General reply

We are very grateful to the constructive comments from the reviewers and Prof Woodwoorth and we thank them and Dr Williams as editor for their efforts. Our replies are included below in bold. We hope they are satisfactory and that the paper is now suitable for publication in OS.

RC2

Overview: In the manuscript titled "Bardsey – an island in a strong tidal stream", Green and Pugh compare tidal constituents based off of in-situ pressure measurements with constituents derived from a satellite data product. They find that the resolution of the satellite data product is insufficient to accurately describe tidal variations in a small-scale tidal strait. As a result, estimates of tidal dissipation based on the satellite product are biased low. The use of satellite-ocean color measurements to describe the vortex shedding caused by the strong tidal currents in the tidal stream is explored.

Evaluation:

Overall, this is an interesting topic and the influence of small scale bathymetry is probably worth bringing to the attention of global modelers and satellite altimetry users. However, the manuscript and the analysis can be improved. For example, the discussion of turbulence and dispersion is vague and can be misinterpreted (see below). The overall framing and importance of the paper can be improved, for example by more specifically discussing why satellite-based or global model based estimates of dissipation matter (see other comments below). Also, a more in depth analysis of the tides is warranted.

- **We thank the reviewer for these suggestions, and we have endeavoured to better motivate the study and explain why dissipation matters in the revised manuscript. The tidal analysis is now extended as well.**

What are the error statistics on the tidal fit (e.g., RSME) and the uncertainty bounds on the constituents?

- **These have been added to the paper. The measurements are accurate to fractions of a cm, and we have added this to the text.**

Did you correct for atmospheric pressure in your in-situ measurements, and does that make a difference (given that a hurricane occurred, maybe it does)?

- **No, because it doesn't matter for tides; the harmonic analysis is frequency specific and will ignore the storm effects; we have added "**The effects of the storm were not noticeable in the tidal signals, as they were at very different natural frequencies. The subsurface pressure measurements at Bardsey include atmospheric pressure variations, and any tidal variation therein. However, at these latitudes the atmospheric pressure S2 variations are very small. At the equator the atmospheric S2 has an amplitude of about 1.25 mb, which decreases away from the equator as 〖cos〗^3 (latitude) , so at 530 N the amplitude is reduced to 0.26 mb, a sea level equivalent of 2.5 mm. In Table 2 the three constituents listed are the two biggest, M2 and S2 , and (as an indicator of the presence of shallow water tides) M4, the first harmonic of M2. These shallow water effects are enhanced around the island because of curvature on the directions of current flow."

Can tidal statistics derived from 1 month of data be accurately compared to a satellite-based estimate that is obtained from years of sparse data, particularly M4? Perhaps it can, but given the conclusions of the paper this should be explored as an alternate hypothesis for why in-situ and satellite measurements do not agree. Similarly, are there other reasons why a satellite-based estimate may not work well at the coast, beyond resolution?

- **We argue that it can. After all, the observations really show what the tide was doing, to a very high accuracy. They don't agree, so if you had used TPXO9 to estimate the tides for the**

area you would miss out on about NN% of the signal. Land interferes with the actual data return from the satellite because their footprint is quite large. But the altimetry data is assimilated into a numerical model solution, so we feel that the resolution of that model – 2 nautical miles at best – is certainly a large source of error. The following paragraph is now in the text to justify this: "Amplitudes and phases of tidal constituents based on short periods of observations need adjusting to reflect the long term values of amplitudes and phases. The values in Table 2 have been adjusted for both nodal effects and for an observed non-astronomical seasonal modulation of M2. Standard harmonic analyses include an automatic adjustment to amplitudes and phases of lunar components to allow for the full 3.7%, 18.6 year modulation due to the regression of lunar nodes. However, the full 3.7% nodal modulation is generally significantly reduced in shallow water and shelf seas, so locaL counter adjustments are needed. The nodal M2 amplitude modulation at Holyhead, the nearest standard port, is reduced to 1.8% (Woodworth et al., 1991). We have used this value in correcting the standard 3.7% adjustment. The M4 nodal modulations are twice that for M2. The seasonal M2 modulations are generally observed to have regional coherence, so we have used the seasonal modulations from 9 years of Newlyn data (in the period 2000-2011). M4 is not seasonally adjusted, and S2 is not a lunar term, so is not modulated nodally. These very precise adjustments are possible and useful, but overall as stated in the caption to Table 2, for regional comparisons we assume, slightly conservatively, confidence ranges of 1% for amplitudes and 1.0 degrees for phases. **"**

The discussion of tidal velocities—and the comparison to a value in the Admiralty chart—is rather vague.

- **This has been rewritten – see reply to RC 1.**

Surely there must be other measurements (e.g., ADCP measurements) or papers, either in reports or the scientific literature? Maybe not, but it is not clear that an exhaustive search has been made to find such values. Similarly, would suggest that authors check that the tidal phase velocity really is sqrt(gh), given their method of estimating dissipation.

- **Not that we are aware of, and we did shop around. A comment has been added about the phase relationship.**

The use of Landsat is quite qualitative, and could be improved by providing more details and examining many more images (it is not clear whether the figures shown are representative, or just a lucky coincidence).

Yes, it is because we brought these into illustrate to wider implications, not to make a full analysis of the wakes. We have clarified in the text that these are the only images during the measurement periods where Bardsey is visible. This is the reason for why the discussion is brief. Also, focus of the paper is really the effect on the tidal stream itself, rather than downstream effects. This has been clarified: "Landsat-8 data images were used to identify possible eddies in the currents and further illustrate unresolved effects due to the island. Note that we are not aiming for a full wake description in this paper. … and the two images were the only cloud-free ones during the measurement periods that were on different stages of the tide.**"**

Finally, the manuscript is still a bit 'rough'—in many places, the writing and development of the argument could be made more succinct or focused (see comments below). In addition, the literature reviewed/discussed could/should be expanded (see suggestions below).

- **The text has been reworked based on the comments from all reviewers.**

Specific Comments

Line 14—"some 3 km wide, it is surrounded"—run on sentence. Split into two sentences?

- **Done**

Line 20 "seriously under-represents" is a bit colloquial and vague. Can one be quantitative? "Seriously" is also used later—would suggest rephrasing, here and elsewhere.

- **Done throughout.**

Line 23 "at the mainland than at the island"—do you mean near the mainland and near the Island?

- **No, we mean the observations, taken very close the coast, and don't think this warrants rewriting.**

Line 31 "several tidal constituents"—How many, and which ones? Would be good to be specific.

- **Good point, this now reads** . Following the advent of satellite observations, up to 15 tidal constituents have been mapped using altimetry constrained databases…" **We see little point in listing the 15 constituents – they are provided on the TPXO9 webpage.**

Line 34-37—In terms of satellite data analysis, my understanding is that coastal regions have more error. Some of the products out of JPL are specifically tuned to coastal conditions. Perhaps you can comment on some of the near-coast altimetry issues, with references?

- **Comment added (see above as well): "**However, new correction algorithms improve the satellite data near coasts (e.g., Piccioni et al., 2018), but this is yet to be included in global tidal products.**"**

General comment: At some point (Introduction? Conclusion?) might be good to mention the new SWOT mission, which has much higher resolution and might make the issues described here obsolete. If so, what lessons might still be used for global tide models (and other global models)? Or, phrased differently, if global models are not modeling coastal dissipation correctly, how are they (incorrectly) compensating for that in calibration, and what might be the consequences of that?
Fair comment, and this has been added to the discussion: "Future satellite mission may be able to resolve small islands like Bardsey, and improved methods will allow for better detection of the coastlines. In order to obtain tidal currents, however, one still has to assimilate the altimetry data into a numerical model and it will probably be some time before we can simulate global ocean tides at a resolution good enough to resolve an island like Bardsey. **". As for the dissipation: it will probably be slightly overestimated in the offshore cells and underestimated closer to the coast, so the regional total is most likely accurate. This has not been added to the paper, however.**

Line 45—"Rocky mélange"—is this a technical term? Have never seen mélange used outside of novels, but then again I'm not a geologist.

- **It is a correct geological term describing a large-scale breccia. No changes have been made.**

Line 49—Awkward phrasing ("and the separating Sound")

- **Rewritten: "…**island and the Sound means…**"**

Line 50 "this will lead to effects induced"—what kind of effects? Would be good to be specific

- **Clarified: "**This will lead to the effects of the island on the tidal stream, e.g., flow acceleration/blocking and wake effects, being missed in those products.**"**

Line 51 would avoid the use of "very". Also, commas would be good here, as in "uncaptured (by TPXO), active, local tidal"

- **Amended: "**The uncaptured, by the altimetry constrained data, active local tidal dynamics**"**

Line 54 “We will do a direct comparison of tidal amplitudes around the island” What kind of comparison? Using what methods? A bit more specificity would be helpful.

- **Yes, this is vague and now reads “**We will make a direct comparison of the tidal amplitudes and phases measured by the bottom pressure gauges around the island (see Figure 1b for tide gauge (TG) locations and a summary of the *in situ* tides). We also consider whether, and when, in the tidal cycle, flow separation occurs in the wake of the island.**”**

Line 60—what are the units on your kinematic viscosity, which equals dynamic viscosity divided by density? Usually this is on the order of magnitude 0.000001 m^2/s, not 100 as mentioned here. Or is “100” a dispersion coefficient? In that case, would seem to be incorrect to call this a kinematic viscosity, in my opinion (even if units are the same). If you are using a diffusion (dispersion) coefficient, which is often based off of a Reynolds number decomposition/gradient diffusion assumption, would also not call this a Reynolds number. Perhaps there is some modifier one can put in front of “Reynolds number”, to distinguish it from the usual one. Similarly, wouldn’t say this ratio is measuring a transition to turbulence. The flow is turbulent down to a scale of about 1mm (per inertial cascade, to Kolmogorov number). Though I’m not familiar with this “Reynolds number” literature, would assume that this ratio gives some indication of the likelihood of forming large, quasi-2d vortices (what you are calling ‘turbulence’) vs. having those vortices broken up by dispersive processes (turbulence, shear dispersion, chaotic dispersion….).

- **This is a fair comment. Our value is actually an effective horizontal diffusivity, and 100 is a reasonable value for that. The text has been updated to state this, but we still have not changed the name of the Re parameter in line with the cited literature. The paragraph now reads “**We will use some basic fluid-flow parameters. Transition to turbulence can be parameterised in terms of the Reynolds number, Re, defined as Re = UD/ν, where U is a velocity scale, D is the size of the object, and $\nu \sim 100$ is a horizontal diffusivity (see, e.g., Wolanski et al., 1984 for details). It indicates when there is a transition to flow separation behind the island: at low Reynolds numbers, Re<1, the flow is quite symmetric upstream and downstream, and there is no flow separation at the object. As the Reynolds number is increased to the range 10 < Re <40, laminar separation happens and results in two steady vortices downstream. As Re increases further, up to Re<1000, these steady vortices are replaced by a periodic von Karman vortex street, whereas if Re>1000, there is a fully separated turbulent flow (Kundu and Cohen, 2002). **”**

Would note that 2D turbulence is much different than 3D turbulence. The implicit assumption you seem to be making is that once the eddies are formed, they are turbulent. Is this strictly speaking correct? The aspect ratio (horizontal to vertical) of these eddies must be very large, where-as in well-developed turbulence energy should be distributed evenly in x,y, and z (not possible due to continuity in a large eddy in a shallow sea). What is the aspect ratio? Might be good to explore and discuss somewhere, and whether it has any implications for the results. How is the evolution of a 2D eddy different from a 3D eddy? How might bottom friction (or sidewall friction) impact the eddy and make it only quasi 2D? In 3D turbulence, there is a cascade of turbulence from large to small scale. In 2D turbulence, that is not the case—energy transfer goes from small to larger scale (e.g., as when small vortices combine to create a larger one). This is not a paper designed to look at such turbulence issues. However, would be good to be more careful in how turbulence is discussed.

- **The aspect ratio is ~10-100, assuming these reach the seafloor (and there is no reason to assume that they shouldn’t in a strong barotropic unstratified flow). The eddies act as a sink of tidal energy, which is what we are interested in, and their exact nature is maybe not entirely relevant to this paper. Consequently, we feel that discussing the eddies further will not add to the paper. We note that they are there, and that they complicate the picture of the flow around the island in a way that may not be captured in numerical models, as we already discuss.**

Introduction, general: It would be good to briefly review that these small scale 'straits' such as the one being studied are ubiquitous, to frame the larger importance. Angelsey Island in Wales is a nearby example, perhaps. All over the world, there are many Island archipelagos, and some have strong currents such as mentioned here. For example, there is the Greek legend of Charybdis , maybe related to currents through the Strait of Messina (Sicily); see https://en.wikipedia.org/wiki/Strait_of_Messina. In Puget Sound, there is Deception Pass (https://en.wikipedia.org/wiki/Deception_Pass). Within San Francisco Bay, there is Raccoon Strait. Between New York Harbor and Long Island Sound, there is Hells Gate. There are surely many other examples in the world, and some of them may have been studied or at least have references to large currents and whirlpools. Including some information on or review of them may help frame the broader significance of this study.

- **Good point, although Anglesey is certainly resolved in the databases we discuss. Also, we do provide a general overview in the introduction that states that any small island will be unresolved, and we are a bit reluctant to bring in further examples, especially since (to our knowledge) this is the first time this type of study – a direct comparison of the effect of a small island compared to altimetry constrained products - has been conducted.**

Introduction, general: A brief review of diffusion and dispersion might help frame the "viscosity" you use in your "Reynolds number" (assuming my interpretation above is correct). What is shear dispersion, and is it potentially important here (see for example the book by Fischer et al, from 1979)? What is chaotic dispersion, and is it important here (see Zimmerman, 1986, and de Swart et al., 1997)? How can a jet or plume cause horizontal dispersion (e.g., Fong & Stacey, 2003)? What is turbulent diffusion, and is it important here (usually, it's smaller than shear dispersion caused by lateral velocity gradients, but it also depends on the time scale you are considering—shear dispersion becomes effective at larger time scales than turbulent eddy viscosity (and so on).

- **Another fair point, albeit see our response above. The aim of the paper is to investigate how the island change the tides, particularly the tidal stream, and discuss wider implications of those changes. The wake effects were added as an example of an effect that may not necessarily be considered in a modelling or altimetry constrained investigation, but it is not the aim to discuss those in detail, and what has been suggested is most likely not important for the tide but a consequence of the tide. Consequently, we opt to not add any more literature.**

Introduction, general comment 3: You could also review the "shallow turbulence" literature, which seems like it might be relevant here. Uijttewaal & Booij, 2000 and Uijttewaal & Jirka, 2003 discuss a "shear stability parameter". Uijttewaal & Booij, 2000 find that eddies produced by lateral shear (du/dy) become increasingly suppressed by bottom boundary layer turbulence as depth decreases. They find that the growth of lateral shear-induced eddies is limited when their shear-stability is greater than approximately 0.1. Again, it should be noted that 2D turbulence is quite different than 3D turbuluence. This generally it isn't much considered in shallow coastal waters, or at least I haven't come across it very much. But maybe there is some more literature since I last thought about it.

- **See comment above. We agree that 2D and 3D turbulence are very different, but it is again not the point of this paper to discuss that.**

Line 89—Did you adjust your pressure measurements for atmospheric pressure variations? If you didn't, would probably be a good idea to do so, just to be complete and make sure that it doesn't significantly alter your analysis. This is particularly true in your "phase 2" result, in which there was a hurricane.

- **No, because it doesn't matter for tides; the harmonic analysis is frequency specific and will ignore the storm effects; we have added "**The effects of the storm were not noticeable in the tidal signals, as they were at very different natural frequencies. The subsurface pressure measurements at Bardsey include atmospheric pressure variations, and any tidal variation therein. However, at these latitudes the atmospheric pressure S2 variations are very small. At the equator the atmospheric S2 has an amplitude of about 1.25 mb, which decreases away from the equator as 〖cos〗^3 (latitude) , so at 530 N the amplitude is reduced to 0.26 mb, a sea level equivalent of 2.5 mm. In Table 2 the three constituents listed are the two biggest, M2 and S2 , and (as an indicator of the presence of shallow water tides) M4, the first harmonic of M2. These shallow water effects are enhanced around the island because of curvature on the directions of current flow."

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Line 91 "were subjected to harmonic analysis"—sounds like something unpleasant. Maybe rephrase, e.g., "were harmonically analyzed"?

- **Disagree, this is a quite standard phrasing in the tidal literature and we have kept our original phrasing.**

Line 96: "residuals have standard deviations appropriate for the region"—this is vague. Maybe be specific, and compare it to the nearest tide gauge from the same period.

- **True, rectified: "**The non-tidal residuals, the final column in Table 1, compare well with the residuals at Holyhead, the nearest permanent tide gauge station some 70 km north; for Holyhead these were 0.096 m, 0.172 m, and 0.067 m for the same periods (note that bottom pressure measurements at Bardsey include a partial natural sea level compensation for the inverted barometer effect).**"**

Line 99—" consistency in the tidal ages" --it might be good to be more specific and define what is meant by 'tidal age', since not all are familiar with this terminology. Is discussion of tidal age needed? Some more specificity on what is considered a good fit would help. Is a good time variation 10 minutes? 1 hour?

True again, we now define tidal age and we are more specific in the quantification. "The tidal age is the time after maximum astronomical tidal forcing and the local maximum spring tides, or approximately the phase difference between the phases of S_2 and M_2 in hours,…**"**

Line 109—Does the TPX09 product use the best altimetry product for near coastal areas? Again, I think JPL has a coastal data product. Would constituents based off of a coastal data product provide better answers? One of the main conclusions in the paper is that satellite data have issues in small scale regions. Is this true of all data products, or just the one used to create the constituent atlas? Another way of putting this—are there other issues, besides resolution, that impact coastal constituents and therefore your comparisons?

- **Fair question, and there are corrections recently developed that can do a better job at the coast. However, the underpinning numerical model can still not be run globally at enough resolution and the point in the paper is that the global databases – FES and TPXO – are used a lot for tidal work and we want to highlight the issues that may lead to. But, since there are other products that may be better, we have rewritten the introduction: "**Scientific understanding of global tidal dynamics is well established. Following the advent of satellite observations, up to 15 tidal constituents have been mapped using altimetry constrained numerical models, and the resulting products verified and constrained further using in situ tidal data – see Stammer et al. (2014) for details. There is, however, still an issue in terms of spatial resolution of the altimetry constrained products: even the most recent (global) tidal models have only 1/30o resolution (equivalent to ~3.2 km in longitude at the equator, some 1.9 km in the domain here, and 3.2 km in latitude everywhere). The satellite themselves may have track separation of 100s of km (Egbert and Erofeeva, 2002) and the coastline can introduce biases in the altimetry data. This means that smaller topographic features and islands are unresolved, and may be "invisible" in altimetry constrained product even if the features may be resolved in the latest bathymetry databases, e.g., the General Bathymetric Chart of the Oceans (https://www.gebco.net/). This can mean that the energetics in the products, and in other numerical model with insufficient resolution, can be biased because the wakes can act as a large energy sink (McCabe et al., 2006; Stigebrandt, 1980; Warner and MacCready, 2014). Whilst the globally integrated energetics of these models is consistent with astronomical estimates from lunar recession rates (Bills and Ray, 1999; Egbert and Ray, 2001), the local estimates can be wrong. However, new correction algorithms improve the satellite data near coasts (e.g., Piccioni et al., 2018), but this is yet to be included in global tidal products.**"**

General comment: Would be good to establish somewhere what the typical tidal range in this region is, and that diurnal tidal components are small. This will help justify the use of only 4 constituents. (Also, is the use of M4 important? Would be good to establish that quarterdiurnals are important here (or are they)?

- **Good comment, Section 3.1 now opens woith "**A spring-neap cycle of parts of the data from the East and West gauges in Phase 1 is plotted in Figure 2 and show a tidal range surpassing 4 m at spring tide. Note that the diurnal constituents are not discussed further due to their small (<0.1 m) amplitudes. The TG data show M2 amplitudes of 1.210 m (North), 1.347 m (East) and 1.139 m (West, see Table 2).**"**

  Quarterdiurnals are important and included in Table 2.

Line 114—Would define "Highest and Lowest Astronomical tide" (HAT and LAT), before stating that M2+N2 + S2 +M4 are a limited form. Also, strictly speaking, M2 and M4 are phase locked, i.e., 2*phaseM2 – phase_M4 = constant (see e.g., Friedrichs & Aubrey 1988). Unless they have a relative phase of zero, it is incorrect to add their amplitudes together to produce HAT. Or, rather, one should consider the relative phase when adding. Is that done here?

- **This is done, but in a different way. We now say "**The altimetry constrained product used in this paper is that of the TPXO9 ATLAS which is derived from assimilation of both satellite altimeter and tide gauge data (Egbert and Erofeeva, 2002). The resolution is 1/30^{o} in both latitude and longitude (3.7 km and 2.2 km at Bardsey). We used the elevation and transport information, and their respective phases, for the M_2, S_2, and M_4 constituents. In the following calculations, we approximate the largest tidal current speeds or amplitudes as the sum of the amplitudes of the above three tidal constituents. Of course this is only a crude estimate of the full Highest and Lowest astronomical tides. Note that we are not allowing for M_2 to M_4 phase locking, and the relatively small diurnal tides are ignored. We refer to this as the GA (Greatest Astronomical) in the following.**"**

Line 117—This is the first mention that I can recall of Landsat. Why are these images being downloaded? Leading with a topic sentence that provides some context would be good.

- **Done: "**Landsat-8 data images were used to identify possible eddies in the currents and further illustrate unresolved effects due to the island. Note that we are not aiming for a full wake description in this paper. Data were downloaded from the Earth Explorer website (https://earthexplorer.usgs.gov/). True colour enhanced RGB images were created with SNAP 7.0 (Sentinel Application Platform; https://step.esa.int/main/toolboxes/snap/) using the panchromatic band for red (500 - 680nm, 15m resolution), band 3 for green (530 - 590nm, 30m resolution) and Band 2 for blue (450 - 510 nm, 30m resolution). The blue and green bands were interpolated using a bicubic projection to the 15m panchromatic resolution, and brightness was enhanced to allow easier visualization of the wakes. The images used were taken between 11:00 and 12:00 UTC, when the satellite passed over the area, and the two images were the only cloud-free ones during the measurement periods that were on different stages of the tide."

Line 129—The results lead with a table. I would have expected some text before a table. Maybe put the table elsewhere?

- **Done.**

Amplitudes and phases—Can you think of some way to report confidence intervals or uncertainty, beyond the statement about significant figures?

- **The text has been updated to include "**The non-tidal residuals, the final column in Table 1, compare well with the residuals at Holyhead, the nearest permanent tide gauge station some 70 km north; for Holyhead these were 0.096 m, 0.172 m, and 0.067 m for the same periods (note that bottom pressure measurements at Bardsey include a partial natural sea level compensation for the inverted barometer effect).**"**

Line 145—what about frictionally produced overtides? With a strong current, would seem likely.

- **We analyse for them – see M4 in the table and the text above. There are more, but they are small.**

Line 148-151—The use of numbers could be reduced and the point made more succinctly, here and elsewhere. For example, you could say that TPX09 data suggests only a 0.02m and <1 degree difference in M2 in the cross-channel direction, compared to ~0.19m and 6.5 degrees with in-situ data (see Table xxx). A reader can look at the table for the exact numbers, but doesn't necessarily need to know the exact numbers in the narrative arc (or rather, only needs to know that the TPX gives a much different, and less correct, answer).

- **Thank you for the suggestion. This paragraph now reads "**We turn now to a comparison of the tidal analysis data for M_2 from the two sources (see Table 2 for details). When the TPXO9 M_2 data, which has no Bardsey island representation, is interpolated linearly to the TG positions, the result is only a 0.02 m and 0.7° amplitude and phase difference for the Phase 1 locations. Compared to the 0.19 m amplitude difference and 6.5° phase difference in the TG data, it is obvious that there is a substantial deficiency in the TPXO9 model in representing the role of the island due to its limited resolution. These results are supported by the Phase 2 measurements (Table 2). Phase 3 saw an extended and different approach to the data collection. We revisited East, but also deployed two gauges on the Llŷn peninsula, on the approach to the island (South Mainland)), and north of it (North Mainland). At South Mainland, TPXO is again underestimating the tidal amplitude by more than 10%. At North Mainland, some 5 km north of Bardsey, and just north of the Sound, however, the TG and TPXO amplitudes are within 1 cm of each other. This again shows the effect Bardsey and local topography have on the tidal amplitudes in the region. **"**

Line 162-168: For someone not familiar with this area, the heavy use of place names is sometimes confusing.

- **We provide a map with the place names and are not quite sure how we would describe what is happening without using them. We could perhaps label areas "a", "b" etc, but the locations have names found on a map and we will stick with those.**

General comment:
Can one be sure that estimates of M2 and M4 from TPX09 are directly comparable to your one month long measurements, given things like seasonal and interannual variation? Some discussion and exploration would be good. It seems to me that some review of the TPX analysis would help one frame

the results, and help rule out environmentally-based factors as the source of differences in the constitutent analysis. What is the sampling rate of TPX data, and how long of a data set is needed to obtain good estimates of M2, M4 etc? Since a long time period is needed, any seasonal variation in tidal constituents are averaged out (see e.g. one of the Mueller papers, or Graewe et al. 2014, or others) . However, the in-situ data would be effected by seasonal effects, and possibly astronomical factors such as the strength of the spring-neap cycle over the measurement month (through frictional interaction). Meteorlogical events like the afforementioned hurricane could also affect M2 and M4, possibly. One way to look at seasonal cycles would be to evaluate the seasonal cycle in M2 at the nearest long-term tide gauges. Does such an analysis suggest this a factor in the comparison with TPX? A seasonal cycle in M2 would produce an M4 variability as well, and therefore any comparison with TPX. In shallow water, my experience is that M4 can vary a lot from year to year. TPX constituents are measured over many years, and may therefore “average over” interannual variability. Other environmental/astronomical variability could also be excluded as a potential factor in your comparison. Does TPX consider the nodal cycle? Do you adjust for the nodal cycle in in-situ data?

- **The nodal cycle in the M2 tide is averaged over in TPXO, and as we state on line 178 in the caption to table 2 our observational values *have* been adjusted for nodal and seasonal effects. M4 changes with the nodal cycle too, of course, and is again averaged over in TPXO and adjusted for here. The text has been updated to make this clearer: “**Amplitudes and phases of tidal constituents based on short periods of observations need adjusting to reflect the long term values of amplitudes and phases. The values in Table 2 have been adjusted for both nodal effects and for an observed non-astronomical seasonal modulation of M2. Standard harmonic analyses include an automatic adjustment to amplitudes and phases of lunar components to allow for the full 3.7%, 18.6 year modulation due to the regression of lunar nodes. However, the full 3.7% nodal modulation is generally significantly reduced in shallow water and shelf seas, so locaL counter adjustments are needed. The nodal M2 amplitude modulation at Holyhead, the nearest standard port, is reduced to 1.8% (Woodworth et al., 1991). We have used this value in correcting the standard 3.7% adjustment. The M4 nodal modulations are twice that for M2. The seasonal M2 modulations are generally observed to have regional coherence, so we have used the seasonal modulations from 9 years of Newlyn data (in the period 2000-2011). M4 is not seasonally adjusted, and S2 is not a lunar term, so is not modulated nodally. These very precise adjustments are possible and useful, but overall as stated in the caption to Table 2, for regional comparisons we assume, slightly conservatively, confidence ranges of 1% for amplitudes and 1.0 degrees for phases. **”**

Line 189, Equation 189—What about frictional effects? Would seem that a fudge factor might be warranted, or perhaps a scaling symbol rather than an equal sign. In any case, friction is important, and would be good to account for somehow.

- **True, and the revised text discusses this. We don’t account for it, but rather have made the whole discussion shorter (see reply to RC1).**

Line 191-202—Seems like this paragraph could be reduced in size/explained more succinctly

- **Indeed, edited (see reply to RC1 above).**

Line 191-202 – M2 is being used in the scaling equation (Equation 1) and is being compared to a vague maximum velocity of 4m/s. However, wouldn’t the maximum velocity be more likely during a high spring tide, i.e., when the tidal amplitude is caused by M2 +S2 +N2? Ok, I see this is in the next paragraph. However, am leaving this comment in, because this paragraph and the next could be presented more succinctly, perhaps together. Also, would suggest seeing if there are any model or in-situ results in the peer-reviewed literature than provide estimates of the velocities in this strait, and/or the actual measurements which form the basis for the admiralty charts. The ‘4 m/s’ maximum velocity is quite vague, and the context of this measurement is unknown (was it a wind day? Is it a point measurement, or depth/width averaged? Etc, etc). Therefore, using this value as the gold standard for comparison is a bit iffy.

- **Thank you, this has been rewritten (see reply to RC1 as well). As for the current speed: it comes from the admiralty chart and discussions with the local fisherman who lives on the island. There are no other current measurements from the strait and it will have to do alongside our new current estimate.**

Line 224—Ok, I see now that friction is being considered. Maybe it would make sense to include all the theory in the Methods section, so that it is more clear that you are considering frictional effects? Note there is no Equation 2 in the manuscript (i.e., Equation 3 is ms-labeled).

- **The numbering has been corrected. The friction discussion is covered in the replies mentioned above.**

Line 226—Did you check that the phase speed really is sqrt(gh)? Since you have the phase progression and know the depth, would be good to check. In shallow water when there is friction and/or convergence, the phase speed can be quite different than sqrt(gh). See e.g., Jay 1991.

- **This is true, but it is the group speed that transports energy, not the phase, and the group speed for shallow water waves remain sqrt(gh) as far as we are aware.**

Line 226-234—How does this dissipation estimate compare to more local estimates of dissipation, e.g., within the region between England/Wales and Ireland?

- **The total dissipation on the European Shelf is about 180 GW (Egbert and Ray (2001), so it is a small part of that. In Liverpool Bay, you find similar current speeds so the dissipation rates will be similar. In the Irish sea, the currents are about 0.5 m/s, so the dissipation rates will be far smaller. The latter part of the paragraph now reads "**The dissipation in a tidal stream can also be computed from $\varepsilon = \rho C_D |u|^3$, where Cd~0.0025 is a drag coefficient (Taylor, 1920). Using the TPXO9 current speed in the strait, assuming the Sound to be 3.1 km wide and 2 km long, the GA spring dissipation comes out as 53 MW (using u=1.5 m s-1), and the M_2 dissipation (using a current speed of 1.2 m s-1) as 28 MW. This is a substantial underestimate compared to the estimates above (factors of 7 and ~4.5 for the GA and M_2 tides, respectively), which again highlights the importance of resolving small-scale topography in local tidal energy estimates, and the use of direct observations in coastal areas to constrain any modelling effort. This dissipation here is only a small fraction of the European Shelf and coastline, but it is a very energetic area. Although the Bardsey tides are unusually energetic, underestimated local coastal energy dissipation may be substantial in the TPXO9 (and similar) data and numerical models.

Image analysis—how many images were looked at? How representative and statistically significant is the analysis? I would consider looking at more images, to see if the qualitative results are repeatable. For example, you could look at Landsat 7 or Landsat 5 data. You might also consider looking at the ESA Sentinal-3 data as well. It has fantastic resolution and better time resolution than Landsat.

- **There are unfortunately no more images during the measurement periods that are at different stages of the tide and with clear skies. Yes, there are better products, but this is not the main focus of the paper and they are added to show that there are effects even behind small islands that many models will not catch, with potential wider implications. We have highlighted this in the paper: "**Landsat-8 data images were used to identify possible eddies in the currents and further illustrate unresolved effects due to the island. Note that we are not aiming for a full wake description in this paper. Data were downloaded from the Earth Explorer website (https://earthexplorer.usgs.gov/). True colour enhanced RGB images were created with SNAP 7.0 (Sentinel Application Platform; https://step.esa.int/main/toolboxes/snap/) using the panchromatic band for red (500 - 680nm, 15m resolution), band 3 for green (530 - 590nm, 30m resolution) and Band 2 for blue (450 - 510 nm, 30m resolution). The blue and green bands were interpolated using a bicubic projection to the 15m panchromatic resolution, and brightness was enhanced to allow easier visualization of the wakes. The images used were taken between 11:00 and 12:00 UTC, when the satellite passed over the area, and the two images were the only cloud-free ones during the measurement periods that were on different stages of the tide.**”**

General comment: You might consider looking at Pawlak & MacCready 2001 and Warner & MacCready 2014 for discussion of form drag and eddy formation in the wake of small-scale topography in Puget Sound. Though a stratified region, there might be some useful insights or results in those papers. They also use the Bernoulli Equation, but consider the time-varying potential as well.

- **Thank you, we have added more references to the general discussion in the introduction: “**This can mean that the energetics in the products, and in other numerical model with insufficient resolution, can be biased because the wakes can act as a large energy sink (McCabe et al., 2006; Stigebrandt, 1980; Warner and MacCready, 2014).**”.**
  We do not expand on these further since the aim is to see how wrong the altimetry constrained products are.

General comment: Some more explanation of global models and their resolution is needed. Why is dissipation an important issue? Making this connection will help prove the point that smaller scale resolution can be important.

- **Added: “**Whilst the globally integrated energetics of these models is consistent with astronomical estimates from lunar recession rates (Bills and Ray, 1999; Egbert and Ray, 2001), the local estimates can be wrong.”