Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-300-RC2, 2019

Geoscientific Model Development Discussions

Interactive comment on "On fluctuating air-sea-interaction in local models: linear theory" *by* Achim Wirth

Anonymous Referee #2

Received and published: 20 March 2019

General comments:

This manuscript examines the dynamics of three possible parameterizations of air-sea momentum transfer by treating them as stand-alone linear models that are either deterministic or driven by additive noise. For reasons listed below, I cannot recommend publication of this manuscript.

The author introduces three air-sea interaction models that appear much simpler than I suspect is actually implemented in coupled models. Citations to coupled models that use these parameterizations might have obviated this objection, but without them, I cannot say that detailed examination of the three cases as done here merits publication. The simplicity of these models does not warrant such detailed exposition in a published article, and the appendices are generally reproduceable by anyone who has

studied undergraduate linear algebra.

In spite of the general title, only momentum fluxes at the air-sea interface are considered. Clearly, thermal fluxes are just as important, and these are ignored completely. This manuscript treats only a small part of "air-sea-interaction in local models," in spite of the more general title.

Next, the stochastic models involve only additive random forcing. At the timescales at which these parameterizations should be used, multiplicative noise is likely to be much more appropriate. The analytical treatment of both additive and multiplicative stochastic linear models, some of which are inhomogeneous, have been treated in the geophysics literature for the last several decades. As it is not the reviewer's place to perform major revisions to a manuscript, I invite the author to perform a literature search for the easily-found references to such studies.

The same lack of citations applies to discussions of stochastic integration in both the Ito and Stratonovich sense, both of which have been used in weather/climate studies for years, if not decades. The fluctuation theorem has been used less often, but is still found in the mainstream weather and climate literature.

The author should be aware that he is not introducing the stochastic dynamics literature to geoscience. It is true that no one can read everything. However, this manuscript gives little or no citation either to the models whose parameterizations are featured here, nor to the vast amount of stochastic geophysical research that has already been published. Scholarship requires more.

Some specific comments:

Pg. 2, lines 14-15: A forced, dissipative system need not be confined to a strange attractor. Fixed points and limit cycles, as well as dense tori in a sampled phase space, are well-known in nature.

Pg. 4, Eq. (1) and beyond: Some symbols are never defined.

Pg. 6, lines 8-12: The author confuses momentum with inertia.

Pg. 6, line 12: Why is the additional timescale spurious? Most linear systems in nature do consist of a superposition of timescales.

Pg. 7 and beyond, including appendices: The author uses the term “solution” when what is meant is the “particular solution.” Transients are part of the solution.

Most specific comments on the remainder of the manuscript are simply special cases of general comments stated above.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-300, 2019.