

Ocean Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/os-2021-29-RC1>, 2021
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Comment on os-2021-29

Anonymous Referee #1

Referee comment on "The inference of internal solitary waves in the northern South China Sea from data acquired by underwater gliders" by Wei Ma et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-29-RC1>, 2021

General comments

The authors present a method of detecting Internal Solitary Waves (ISWs) using unmanned underwater gliders. The glider flight data and hydrodynamic model are used to estimate the vertical velocity of the water, which is used, in conjunction with the temperature data recorded by the glider, to detect the presence of ISWs. This is confirmed by comparison with a coincident MODIS satellite image.

In principle, the method sounds promising. Unfortunately, the description of the method is inadequate. It would be impossible for anyone else to reproduce this method from the information presented here, and it is not clear how their method for estimating vertical water velocities from gliders differs from previous authors.

There is also a noticeable lack of discussion. No comparisons are made with previous studies of ISWs, or with previous studies detecting internal waves from gliders. There is no discussion of the wider applicability of their method outside the South China Sea (where ISWs are known to occur with very large amplitudes). There is virtually no discussion of the advantages of using gliders for this work over other measurement platforms apart from some extremely general remarks in the introduction. (For example, in the Introduction it is mentioned that surveys from research vessels are time consuming and costly, but there is no discussion later on of the scientific advantages or disadvantages of using gliders to observe ISWs instead of research vessel surveys.)

I also do not feel this article is a good fit for Ocean Science. Research articles are supposed to report "substantial and original scientific results", which this article does not do since it is almost entirely about the method. It could possibly be considered for a technical note but is perhaps too long. Perhaps a journal more focused on methods and technology, such as the Journal of Atmospheric and Oceanic Technology, would be a

better match?

Major concerns

Section 3: Estimation of vertical water velocities

Correctly estimating the vertical velocity of the water is vital for this work. Unfortunately, section 3 is not well written, to the extent that I am unable to determine if the results presented here are valid or not. The main problem is that the authors have simply left out significant parts of their methods. It is always difficult, when building on the work of previous authors, to strike a balance between not repeating too much of what has been published previously and yet still creating an article which is reasonably comprehensible on its own. However, the authors have gone too far towards not repeating material published previously. I suggest the authors compare with the article by Fer et al., 2014, JAOT. Their section 4(a) describes the glider flight model, including the basic kinematic equations and definitions of every parameter. Fer et al. explain which parameters are simply given optimized values from previous published work, which are calculated from the glider's data, and which parameters are optimized for their dataset and which cost function is used to do so (this is very important, there are several possible cost functions). This whole section is obviously very heavily based on the previously published work of Merckelbach et al., 2010, but Fer et al. strike a good balance between not repeating too much of Merckelbach's work while still including enough that the method is comprehensible without reading Merckelbach's paper. I suggest the current authors rewrite the first part of their section 3 along the same lines.

The next part of section 3 concerns the sensitivity analysis of the various flight model parameters. However, the authors do not explain where the 'original values' of each parameter come from, nor do they explain how the effects of these parameters on the mean theoretical vertical velocity are calculated, or how they calculate the local sensitivities of these parameters. Once again it is impossible to follow their method. On line 142 they refer to an equation 5 when only one equation has been presented at this point in the paper (possibly this is meant to refer to Equation 5 in a 2018 paper by Ma et al., but if the equation is important enough that it is necessary to refer to it specifically, then it's important enough to be reproduced in the current paper).

Importantly, it is not clear what *use* the authors make of this sensitivity analysis, other than to decide which flight model parameters need to be optimised. The list of parameters they decide need to be optimised are the volume, lift and drag, and compressibility. The first three are commonly optimised by glider users, and the last is obviously going to be significant in gliders with air bubbles, such as the authors mention on lines 172-175. Hence it is not clear how their method for estimating vertical water velocities from gliders differs from previous authors.

Lines 130-131 say “the data acquired in the stable upward motion, where the roll is zero and the pitch is constant, are input into the glider model”. Does this mean that only data from glider ascents were used to tune the flight model parameters, not descents? What if the drag coefficients were not the same for ascents and descents? Is it a feature of Petrel-II gliders that they do not roll or pitch during ascents? Or something specifically set by the pilots? This needs explanation because other types of glider do roll and make small pitch adjustments during ascents. And if Petrel-II gliders do not pitch during ascents, what effect does this have on the flight (particularly the angle of attack) when encountering changes in water density?

Lines 188-190: “The glider vertical velocity w_p relative to water velocity is obtained by the time rate of change of pressure measured by the CTD, but those signals contain noises, or even glitches. Due to the excellent time-frequency characteristic, the wavelet transform is applied to restrain those noises.” This requires a great deal more explanation. It is not common for glider pressure sensors to have glitches. What type of glitches and how severe were they? How did the authors determine that these were glitches? What wavelet transformation is applied, exactly, and what effect does it have on the calculated values of w_p ? This could be crucial for the final values of w_c .

Towards the end of section 3, the authors give mean vertical water velocities for three-day periods of 2.22mm/s, but do not feel the need to discuss these unusually large values. Most estimates of vertical water velocity suggest values around 10^{-5} m/s in the upper ocean (e.g., Liang et al., 2017, JGR Oceans). It is not stated whether these large velocities were upwelling or downwelling, nor is there any discussion of why these region has such large values. I did find myself wondering if these were speeds rather than velocities, which would still be very large but not quite so unusual.

The authors also seem unconcerned that the estimates of vertical water velocities from descents and ascents differ by 2.9mm/s on average. Again, this difference is two orders of magnitude greater than most estimates of vertical water velocity. This raises questions about the effectiveness of their cost function, and/or whether they should use a different cost function to minimise differences between descents and ascents. But since the authors do not state which cost function they use it is hard to understand how such a large difference was considered acceptable. I feel the authors need to devote some effort towards convincing the reader that their results are still valid even though the difference between vertical water velocities estimated from dives and from climbs is two orders of magnitude greater than previous estimates of vertical water velocity.

Section 4

Under the assumption that the authors' methods are valid (despite the lack of clarity in section 3), section 4 presents some fairly convincing results that the authors have successfully detected ISWs using gliders. However, this was in a region where ISWs are known to occur and are known to have extremely large amplitudes. What about in the rest of the ocean? The introduction begins by saying that ISWs can propagate over thousands of kilometres. I don't think the authors have demonstrated the wider applicability of this method in regions where the ISW amplitudes may be smaller than in the SCS. Hence lines 292-300 are overstating the significance of their results.

Similarly lines 287-288 seem to be suggesting that moorings would do a better job than gliders of observing ISWs since they can observe the thermohaline structure at multiple depths simultaneously. Lines 284-291 suggest that using the vertical water velocities as well as the thermohaline data gives you a better chance of detecting ISWs using gliders, but no comparison is made with ship CTD-ADCP surveys. Overall section 4 is a little unconvincing of the advantages of using gliders. (This overlaps with my comment below about section 5.)

Section 5

The Summary and Discussion section is in fact all summary, and would benefit from some discussion. In the introduction the authors mention several other studies of internal waves/tides from gliders, and thus section 5 could contain some comparison with these previous studies. Similarly the authors could compare their results with other studies observing ISWs in the SCS from other observational platforms. I think they also need to discuss the advantages of using gliders over other platforms that could be used to detect ISWs - and try to find advantages in terms of the quality of science that can be done, not just in terms of what is more costly and time consuming. Otherwise it is not clear what the advantages of this new method are.

Quality of written English

The article would benefit from professional proofreading, as there are numerous grammatical errors, incorrect usage of words, and examples of awkward sentence construction which make the paper considerably less readable. It would also be helpful to focus on using plain English - the meaning is sometimes obscured by the use of unnecessarily elaborate language. For example, line 195 reads "The tendency of theoretical velocity (w_g) is coincident with that of vertical velocity (w_p)", which I think just means " w_g is broadly similar to w_p ". I have not noted all such instances of clunky English because that would have meant rewriting nearly every sentence in the article.

Inappropriate colour scales

Figures 8 and 10 use a "rainbow" colour scale, which is not appropriate. It has been widely documented for at least 20 years that rainbow colour scales are not perceptually uniform and can therefore distort interpretation of data and lead to incorrect conclusions. Moreover, figures using rainbow colour scales are not accessible for people with colour blindness, which affects approximately 7% of men and a smaller proportion of women. Use of rainbow scales is therefore discriminatory. See, for example:

https://tos.org/oceanography/assets/docs/29-3_thyng.pdf

<https://www.climate-lab-book.ac.uk/2016/why-rainbow-colour-scales-can-be-misleading/>

<https://blogs.egu.eu/divisions/gd/2017/08/23/the-rainbow-colour-map/>

Figure 8(c) and figure 10 also suffer from having colour bars which do not match the figure. The figure panels show temperature with a colour change every 1 degree, but the colour bars show continuously changing colour. It is important that the colour bars are an exact match to the figures so that readers can easily match colours on the figure to colours in the colour bar.

Minor points that apply throughout the paper

Units: please read the guidance for authors for this journal and correct your use of notation throughout - e.g., m/s should be written m s^{-1} . It is not necessary to include a dot between the m and the s^{-1} . Also it is conventional to leave a space between the number and the unit, e.g., "1000 m" not "1000m".

Figures: font sizes are too small and hard to read. I had to zoom in to 200% size to read them.

References in text: All references should be referring to the authors. For example, line 114 should read "derived by Ma et al., (2018)" not "derived in Ma et al., (2018)". The latter suggests some very unusual internal surgery done inside the unfortunate Ma!

Use of acronyms and symbols throughout: once you have defined a symbol, you should then always use it. For example, once w_g has been defined you do not need to keep saying "the theoretical vertical velocity (w_g) ...", just say " w_g ". Similarly for acronyms.

Other comments

Line 24: "The availability of this method" should read "The effectiveness of this method".

Lines 43-44: glide speed and horizontal distance travelled are rarely constant and will be affected by ocean conditions and choices made by the glider pilots. Consider giving ranges, or at least averages.

Line 48: remove "at temporal scales" and "on spatial scales".

Line 84: could you say a little more about why these gliders perform "best" for profiles deeper than 600 m - and, indeed, in what manner their performance improves for deeper

profiles?

Line 90-1: What does "According to marine meteorological conditions" mean here? And in what sense did they conduct operations "cooperatively"? Do you just mean "simultaneously"? In fact, could this paragraph from line 89 to line 92 be rewritten as: "ISWs occur more frequently from April to August (Zheng et al., 2007). Since we aimed to take observations during a period of high ISW occurrence, the four Petrel-II gliders took simultaneous observations in August (2017). They were deployed to the northeast of Dongsha Atoll and then proceeded southwest and back, along trajectories shown in Fig. 1(b)."

Figure 1(b): the caption needs to explain the whole figure, not just the trajectories - it should mention the bathymetry and that areas mentioned in the text are labelled. The colour bar label should read "Depth (m)" not "Depth/m" - the latter implies you are dividing the depth by metres, which makes no sense.

Lines 100-101: give the expected accuracy of the GPCTD (can normally be found on the manufacturer's data sheets). Also state what pressure sensors are mounted on the gliders, and give the expected accuracy of the pressure sensors.

Line 103: "spatial" should be "horizontal".

Table 1 caption: "Glider observations in August 2017. The distance refers to the total horizontal distance travelled over the entire deployment." You don't need to say 'several gliders' in the caption because there's obviously several gliders in the table.

In lines 145 - 147 the authors refer to the sensitivity of structural parameters to mean theoretical vertical velocity, and then quote numbers which are obviously the sensitivity of mean theoretical vertical velocity to the parameters. Yes, these are simply the inverse of each other, but why not make the terminology consistent with the quoted numbers? Similarly in lines 152-153. The use of the symbol delta seems inconsistent between figure 2 and the text - and certainly it is unnecessarily confusing. Why not just say "-0.004 cm s⁻¹ per percent change in epsilon", for example? (Line 147).

Line 161: describe the specific cost function used - as mentioned earlier, there's more than one cost function which has even been applied to glider flight models.

Line 163-164: Figure 4 actually shows the parameters against date, not dive number.

Line 169: "The lift coefficient C_{L0} does not change enough to cause a significant change in w_g ." What matters is not how much all these parameters change relative to each other (especially since their apparent significance is entirely dependent on the y-axis scales you happen to have chosen for figure 4), but how much they would change w_g . I suggest you do some simple sensitivity calculations to illustrate this for the reader, something along the lines of: "the mean w_g for the ascent of dive N is M, if using the correct C_{D0} for dive N, but would be calculated as P if using the C_{D0} value from the start of the deployment." Dive N would be towards the end of the deployment.

Lines 179-184: start by explaining what a depth-keeping experiment is - remember not all your readers will be familiar with glider capabilities. State the dive number and date so people can see when this was in figure 4. State the values of the original and corrected V_g and gamma. The glider will maintain a constant depth when its density is equal to the density of the surrounding water, which is equivalent to its buoyancy being equal to its weight (not gravity). If you have not previously done so in your rewrite of the early part of section 3, give the equations which allow you to calculate the glider's theoretical density for comparison with the water density. Then figure 6 will make sense. You should also explore how much of the improvement is due to the correction of V_g and how much is due to the correction of gamma, rather than just showing the combined effect.

Line 188: w_p is not relative to the water, w_g is.

Lines 189-90: Does "excellent time-frequency characteristic" mean "high sampling frequency"?

Figure 7: The x-axes of panel a and panel b are not aligned. Panel b contains slightly more than 4 dives, panel a contains nearly 6 dives. In panel a, you do not need two y-axes because they're identical. Just label the axis as 'velocity ($m s^{-1}$)' and then the legend explains which is which. The caption needs to explain panel a and panel b separately. The caption for panel b needs to explain what is meant by 'shift', since this is mentioned nowhere in the text (I think you mean battery position along the long axis of the glider, relative to the central position?), and that theta is pitch. The caption should also state that this is a representative 12 hour period, and you should make sure the main text reflects this - e.g., line 191 should say something like "a 12-hour period, representative of the entire dataset, is shown in Fig 7" not "as shown in Fig. 7". The second sentence in the caption: "Given the unsteady motion during the eccentric battery pack shifting or rotating, and variable buoyancy engine working, the velocities in those moment are excluded" - this needs to go in the main text with more detail. Are these times excluded while you're optimising the flight model parameters, or while you're examining the resultant water velocities for ISWs, or both? Do you just exclude the exact times when the glider is pumping/pitching/rolling or do you also exclude a little more data after those times to allow the flight to stabilise? The glider will not respond instantly to changes in battery position.

Lines 195-203: You say Merckelbach et al. proposed a robust approach to estimate vertical water velocities, but you've just spent the last 4 pages telling us how you estimate

the vertical water velocities. Do you mean Merckelbach et al. proposed a method to estimate the *errors* on vertical water velocities? If yes, you still need to summarise their method here, because at the moment there is no explanation for the stated inaccuracy of 4mm/s.

Line 199: What is the significance of mean water velocities for 3-day periods? Why 3 days and not a mean over the whole time series?

Throughout Section 4, you appear to be using profile number for dive number, in both the text and the figures. If you wish to refer to each profile separately, rather than referring to "Profile-47 in the diving phase", which gets rather clumsy, then simply explain that you are numbering them profile by profile. So, for example, the descent of dive 1 becomes profile 1, the ascent of dive 1 becomes profile 2, the descent of dive 2 becomes profile 3, etc. Thus you can use 'dive' when you mean the descent and the ascent together, and 'profile' for the descents and ascents separately.

Also throughout section 4, it is common to use the Greek letter sigma for standard deviation, rather than "std".

Lines 205-206: Remove the first two sentences of this paragraph, they simply repeat information presented in section 3.

Line 207: Give a more complete explanation of your chosen depth range, e.g., "Depths above 50 m are excluded because ..., depths below 500 m are excluded because ...". Also remove the word "therefore".

Figure 8: do not use the same colour scale for two different properties in the same figure. In other words, potential temperature should not use the same colour scale as sigma. On panel c, I suggest you change the axes tickmarks to come out from the axis instead of in, because they're completely invisible at the moment. The x-axis label would be better as "day in August 2017 (UTC)".

Figure 8 caption: the caption needs to describe the whole figure, not just the aspects to which you wish to draw the readers' attention. So the caption for panel a needs to explain that the greyscale and contour lines are bathymetry, the black square encloses the area shown in panel b, the pink dot-dash lines denote the observing time at intervals of 1 day (this should not be in the main text)- as well as the colours representing sigma for each dive between 4th and 12th August. For panel b, the caption should simply say 'as panel a, zoomed in on the area enclosed in the black square in panel a'. Avoid words such as 'dramatic', this is unscientific. Panel c's caption needs to explain the x-axis ticklabels a little more - e.g., is the tick for 6th August at midnight at the beginning of the 6th, or at noon on the 6th, or at midnight at the end of the 6th? All are possible.

Line 217: do you mean sigma increases to the east or to the west? And what about the obvious increase to the south?

Lines 223-225: "sigma increases to 5.06cm/s (20°54.08'N, 117°49.23'E, dive NN), 4.83cm/s (20°50.02'N, 117°46.67'E, dive NN), and 4.22cm/s (20°39.48'N, 117°40.42'E, dive NN), and these values are considerably larger than the average."

Line 229: a velocity cannot be 'sharp'.

Line 237-8: "further analyze the profiles with unusually large sigma (Fig 8(b))."

Figure 9 caption: "Depth, w_p and w_c measured by Glider-08 on the 9th and 10th of August." Figure 11's caption should be altered similarly.

Figure 9, profiles 46-48 is labelled as commencing at 6pm on August 9th. Figure 11, profiles 61 to 65, is labelled as commencing just before midnight on August 9th. This seems inconsistent.

Lines 245-6: "The strong upwelling forces the glider to change its predefined movement direction from downward to upward at 22:20 UTC." This reads as though the glider is somehow forced to pitch nose up and pump oil to the external reservoir to try to fly upwards - which of course is not what you mean. I suggest "The strong upwelling around 22:20 UTC induces a temporary reversal in w_p from downward to upward."

Figure 10 caption: "Potential temperature recorded during profiles 45-65 (Glider-08)." What are the white triangles along the upper x-axis? As with the day numbers in figure 8(c), you need to explain whether the x-axis tickmarks refer to the start, middle or end of that dive/profile.

Figure 10 compared with lines 252-3: on figure 10 it looks very much as though the downward displacement of isotherms occurs during dive/profile 46, not 47.

Line 257: "making the thermocline sunk to nearly 300m depth." You have not defined a specific thermocline. On the figure it appears to be the 13 degree isotherm that sinks to nearly 300 m? Assuming the x-axis tickmarks refer to the start of each profile, there seem to be depressed isotherms between approximately 170 - 350 m during profile 60,

and it's only the 11 degree isotherm that is depressed during profile 61.

Lines 270-279: if ISWs can be detected from satellite images, why do we need to use gliders at all? What extra information is gained by using gliders? (This might actually be something to include in the summary and discussion section rather than here.)

Data availability: it is stated that the data from the field experiments is only available on request from the authors, with no detailed explanation of why the data is not available from a public data repository. This does not seem adequate to meet Ocean Science's data availability requirements.

References

Fer, I., A.K. Peterson, J.E. Ullgren, 2014, Microstructure Measurements from an Underwater Glider in the Turbulent Faroe Bank Channel Overflow, *Journal of Atmospheric and Oceanic Technology*.

Liang, X., M. Spall, C. Wunsch, 2017, Global Ocean Vertical Velocity From a Dynamically Consistent Ocean State Estimate, *JGR Oceans*.