Nonlin. Processes Geophys. Discuss.,
https://doi.org/10.5194/npg-2018-54-RC2, 2019

Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Competition between Chaotic Advection and Diffusion: Stirring and Mixing in a 3D Eddy Model" *by* G. J. Brett et al.

Anonymous Referee #2

Received and published: 4 January 2019

Review: Competition between Chaotic Advection and Diffusion: Stirring and Mixing in a 3D Eddy Model

The authors study the transport of a passive tracer in a model of an oceanic eddy with regard to the effect of a deterministic disturbance and an additional diffusivity. The oceanic eddy is modeled by two approaches, first a kinematic model is elaborated upon and then compared to a DNS simulation. Both models share the idea of a rotating cylinder with an additional stirring at the top by a differentially rotating lid. The deterministic disturbance is introduced by a displacement of the axis of rotation. Both models give qualitatively similar chaotic states which are analyzed by Poincare maps. Further different approaches are taken in order to introduce some diffusive process to the system. These approaches cannot be directly compared since they are

implemented within the different eddy models but qualitatively the results hint into the same direction. Especially interesting is the variety and detailed analysis of three different measures that determine the transport, mixing and stirring behavior of the model system. Namely the ratio of the tracer filament arrest scale (Batchelor scale) to the width of the chaotic region, the rate of dispersal of closely spaced fluid parcels (passive tracers), and the Nakamura effective diffusivity along with the volume integrated tracer variance function. It becomes evident, that chaotic advection introduced by a deterministic disturbance can largely enhance mixing at intermediate times and even for longer times. Many aspects of the underlying mechanisms that lead to these results are discussed and it is found that the deterministic disturbance is especially important for shallow eddies with a large area called "chaotic sea" within the Poincare map.

The article is very well written and touches many important aspects of the intricate problem of the interaction of chaotic advection and diffusive processes. I do recommend the article for publication after some very minor revisions and I really enjoyed reading it. Besides some typography issues and some clarification proposals to figures and definitions I have also some general questions. I hope the authors response might deepen my comprehension and maybe the reply aids to improve the readability of the very nice text even further.

In chronological order (more or less):

Line 69: I would rather talk about important characteristic for the problem considered instead of a "person". Something like: *The terms "important" and "relevant" are somewhat subjective, and a particular aspect, such as the existence of barriers, that is of interest for one scientific question may not be so to another.*

Line 244: though

Fig.2/Fig.3: The scale of the upper row of figures looks not equidistant in my viewer. It is also confusing that the lower plots have a scaling range and the upper ones do not. Further it is said in the caption that the simulations are from another work while in the text the simulations are claimed to be done within this study. This is a bit confusing.

Line 252: Maybe I missed it but was the chaotic layer thickness introduced before being used here? I feel like maybe it is necessary to revise the usage of chaotic sea, width of chaotic sea, chaotic layer and chaotic region, see also line 16 in the abstract. A mere note would help easier reading.

Line 264: Should it not be equation (13)?

Line 273: Does it make sense to talk of a timescale of a non-dimensional timestep? In general I was sometimes confused by the varying usage of non-dimensional units and dimensional ones. Maybe it could be helpful to use only adimensional ones in the text and make a little table for the conversion?

Line 319: Cauchy-Green.

Line 341: $\gamma = \sigma$?

Line 349: I feel like there is some other reference missing (or not?). Did not Okubo just study up to a scale of 100km in that report?

Fig.5, caption: Which δ values? (22) or (15)? Even though it becomes clear studying the text it would be easier if it was stated. Just to be curious: What is the result for (15)?

Line 364: Fig. S 2-3

Line 366: 10-20 rotations <-are those the integration times for the FTLE?

Line 377: "rotating can model“ is it the standard name for the model used? Why not introduced before? It difficultates understanding using a new name.

Line 421: pf→of

Line 507: Year of citation of Shuckburgh, E. and Haynes, P. should be 2003.

Line 545: The numerical simulations are run using the solver NEK5000 . . .

Fig. 8: X_0 instead of ϵ in caption and in legend the x is small? Further I would find it really helpful for the comparison of the temporal evolution and the peaks of the different cases to have a grid in the background of the plots.

Line 596: After "dye contours“ put reference to Fig 10(e)?

Line 599: *A caveat is that k_{eff} is a nonlocal property, the value of which at any point in space and time is influenced by processes occurring at all other locations having the same C at a distinct time t (?).* This time dependence could also be explicitly noted in equation (25) to clarify. I must admit that I am still a bit confused about the details of the calculations of k_{eff} here. As I understand it the k_{eff} values are calculated at each time step from volumes of equal concentrations (and dC/dV is taken from the cumulative distribution of volumes with concentrations $c <= C$ as in the reference [Shuckburgh and Haynes, 2003]). One then gets a function $k_{eff}(C, t)$. The plots of k_{eff} at fixed instances t would thus just be derived by finding the corresponding value of k_{eff} at that $C(t)$? Or is there any such thing as an equivalent latitude used here? I wonder why the two plots of $k_{eff}(t)$ and $C(t)$ look so different. The k_{eff} looks much more noisy. How does $k_{eff}(C, t = 39)$ look like?

Line 607-608: Do the results depend on the details of the initial condition? What must be met to ensure that the results are independent?

Line 631: *Thus the combined effect of smaller diffusivity and finer filaments (i.e., stronger tracer gradients) leads to more rapid mixing across tracer contours.* This is my favorite insight of the analysis and it is, at first glance, counterintuitive to me and still makes so much sense.

Line 667: Here I am confused about the statement: A smaller diffusivity leads to less mixing (X^2). Especially when compared to details of Fig. 8.

Line 681: I feel that pros and cons is a bit colloquial and would suggest advantages and disadvantages (or so). But that is just my personal opinion.

As a general remark to the Figures and their descriptions, there is quite some mixed usage of either left-middle-right-bottom-top and a,b,c,d,. . .. Maybe it would become easier to read just using a,b,c,d,. . . Especially Fig.'s 8/9/10.

On a similar vein I also find the usages of the word turbulence, turbulent diffusivity, diffusion eddy diffusion and similar expressions confusing too. I would try to avoid using these concepts interchangeably and only use those expressions that were defined before.

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