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Interactive comment on "Technical Note: Volume Transport Equations in Combined Sverdrup-Stommel-Munk Dynamics without Level of no Motion" *by* Peter C. Chu

Anonymous Referee #2

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This work has a number of fundamental flaws that prevent me from recommending it for publication. There are too many to connect all of the consequences to the final result, so I will just list them in the order they appear in the document.

1. The manuscript begins by stating small Rossby and Ekman numbers, but then retains the frictional terms and drops the acceleration and inertial terms. Thus, it is not a formally correct asymptotic limit, which would require boundary layers at the top and bottom where frictional terms are not negligible but a geostrophic interior flow.

2. It is unclear exactly how much density variation is to be preserved. (2) is a steady

state version of the compressible equations, but later in (3-7) ρ_0 is used in the fluxes and stresses without comment, which implies that a Boussinesq approximation is intended. The inconsistent treatment of density variations leads to mistakes in properly arriving at the JEBAR bottom boundary effects. An extensive literature on this topic, going back to Shtokman (1946), shows that the results presented here are not new (see Sarkisyan, 2006 and Sarkisyan and Sundermann, 2009). Even if these mistakes were corrected, the JEBAR approach can be misleading (Cane et al. 1998), and a vorticity budget needs to be carefully constructed so as to agree with its solution method–which is typically numerical nowadays (see Yeager, 2015 for a nice example).

3. Relatedly, the author proposes that the key depth H can be made into a spatially-dependent function $H(x, y)$. Then, some of the terms required–which importantly include frictional boundary layers over a sloping bottom–are neglected in arriving at the form of the frictional terms in (3-4). (7c) does not fully handle the bottom boundary, because in those terms ρ has been replaced with a constant, while in (5) they have not. In general, the author is careless with the Boussinesq approximation and the evaluation of the pressure gradients along the bottom boundary. (28) is not the vertically-integrated vorticity equation, because it is missing these interaction terms.

4. The surface is taken at $z = 0$ in (10), implying that a rigid lid approximation is being used, but a rigid lid approximation is inconsistent with (22), because the largest generator of near-surface geostrophic velocity is the gradient of sea surface height, or equivalently in the rigid lid approximation, the pressure boundary condition taken at $z = 0$. That is, (22) is *not* the meridional geostrophic transport.

5. It is stated that Stommel's (1948) solution relies on a level of no motion, which is incorrect. Stommel's original model is in fact a low Rossby number, uniform density, flat-bottom model with a spatially-varying sea surface height. Later pa-

pers revisiting Stommel's bottom drag balance added levels of no motion, or reduced gravity or equivalent barotropic reformulations to aid in re-interpretation (e.g., Fox-Kemper and Ferrari, 2009), but Stommel does not choose to assume a level of no motion.

6. (7b) is a terrible approximation over a sloping boundary in a baroclinic fluid. The along-boundary velocity, which experiences wave and frictional drag, is only lightly related to the depth-integrated mass transport. Indeed, in the Atlantic, beneath the Gulf Stream the bottom velocity is often flowing in the opposite direction of the surface current due to the deep western boundary current.

7. The primary reported results are (20, 23, 24, 25). All of these formulae are readily found (in more accurately derived versions) in the JEBAR literature, which also do not assume a flat bottom or a level of no motion.

8. The correct form of (30) is better interpreted as motion relative to f/H contours, see Holland (1967).

9. The overarching idea of this paper is that we do not have the machinery to evaluate the effects of forcing products without an assumption of a level of no motion, but we do this every day with GCMs. They produce solutions which are consistent with the vertically-integrated vorticity budget, which occasionally resembles the Sverdrup relation, and occasionally does not (see Yeager, 2015). Advanced parameterizations can be used for coupling to smaller scales, rather than inaccurate closures like (7b). Therefore, I am at a loss as to what the point here is, since the analytic work is not correct and if one is to use numerics then a better, more consistent solution is already in hand and published.

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