

## Second Referee Report

**Title:** Asymmetric and Axisymmetric Dynamics of Tropical Cyclones

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**Synopsis:** This paper compares 3D and axisymmetric simulations of tropical cyclones (TCs) carried out with a commonly used cloud model (CM1). It is shown that 3D and axisymmetric pathways of TC development have significant differences, and that the results of axisymmetric simulations are sometimes misleading. It is verified that intensification rates can differ substantially in 3D and axisymmetric simulations, and that a mature 3D vortex tends to be weaker at peak intensity than its axisymmetric counterpart. The differences are primarily attributed to the dissimilarity of deep cumulus convection in 2D (axisymmetric) and 3D systems. After a basic overview of evolutionary differences in 3D and axisymmetric simulations, the consequences of convective dissimilarity on balanced spin-up are discussed. Subsequently, this paper compares and contrasts various terms contributing to the acceleration of the mean azimuthal velocity field in 3D and axisymmetric TCs. Evidence is presented suggesting that transport induced by asymmetric disturbances in 3D simulations cannot be parameterized adequately with a conventional “eddy diffusion” term in an axisymmetric model. It is also shown that the 3D simulations are inconsistent with a recent hypothesis of Richardson number criticality in the outflow layer of a TC. Last but not least, it is argued that the surface-drag dependence of the intensification rate differs between 3D and axisymmetric models partly because decreasing surface drag in a 3D model weakens an important mechanism for organizing convection in the azimuth. Each section includes considerable discussion/commentary on earlier work pertaining to the subject at hand.

**General Assessment:** This study provides computational support to some recent ideas on the differences between axisymmetric and non-axisymmetric TC development. The results could have a significant influence on the advancement of tropical cyclone theory. However, there are some aspects of the data analysis that still concern me.

**Recommendation:** Accept for publication assuming that the authors respond adequately to my concerns.

### Primary Concerns/Comments:

1. In section 5, the authors state that the computed time derivative of the mean azimuthal velocity  $\langle v \rangle$  “agrees reasonably well quantitatively” with the sum of all tendency terms (here denoted by  $\Sigma$ ) on the right-hand side of Eq. (12). Some clarification would be appreciated. Figures 10h and 10i suggest to my eyes that the differences between  $\Sigma$  and  $\partial_t \langle v \rangle$  could be of order unity in various regions of the eyewall above the boundary layer (BL). In addition,  $\Sigma$  seems to be much greater than  $\partial_t \langle v \rangle$  near the surface, in the vicinity of the RMW. Similar differences between  $\Sigma$  and  $\partial_t \langle v \rangle$  are seen in Figs. 12g and 12h. In my mind, this brings into question the accuracy of the individual tendency calculations. My concerns are heightened by the omission of  $\Sigma$  plots from Figs. 11 and 13.

In their reply to the first set of reviews, the authors state, “Due to sampling in time, perfect budgets are difficult to assure. Most of the error does result in the BL.” While I appreciate the difficulty in obtaining accurate budgets, I am not entirely satisfied with this response. What is the decorrelation time for the various tendencies? Is it not possible to reduce the error to 10-25% with

sufficiently small output intervals, or with run-time analysis? The conclusions stated in section 5 seem reasonable, but I feel that the supporting evidence is weakened by the apparent differences between the  $\partial_t \langle v \rangle$  plots and  $\Sigma$  plots.

**2.** Section 4.1 outlines a procedure for assessing the extent to which classic balanced spin-up theory applies to the actual spin-up mechanism above the boundary layer. Regarding this procedure, it is unclear to me why comparing a volume-average of the gradient wind tendency ( $\partial_t v_g$ ) to that of the spin-up function  $S$  [Figs. 9b and 9d] is more useful than comparing the average of  $\partial_t v_g$  to the same quantity obtained from axisymmetric balance theory.

**3.** Since the editor is not imposing a length limit on the manuscript, it might be helpful to add a paragraph explaining why 3-km horizontal grid spacing is deemed sufficient to realistically simulate the fluctuations of interest in the eyewall, which seems to be less than 10 km wide at late times.

**4.** In section 7, the authors justifiably focus on the development of TCs over a time-scale relevant to forecasting (no longer than 12 days). Figure 21a shows the results of several 3D simulations, in which the weakest TC at  $t = 12$  days corresponds to the simulation with the greatest value of  $C_k/C_D$  (specifically,  $C_k/C_D = 2$ ). Footnote 18 suggests that this result contradicts Bryan's conclusion that the maximum intensity generally increases with decreasing  $C_D$ . I am not sure that I agree with this assessment, since Fig. 3 of Bryan 2012b appears to show that the time required to reach peak intensity with  $C_k/C_D \approx 2$  (and with an SST of 27 °C) is close to 20 days. Figure 17 of Bryan 2012a seems to provide further evidence (obtained from 3D simulations with an SST of 29° C) that late-time intensity increases with decreasing  $C_D$  for values of  $C_k/C_D$  between 0.25 and 2. So, it would seem safer to say that the 3D TC with  $C_k/C_D = 2$  in Fig. 21a could become stronger if the simulation were continued for a sufficiently longer time period.

### Secondary Comments:

**1.** Some of the figures [e.g. Figs. 9, 19 and 23] are too small. I had to enlarge them by 200-300% to clearly see the details discussed in the text. I suggest that the technical editors enlarge all of the busy figures.

**2.** I caught a few scattered typos that could be fixed by running the manuscript through another spell-check.