Nonlin. Processes Geophys. Discuss.,
https://doi.org/10.5194/npg-2018-54-RC4, 2019

Nonlinear Processes
in Geophysics
Discussions
Open Access

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 #4

Received and published: 18 January 2019

This is an ambitious, and in many ways, very good paper. The authors seek to elucidate the competition between chaotic advection and turbulent diffusion in an idealized model of a 3d eddy. Two types of numerical simulation are presented. The first type (I) makes use of an analytically prescribed steady 3d incompressible flow. For this type (I) of simulation two different parameterizations of turbulent diffusion are considered: a) a scale-dependent eddy diffusivity; and b) a stochastic additive noise perturbation to the Lagrangian equations of motion. The second type (II) of simulation makes use of a numerical simulation to the NS equation together with a numerical solution to the advection diffusion equation. Type I and type II simulations could not be more different. The type I simulations are based on some really strong assumptions: 1) there is a separation of scales between the background flow and the turbulent perturbation;

2) the background flow is steady; and 3) the turbulence can be parameterized as a diffusive process (in type I simulations there is no actual turbulent flow). None of these assumptions apply to the type II simulations. Indeed, there is no guarantee that the type II simulations approach anything resembling a quasi-steady flow. With this fundamental difference in mind, I find it remarkable that the qualitative agreement between the type I and type II simulations is as good as it is. Evidently, the authors' analytically prescribed kinematic flow does a really good job of capturing the important underlying physics that are tied to the flow geometry considered. That is impressive. But this good qualitative agreement serves to highlight the importance of assumptions 1) and 2) (maybe also 3)) listed above. It is my view that the validity of almost all of the conclusions/inferences presented in the paper is limited to flows for which assumptions 1) and 2) (again, maybe also 3)) apply. Assumption 2) is both especially important and especially restrictive, and this should be stated clearly by the authors. It is assumption 2) – together with incompressibility – that leads to the underlying structure of a 2 dof autonomous Hamiltonian system. In other words, without assumption 2) there is no basis for the entire chaotic advection paradigm (nested tori, chaotic seas, resonant bands, Poincare sections, etc.) on which the paper is built. Thus, while I found many of the the authors' inferences/conclusions interesting and insightful, I think that the validity of those conclusions/inferences is limited to this class of flows. If the authors disagree, they should explain why. Two additional minor quasi-technical points are worth mentioning. First, in many cases throughout the text the authors make statements about exponential stretching, exponential growth, positive Lyapunov exponents, etc. The authors should be a bit more careful about saying that these behaviors are average behaviors for long times. Second, for the type Ib simulations, the algorithm that the authors use to integrate the stochastic differential equation, Eq 23, is quite crude; there are simple explicit integration schemes for stochastic differential equations (SDEs). (Does the mystery factor of 1/3 described on lines 405-407 somehow account for the fact that a Runge-Kutta algorithm is trying to mimic a real SDE integrator?) Overall, the paper is good and well written. I recommend publication, but I urge

the authors to make some small changes consistent with the remarks above.

Interactive comment on Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2018-54, 2018.