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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Brief Communication: A nonlinear self-similar solution to barotropic flow over varying topography" *by* Ruy Ibanez et al.

Anonymous Referee #2

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This paper presents steps towards an approximate similarity solution to a form of the steady shallow-water equations that includes variable topography and a representation of Ekman pumping; this builds on earlier work by some of the authors. Eventual application to ocean flows is a stated goal, but no direct comparison is attempted here.

Unfortunately, I did not find the paper convincing, for two reasons. First, the assumed power law form of the topography seems arbitrary on physical grounds; I understand that it is needed for the similarity solution to work but that is just a mathematical reason with no oceanic relevance per se. Second, and more importantly to my mind, the derivation of the key governing equations (2) & (3) seems somewhat wrong (details below). If that is true then the similarity solution that follows later is hard to accept, of course. Perhaps this is just a confusion that could be cleared up under revision, but to

my mind a theoretical paper that is unclear on the foundations can serve little purpose. I regret that I cannot recommend this paper for publication.

Derivation steps leading to (2) & (3):

At first sight, (1) appear to be the standard shallow water equations with a free surface, augmented by Navier-Stokes frictional terms and another term representing an Ekman flux. The free surface seems to be there because of the time derivative in the height equation for h. However, closer inspection shows a pressure p in the momentum equations that is not linked to the height field h. The relationship between h and p is not discussed; as it stands (1) has more unknowns than equations.

My impression is that these are in fact the shallow water equations with a rigid lid on top, in which case p is the pressure just under the lid. But in that case the time derivative of h should not be there, so this is confusing to me.

Either way, the authors then take a curl, which makes the pressure term p disappear. They then assume that a mass transport stream function psi exists such that psi_x=hv and psi_y=-hu. This implies that the divergence of (hu,hv) is zero. This is again possible if a rigid lid is assumed such that h_t=0, but only if there is no Ekman pumping.

The authors then assume that the flow is steady, but because of the Ekman flux term in the continuity equation the divergence of (hu,hv) is NOT zero, so there cannot be such a stream function psi. It seems one can either have a stream function or the Ekman flux, but not both. But then the authors claim that (3), which approximates (2), should retain the Ekman term as a leading order dissipative term. To me, this looks inconsistent with the existence of a stream function psi in the first place, as indicated above.

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