just referring to the literature.

We add the following paragraph:
"For example, a new kind of quasi-steady nonlinear internal waves (so-called breathers) has been predicted using the framework of Gardner's equation. The possibility of generation of such phenomena by solitary waves of the second mode and the basic properties of its long-term propagation have been obtained in a numerical "wave tank" using Euler's equations (Lamb et al., 2007; Terletska et al., 2016). Several features of the process of generation of table-top solitary waves were also extracted based on Gardner's equation (Kurkina et al., 2016). The effect of a change in the polarity of solitary waves predicted by the asymptotic theory has been repeatedly observed in various areas including the South China Sea. It is however inevitable that many specific features and details (e.g. radiation of short waves, properties of strongly nonlinear disturbances or breaking of solitonic structures) cannot be reproduced using equations for weakly nonlinear waves and specific configurations of stratification may require the use of higher-order analysis and equations."

4. On p. 10 it is stated that "Gardner equation is not applicable in locations where the coefficients at the quadratic term vanishes and one has to employ a modified KdV equation..." This is not clear to me. Gardner equation becomes the mKdV in this case, so, what is meant here?
The text is changed as follows: "Gardner's equation transforms into the modified KdV equation in locations where the coefficient at the quadratic term vanishes and one has to use this equation in order to properly describe weakly nonlinear dynamics of internal waves in such regions."

Technical corrections

1. A footnote with the web link to GDEM database would be useful to readers.
We inserted it into the body text:
https://data.nodc.noaa.gov/cgi-bin/iso?id=gov.noaa.nodc:9600094

2. p. 1, "...solitons (solitary waves that interact elastically)" The comment in the bracket is not relevant in the context of this study, remove.
It is deleted.

3. p. 5, "... are invariant with respect to the particular choice of z..." is better replaced with "... do not depend on the particular choice of z*..."*
Yes, this is easier to understand for many readers

4. Figure 4, caption is unclear. Please, check.
Thank you; we reformulated the caption. Also, we unified description of the axes and labels for the two panels of Fig. 4.

5. The list of references is too long for the size of the paper.

We are again in an intricate position as other referees implicitly wished to see even more references. Due to the revision we had to add some references. However, we deleted several sources that are not particularly critical for this manuscript: (Ramp et al., 2004; Talipova et al., 1998; Talipova and Pelinovsky, 2013).