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Nonlinear Processes in Geophysics Discussions

Interactive comment on "Extracting statistically significant eddy signals from large Lagrangian datasets using wavelet ridge analysis, with application to the Gulf of Mexico" *by* Jonathan M. Lilly and Paula Perez-Brunius

Anonymous Referee #1

Received and published: 20 October 2020

October 2020 review of:

"Extracting statistically significant eddy signals from large Lagrangian datasets using wavelet ridge analysis, with application to the Gulf of Mexico" by Jonathan M. Lilly and Paula Pérez-Brunius

General comments:

This is a methodological paper about eddy detection in oceanic surface drifter data from the Gulf of Mexico. It builds on previous methodology, esp. by the first author.

The focus is on refinement of the methodology, as well as documenting the underlying theoretical principles. Only a small portion of the paper addresses the oceanographic findings about the Gulf of Mexico circulation.

The strengths of this paper include its thorough presentation of the mathematical theory behind the method. It is also very well-written and presented in a clear, logical order. I have no doubt that the method is sound, well-understood, and well documented. The application to the observational data from the Gulf of Mexico shows that it works and delivers results.

However, the paper does have weaknesses:

Given that a number of methods with similar objectives already exist, it is not really clear why another one is needed. Other methods might not be as elegant or well-documented, but does this alone really justify an entire paper? The case would be clear if it were shown that existing methods deliver wrong or ambiguous results, or as a minimum, if there were a table that showed each method's capabilities and shortcomings. The authors mention the existence of earlier methods (e.g. references from Dong et al., 2011), and claim that these methods are problematic to apply to the given dataset. I find this claim unsubstantiated, and possibly incorrect, but have to admit that I have not tried any of the methods myself here. Therefore, this paper is an incremental improvement from the previously existing method by the first author - no more, no less. To my knowledge, the attempt to quantify the background flow and thereby the statistical significance of the detected eddy numbers is novel. This is a valuable addition to the method.

At 51 pages, the paper is very long. I was going to recommend significant shortening, but have since realized that the text reads very fluently. It would not help to have the same content at half the length, but having to read every paragraph twice to understand it. Therefore, I will not recommend drastic shortening here. If some minor shortening is desired, e.g. to make room to discuss the other eddy methods, figures 11 and 13 and their discussions could be descoped or downgraded into an appendix.

The paper purposefully keeps the oceanographic results to a minimum, and announces a future manuscript to follow up. This is appropriate for a methodology paper, but a bit more context should be provided: Have other Lagrangian eddy detection methods reported the same asymmetry between cyclones and anticyclones, or is this asymmetry a new finding or specific to the Gulf of Mexico? Are similar eddy properties, incl. the asymmetries between cyclones and anticyclones, generally present in eddy-resolving numerical circulation models?

There is a large number of self-citations in the references. While it is a good thing to provide background information, a malevolent interpretation could be that not many people other than the authors themselves care about this topic. This makes the paper appear less significant. As of Oct. 19, 2020, there were no public discussion comments about the online version of the paper. This, too, reflects poorly on the significance of the paper. Perhaps the authors can still ask a handfull of peers to submit comments before the deadline? This could at least show that interested readers exist.

In the introduction section, the word "dynamic" is used in a way that does not match my expectations. I was expecting "dynamics" to refer to explanations (forces, energy budgets, Navier-Stokes equations) behind the observed motions, and would distinguish this from "kinematics", the mere description of what the motion looks like geometrically. The introduction suggests that a method - I understand this to be the method the paper is describing - should be "rooted in dynamical theory" (line 30). However, the method presented here is purely kinematic, in that it encodes a clever way to find trajectory segments that match a particular geometry. There are some dynamical aspects such as the discussion about limiting anticyclonic frequency ranges as well as inertial and tidal motions, but the "root" of the method is purely kinematic, just like the other methods summarized by Dong et al. (2011). I think it is perfectly okay for the method to be "kinematic" only, but I feel the introduction is overstating or at least mis-stating what the method actually does.

Overarching review questions (from the NPG website) - I have answered them separately in the questionnaire as "no" wherever I had some lingering doubts. Please refer to the list here for details:

* Does the paper contain new and significant results?

The method is "new" only insofar as it is an improvement of an existing method. See also comments above.

* Is the paper of an international standard?

Yes, it is.

* Is the presentation clear and concise?

The presentation is very clear. See comments about length above.

* Does the paper put the obtained results into context, with relevant references?

The paper explains its own methodology well, incl. references to earlier versions and mathematical background, but lacks a bit w.r.t. context with other methods. See comments above.

* Is the length of the paper appropriate?

The paper is very long, but as I wrote above, this is acceptable given that it reads well.

* Is the text fluent and precise?

Yes, the fluency of the text is a strength of this paper.

* Are the title and the abstract pertinent and understandable to a wide audience?

Yes, these are appropriate.

* Are all figures necessary and of appropriate quality?

Comments about length, above, identify two figures that are not absolutely necessary.

The detailed comments below call out a few figure details, in particular a several cases where the figure sizing needs to be adjusted (captions overflow the pages). Some of the thinner lines would display better on my screen if they were either thicker or darker, but this may be an issue with my screen rather than the authors' choices.

Specific comments:

P. 2, ll. 24 ff.: I have glanced over the Dong et al. (2011) reference and the eddy extraction methods listed therein, and am not convinced that applying these to large datasets would be as problematic as the authors here suggest. Firstly, the whole global drifter dataset is a few ten thousand trajectories, which is not prohibitively large. This amount of data can be handled by present-day laptop computers, let alone larger machines. Secondly, at least some of these existing methods identify looping trajectory parts through different mathematical techniques and actually provide computer code to do so. Running existing code over several thousand trajectories does not look problematic at all to me. I agree that the methods I have looked at are not as eloquently described as the one here, and that estimates of errors or significance are lacking. My interpretation is that nobody has tried to run several of these methods side-by-side over a global dataset, not because it is "problematic", but rather because it requires time and money (somebody's salary) to do so.

P. 30, ll. 680 ff.: This paragraph describes a complex situation with multiple oscillatory signals superimposed, and the wavelet spectra (fig. 10) give some insights into what is going on. That said, I am missing the statement that the method looks for elliptical signals, and in this particular situation, the signal just isn't very elliptical. I have looked at altimetry maps from the time and see the cyclonic eddy (the large one discussed here) being pushed around by e.g. an SSH high to the north, which is consistent with what is written here. We cannot expect this to result in pretty, elliptical trajectories (at least if the interaction happens on the same time scales as the eddy rotation), so any detection method will detect all sorts of artefacts when trying to match ellipses. I am not suggesting any changes to the manuscript, but want to reiterate that reality is not

as geometrically perfect as the model we are imposing on it.

Use of wording "geostrophic turbulence" in lines 83, 187, 367: The wording suggests that geostrophic turbulence is some background noise process, and that the eddy signals examined here are something distinguished from this background. Is this really a good interpretation? Effectively, geostrophic turbulence is what fills the oceans with eddies, and if a drifter is trapped inside one of those (as opposed to e.g. a Loop Current Ring), the methodology here should rightfully find it. Drifters might bounce back and forth between multiple eddies from this background flow, in which case any individual eddy event is not detectable here because the trajectories are not looping but rather some random walking. Anyhow, I have stared at surface flow animations on the following website, which show e.g. the Loop Current, but also the open ocean as a "sea of eddies" that I assume is geostrophic turbulence. I would think that the open-ocean eddies would (and should) be part of what this method detects, and feel that "geostrophic turbulence" is part of the signal rather than the noise. Here is the website with the visuals: https://svs.gsfc.nasa.gov/cgi-bin/details.cgi?aid=3827

Minor issues and typos:

Typo, p. 2, l. 24: "Solutions to this problem _has_ been...", should read "have"

Insert space, p. 6, l. 131: "region is left to a sequel._All code developed"

P. 7, footnote 2: Consider moving this footnote into the "Acknowledgments" section.

Insert space, p. 13, l. 294: "univariate signal x(t)_was"

Notation in section 3.1, table A1, and throughout manuscript: I would have preferred the symbol "lambda" for longitude, which is commonly used alongside "phi" for latitude. Change or ignore, at your discretion.

P. 15, l. 360: Incorrect exponent, should be: Omega = 7.292 * 10ˆ(-5) rad sˆ(-1)

P. 21, footnote 5: Consider moving this footnote into the "Acknowledgments" section.

P. 25, fig. 8a: On my screen, the thin gray lines are hard to see. Recommendation: use more conspicuous colors (e.g. black, dashed lines?).

Typo, p. 28, l. 657: "nonstationary" -> "nonstationarity"

P. 28, fig. 9: I was confused by the black line in panel a, but I suppose this is just the bathymetry? Recommend to remove it, or replace with gray shading, to avoid the association with the black line in panel b.

P. 29, fig. 10: Reduce size to allow caption to fit on page (or consider removing panel a). I would also recommend slightly thicker lines for the colored lines that match fig. 9, just to increase visibility.

Typo, p. 30, l. 696: "It amounts to _a_ idealized...", should read "an"

Typo, p. 32, l. 733: "no effect in improve the stochastic"

P. 33, ll. 760 ff., recommendation: expand a few occurrences of "data" into "real-world data" or similar, just to be extra sure which dataset is being talked about.

P. 33, l. 771, recommendation: remove "more" from "discussed more later"

P. 34, figure 12: Reduce size to allow caption to fit on page.

Typo, p. 36, l. 806: "Thus" -> "This"

P. 36, l. 810: Should "minimum and maxima frequency" be "minimum and maximum frequencies"?

P. 40, fig. 15: Reduce size to allow caption to fit on page.

P. 43, ll. 968/969: The "spheretrans" algorithm is not really "discussed here", apart from the following few sentences (which are appropriate for the purpose). Recommendation: replace "will be briefly discussed here" with "works as follows".

P. 43, l. 980: The GOMED link is missing the "doi.org" part.

P. 43, data availability in ll. 975 ff.: It seems that access to the GOMED dataset (the second of the two links in this paragraph) is restricted and only granted after an application process. This is fine, but the access restrictions on GOMED should be explicitly mentioned in this paragraph (e.g. as requiring registration, non-commercial use only, no derivatives or redistributions allowed). Access restrictions on the trajectory data (the first link) are described properly.

P. 1, l. 11: Following on the previous topic: the abstract claims that the GOMED dataset is made freely available. This is not true and should be corrected, e.g. as "...dataset are made available for non-commercial research use."

P. 4, l. 94: Ditto.

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