RE: acpd-13-C4415-2013-supplement.pdf "Second Referee Report"

Introduction

__Title:__ Asymmetric and Axisymmetric Dynamics of Tropical Cyclones

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__Journal:__ Atmospheric Chemistry and Physics

__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 Σ) 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 Σ and $\partial_t\langle v \rangle$ could be of order unity in various regions of the eyewall above the boundary layer (BL). In addition, Σ seems to be much greater than $\partial_t\langle v \rangle$ near the surface, in the vicinity of the RMW. Similar differences between Σ 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 Σ 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 Σ plots.***

Response:

The reviewer's question has prompted us to reexamine this issue. On review, we have identified three significant causes for error in the calculation. 1) Time sampling introduces a random error. 2) Recreating the modeled parameterization of subgrid scale mixing and of surface fluxes. 3) Use of centered spatial differences to compute advection tendencies rather than the 5th-order upstream advection scheme used in the CM1 model. Estimates of the error –type 3- in areas of large second derivative of the tangential wind, or of large change in the advecting wind in the direction of motion, indicate that the error can be of order 50%. The nature of the errors of types 2 and 3 is such that they tend to overestimate the tendency, but not to reverse the overall spatial gradients of the tendency or change the overall sign of the tendency. We now note these errors of the calculation in the text.

Implemented edit (p13361):

> Figure 10h shows the azimuthal-mean tangential wind tendency from model output (the left hand side of Eq. (12)), while Fig. 10i shows the corresponding tendency diagnosed from the sum of mean and eddy terms plus the subgrid scale (boundary layer and diffusion) processes (the right hand side of Eq.(12)). The two panels agree reasonably well quantitatively, although three sources of error in our calculation must be acknowledged. These are the sampling of the output data, the evaluation of parameterized internal diffusion and surface fluxes, and the use here of centred spatial differences to calculate advection, whereas the CM1 model uses a 5th-order upstream

advective scheme. The nature of these errors is not to change the overall sign or reverse the overall direction of the gradient of these computed tendency fields, but the errors become most apparent in the boundary layer when the storm approaches a mature intensity and where both the second and third sources of error are especially prevalent. For the time interval shown in Fig. 10, the maximum tangential winds are found to reside in the eyewall region near the top of the boundary layer, where the radial spin-up mechanism associated with the sum of the vorticity influx and vertical diffusion terms is a maximum.

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.***

Response:

Comparing the spin-up function with the gradient wind tendency is certainly expedient, since the latter quantity is readily computed from a single analytical formula. We propose also that S bears some relationship to intensity change, because these two quantities track each other in the 3D case fairly well. In part, this behaviour illustrates how inadequate the strictly AX representation of convection is. As for the reviewer's proposal to compare the gradient wind tendency observed to that found from an "axisymmetric balance theory," we do not understand completely what the reviewer is suggesting. We see at least three possibilities: we could perform an "inversion" for the axisymmetric geopotential tendency (above the boundary layer) using the axisymmetric spin up function as calculated in the current mss.; we could develop a more complete calculation of the spin up function that accounts for the mean heating rate and also the rectified eddy contributions thereof (following Shapiro and Montgomery 1993 JAS for weakly asymmetric dynamics); or we could perform an inversion for the geopotential tendency using the more complete spin up function that accounts for both the mean heating rate and rectified eddy contributions (following McWilliams et al. 2003 GAFD). Any one of these courses of action requires substantial new calculations worthy of their own treatment in a separate manuscript.

Implemented edit: none

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.***

Response:

RE: “in the eyewall, which seems to be less than 10 km wide at late times.” We don’t see any evidence that the inner edge of the updraft at 1km height is any less than 15 km radius, thus the eye is no less than 30 km wide.

RE: “explaining why 3-km horizontal grid spacing is deemed sufficient to realistically simulate the fluctuations of interest” We interpret this as either the reviewer thinks we made this claim without justification, or that the reviewer thinks that with better resolution we’d be better able to resolve the fluctuations in the eyewall. We cannot find evidence that we made such a claim anywhere in the mss. However, we do believe that with higher resolution the 3D simulation would better model reality, by representing fluctuations on the size of a few kilometers. We chose 3 km grid spacing as a compromise between having high spatial resolution and the required computational expense at the time we started this project. (Actually, we carried out the first calculations with a 7 km horizontal grid spacing believing that such grid spacing was sufficient to produce a simulated hurricane vortex with an adequate representation of asymmetries. The 3 km solutions are certainly more defensible for representing deep cumulus convective processes.)

Implemented edit (p13337):

> There are two principal numerical simulations: (1) a three-dimensional simulation at 3 km horizontal grid spacing on the interior grid mesh (hereafter called “3D3k”) and (2) a corresponding axisymmetric simulation at 3 km radial grid spacing (hereafter called “AX3k”). The 3 km grid spacing is sufficient to produce a simulated hurricane with a variety of asymmetries including eyewall asymmetries necessary to examine the role of asymmetries as opposed to axisymmetric processes in the intensification of a simulated tropical cyclone. Other sensitivity experiments are detailed in Sects. 3 and 7.

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.***

Response:

We use the term “mature intensity” to represent a range of values about the large values of intensity associated with the maximum of our time series. We have purposefully avoided calling this a “maximum” intensity. First, sufficient random realizations of slightly different initializations in the same environmental conditions may produce some storms with greater or less intensity and these absolute maximum intensities will generally occur at different times. Second, we cannot dismiss the possibility that if these simulations are extended for a period of a month or more, a more intense storm may eventually result. However, the maximum absolute intensity over all random realizations is not our main target in this study. Our objective is to understand the dynamics of tropical cyclone spin up and maturity in the 3D and AX configurations on realistic forecast times scales and to understand the dependence of these processes on the key phenomenological processes and parameters, such as CD, f, etc.

Revised footnote 18 (p. 13380):

> In a very recent paper, the results found by Montgomery et al. (2010) were criticized by Bryan (2012) on the grounds that the calculations were not run long enough to achieve a steady-state solution in which the vortex intensity is at its absolute maximum value. As discussed in Smith et al. (2012, 2013) and in footnote 9, we believe it is questionable whether a steady-state solution exists. Nevertheless, the calculations summarized and shown here using the CM1 model are for a significantly longer time period (12 d, the same length of time as most simulations published in Bryan (2012)) and more than sufficient to address the intensification and mature phase for realistic forecast time scales. Our findings support further the results and interpretations of Montgomery et al. (2010) and Smith et al. (2012) for this problem posing that is not focused exclusively on the absolute maximum steady-state intensity at some long time (> 12 d). In particular, the 3D results to be shown here are counter examples to the implied conclusion of Bryan (2012) that the maximum intensity for realistic forecast time scales is inversely proportional to C_D(see Fig. 21a).

New reference:

> Smith, R. K., Montgomery, M. T., and Persing, J.: On steady-state tropical cyclones, Q. J. Roy. Meteorol. Soc., submitted, 2013.

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.***

Response:

We would like to point out to the reviewer that we have repeatedly fought with the publisher for larger plot sizes as well. We regret the frustration that this may have caused the reviewer. Our figures are designed for portrait paper and not landscape, so in the end they will print out well (at least in our development of these figures using the Copernicus templates for final publication format.)

2.

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

Response:

A fresh check caught these, which will be edited:

- “Boussineq” p. 13366
- “funtion” p. 13353
- “paramaterization” p. 13352
- “wildy” p. 13340
- “should should” p. 13335