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. What are the error statistics on the tidal fit (e.g., RSME) and the uncertainty bounds on the constituents? 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)? 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? The discussion of tidal velocities—and the comparison to a value in the Admiralty chart—is rather vague. 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. 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). 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).

Specific Comments

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

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.

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

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

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?

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?

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.

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

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

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

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.

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….).

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.

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.

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).

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.

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.

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

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.

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?

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?

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)?

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?

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.

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

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

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

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).

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

General commnent: 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?

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.

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

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.

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).

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.

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?

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.

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.

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.