Reviewer #2:

Referee: Rupert Klein

General comments:

The authors investigate the effect of asymmetric perturbations on the intensification of tropical storms based on a reduced two-layer model. The model consists of a slab boundary layer (SBL) and a superimposed layer with shallow water-type (SWM) dynamics. The SBL model is to represent the near-surface layer of the atmosphere influenced strongly by vertical turbulent transport, while the SWM represents about the lower 2-3km of the troposphere. The rest of the troposhere up to the tropopause is modelled implicitly by assuming that it has no dominant effect on vortex intensification and that it takes up and redistributes any vertical mass fluxes that may emerge out of the shallow water layer due to convection. These model components, including parameterizations of unresolved scale processes are adopted from the established literature, where they have already been used and argued for in similar contexts. In this sense, I consider the ingredients of this two-layer model to have stood the test of time as qualitative representations of some important aspects of large-scale atmospheric vortices. I do have some questions regarding the layer coupling, which I will post below.

The numerical scheme implemented to solve the model equations judiciously borrows from spectral and finite difference discretizations and is solidly state of the art.

The paper provides a detailed numerical study that juxtaposes model results with one- and two-way coupling of the two layers. In the one-way version, the SWM influences the SBL but not vice versa. The study clearly reveals that two-way coupling is crucial for reproducing key observed features of accelerating storms, such as a shrinking of the radius of maximum wind RMW during the intensification period, and – more importantly for the present paper – the interplay of Fourier mode one and two asymmetries during the process. Plausible physical interpretations of the processes observed in a series of model runs are provided, yielding an interesting set of hypotheses regarding the mechanisms behind what is called "rapid intensification" of tropical storms.

The paper is very well written, with a concise and clear literature review, well-structured technical descriptions of both the mathematical model used and of its numerical discretization, and with clear discussions of the simulation results.

Thank you for reviewing the manuscript and providing constructive comments. We have made edits to the manuscript incorporated with your suggestions. Reviewers' comments are shown in black, our response to each comment is shown in blue, and changes to the manuscript are shown in red.

Specific Comments

1. I do have one concern regarding the structure of the two-layer model. In lines 174, 175, the authors state that "Lack of strict mass conservation is not a problem for the length of time integration and the aims of the study considered here, but the model is not expected to reach a steady state with this numerical approach." I urge the authors to provide an extended argument leading to this conclusion for the following reason: The spin-up of a vortex is largely driven by the conservation of angular momentum and the fact that in the boundary layer mass is moving inwards, thereby inducing acceleration of the primary circulation. The inward-moving mass must, for conservation reasons, go somewhere. If I understand it

correctly, it is assumed here that the mass more or less slips through the SW layer and then disappears in the implicitly modelled bulk of the troposphere. What justifies assuming that the SW layer does not pick up at least part of that mass - an effect that would counteract that of the assumed "mass sink" attributed to convection and entrainment? And why would the implicitly modelled upper part of the atmosphere, which does absorb the upward mass flux and should, therefore, reveal a slow-down, not influence the shallow water layer at all?

Thank you for the insightful comment. You are correct that that the convective mass flux is continuously removed from the assumed mass sink during the numerical integration and that it implicitly must be deposited into the layer above. We have assessed the total amount of mass removed from the simulation in Fig. R1 to address the reviewer's concern.

The fluid depth is nearly constant for the one-way experiment as expected, with only small fluctuations due to the open boundary condition on h and some very small dissipation from the model numerics. As the reviewer notes, the mass in the boundary layer can be considered to be constantly replaced through the radial inflow associated with the inward movement of the angular momentum surfaces. Mass continuity is prescribed exactly due to the diagnostic w and the fixed height of the slab boundary layer. Any air that leaves the boundary layer is implicitly replaced by exactly the correct amount via radial inflow.

On the other hand, the mass from the shallow water layer in the two-way experiment was reduced around 27 %, which could be considered a substantial reduction in 24-hours. To address the reviewer's concern, we have incorporated an additional discussion in the manuscript at L376:

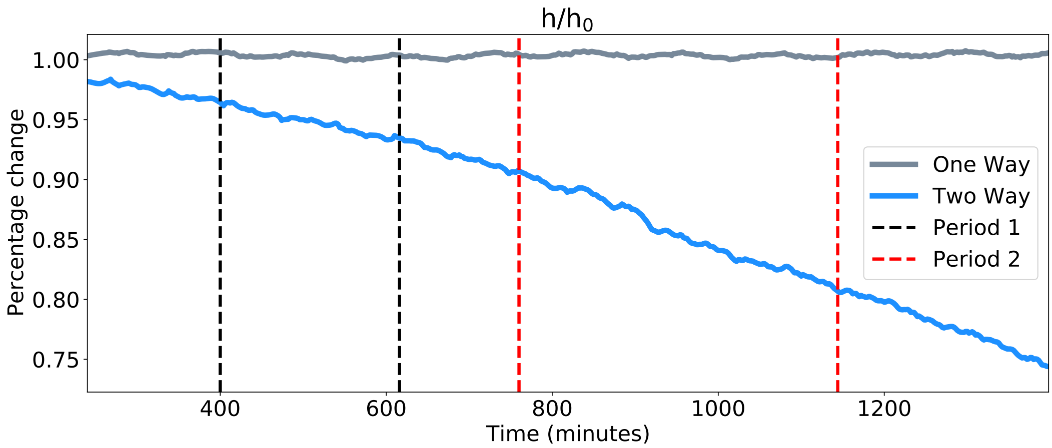

Figure R1: Evolution of the mass fraction removed from the shallow water layer over time.

A calculation of the fluid depth removed from the shallow water layer indicates that the amount is very small at any single time step or gridpoint (generally less than 0.1 mm), but that the cumulative effect is substantial and the total mass is reduced by $\sim 27\%$ of its original value. Our results are consistent with the conclusion of Schubert et al. (2016) that vortex intensification in the shallow-water framework is proportional to the volume of mass removed from the vortex interior. We have effectively assumed that the updraft erupting from the boundary layer is accelerating through the shallow water layer, such that the vertical motion at the top of the layer is larger than at the bottom and mass is removed. This assumption is consistent with a bottom-heavy convective mass flux profile in the lower troposphere. If the top of the layer is considered to be an isentrope, this the mass removal is equivalent to a positive vertical gradient of diabatic heating across the layer. Implicitly then, the mass removed from the layer is being deposited into an unresolved upper-tropospheric layer where it must either accumulate or be removed through radial outflow.

In the simulated boundary layer the mass can be considered to be constantly replaced through the radial inflow associated with the inward movement of the angular momentum surfaces. Mass continuity is prescribed exactly due to the diagnostic w and the fixed height of the slab boundary layer. Any air that leaves the boundary layer is implicitly replaced by exactly the correct amount via radial inflow. We can make such an implicit assumption in the unresolved upper-tropospheric layer as well, such that the mass deposited from the lower-layer is exactly ventilated by the radial outflow. Under that assumption, the upper-layer is entirely passive but maintains mass continuity in the full the atmosphere. Interestingly, it does not appear to be essential to explicitly simulate this layer to achieve rapid intensification, but addition of those effects could result in a change of the intensification rate. An additional third layer could allow for the development of an upper-level anti-cyclone and permit the inclusion of vertical wind shear and top-heavy stratiform mass flux profiles. A third layer would also allow for the effects of entrainment by considering the fraction of mass passed through the lower layer as in the axisymmetric simulations by Ooyama (1969). We leave such additions to the modeling framework to future work, but acknowledge that the specific intensification rate simulated here depends on the assumptions made about the mass flux profile and the passive role of the implicit upper-layer.

2. The authors report to impose homogeneous Neumann inner boundary conditions for vertical velocity, w, and boundary layer height, h. While I can see how that can be justified for the height from radial momentum balance, I don't see (i) why this condition should hold for w and (ii) why there should be a boundary condition for w in the first place. According to (6), the vertical velocity is the product of the boundary layer height and the horizontal divergence. Even if the height satisfies a homogeneous Neumann condition, I don't think the horizontal divergence would do so. If I am right, the radial gradient of w is height times the radial gradient of the divergence. Moreover, nowhere in the governing equations does the radial derivative of w occur, so why should a boundary condition for w be needed at all. What am I missing?

Thank you for carefully reviewing the manuscript and bringing this to our attention. Since the boundary layer updraft is diagnostically calculated, prescribing a boundary condition for w is unnecessary. We have removed the statement and added an additional sentence to clarify this point.

w is diagnostically calculated throughout the model integration over time.

Minor comments:

l. 113: shallow watter $->$ shallow water
l. 146: later $->$ layer
l. 374: evoluti99on $->$ evolution

We have corrected the typos as suggested. Thank you!