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Interactive comment on "Idealised simulations of cyclones with robust symmetrically-unstable sting jets" *by* Ambrogio Volonté et al.

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The paper investigates the development of sting jets (SJs) within idealized cyclones simulated with the MetUM model. There are only very few studies on idealized sting jets and such a study is very welcome. The model set up is close to a previous study made by the same team a few years ago (Baker et al. 2014) but some improvements in the definition of the initial state have been accomplished by more accurately setting a thermal wind balance. The implementation of this new initialization procedure allowed the authors to perform a large set of sensitivity experiments by changing the jet strength, the surface temperature, the initial humidity, and the resolution. The description of the initialization procedure is however at times a bit hard to follow. Some steps should be more precisely described to be more readable (see my first main comment).

Before presenting the sensitivity experiments, the study is focused on one control experiment with a given set of parameters. The first objective of the analysis of the control experiment is to show the existence of a sting jet and to present its characteristics. Then, a subsection is dedicated to studying mesoscale instabilities like conditional symmetric instability (CSI), symmetric instability (SI), inertial instability (II) and conditional instability (CI). A deeper investigation of the dry instabilities (SI, II) is performed to show that the mechanism provided by Volonté et al (2018) based on the real storm Tini is herealso at play. This mechanism provides an explanation for the formation of negative regions for the vertical component of the absolute vorticity via the role played by the tilting term in the vorticity equation. I think this part could be improved in terms of the presentation and choice of the figures (see my second main comment). Finally, the analysis of the large set of sensitivity experiments constitutes the most original aspect of the paper. It helps improving the dependence of sting jet formation on large-scale environmental parameters (jet strength, humidity, surface temperature) and model resolution. The analysis is accurately made with a good choice of figures. The main result is that a more intense jet and a higher resolution help increasing both the SJ strength and the volume of unstable regions even though there is no one to one correspondence between the SJ strength and the level of instability. The results of this study certainly improve our understanding of sting jet formation. I consider the paper as worth for publication after considering the following comments, some of them being major. My main criticism is that some paragraphs/sections are tough to read and should be clarified.

Main comments:

1) Presentation of the initialization procedure.

a) Section 2.1.1. Is there a reason why a well-balanced baroclinic jet is more difficult to obtain in the MetUM model ? Since several pages are dedicated to this problem, it would be good to precise if it is a general problem. Please mention if it is a difficulty to be encountered when working with a non-hydrostatic model. I have some doubts because Polvani and Esler (2007) used a primitive equation model and the present study is said to be inspired by Polvani and Esler (2007). Also, could you mention the type of instabilities met in Baker et al (2014) ? Another aim of the initialization procedure is to set a given surface temperature in the centre of the domain. Lines 100 to 105 are not fully clear to me.

b) Section 2.1.3. I was often confused in this section about what we are looking for. What are the known variables (u ?) and what are the unknown ones (theta_v(r,phi)?) ? Maybe after Eq. (19), some sentences would be useful to add to better understand the steps that are following Eq. (19). In my mind, once u and theta_vˆref(r) are prescribed, we have theta_v(r,phi), so we have everything we need. So I was really lost between lines 185 and 205. And then when the procedure is applied to the chosen case, does Eq. (27) correspond to the definition of theta_vˆref(r) ? Do Eqs. (28) to (33) correspond to the definition of u ? If yes, it means that theta_v(r,phi) is known from Eq. (19), isn't it ? Maybe the details of some steps could be put in an appendix and only the most important aspects of the procedure are kept in section 2.1.3.

2) Mechanism leading to the formation of II and SI unstable regions.

a) Lines 500 to 510 are tough to understand. This is another paragraph where I was lost. I think it would be useful to show the vertical velocity in Figs. 6 to 8.

b) I would encourage the authors to compute the tilting term and to show it in maps. If I well understood Fig.8a corresponds to -dv/dp and Fig.8b to du/dp and then the tilting term should be du/dp domega/dy - dv/dp domega/dx. So we need to visualize omega to understand how the tilting term is. Since it is the key finding of this section, I found a bit too bad not to spend more time to slowly present the whole detailed arguments to the reader.

3) The relationship between descent intensity, SJ intensity and mesoscale instabilities.

a) I agree that if there is a larger volume (or number of trajectories) satisfying the instability criteria, we should see stronger descents. This relationship is clearly seen

in Figure 11d. My concern is more about the Delta|U| during the descent. Why do we expect to get a stronger Delta|U| ? I understand that more unstable regions for SI should lead to more intense transverse circulations but what I do not understand is the acceleration of the along-flow wind speed in presence of such instability. Maybe my question is related to my lack of knowledge on SI in a 3D context, but I think some comments on that would be beneficial for the reader. In other words, the relationship between the slantwise instability across the flow and the along-flow wind speed is not clear to me.

b) Line 725: this is an example of straighforward remark that I do not understand "This acceleration suggests a link with mesoscale processes on top of the synoptic evolution".

Minor comments

1) Line 47: at this stage, it is not clear why a focus is made on dry instabilities rather than moist. Maybe you should mention that you want to check the relevance of a mechanism seen in the real storm Tini.

2) Lines 53-54: I think the end of the sentence has a strange structure and should be reworded

3) Line 82: "the strictly" –> "be strictly"

4) Line 87 "of descent"

5) Line 101: please explain a bit more the problem encountered in Baker et al. (2014). See my main comment above.

6) Line 176: phi_s should be defined.

7) Lines 194-195: I do not fully understand the sentence. By constant value, do you mean independent of x and y ?

7) Line 195: Why is the result of Eq. (19) not enough to define theta_v(r,phi) ? See

main comment above

8) Lines 273-274: what kind of structural changes do we have inside the cyclone by changing the wavenumber ?

9) Line 276: I think the authors should mention they want to check the relevance of a mechanism for the formation of unstable regions for dry instabilities revealed in Volonté et al. Otherwise, it is not clear why a focus is made on dry instabilities

10) Figure 3: It is not clear why levels 850 hPa and 805 hPa are shown. I would prefer to see one level linked to the SJ (e.g., 805 hPa) and another in the boundary layer linked to the cold conveyor belt jet.

11) Line 389: The low-level wind maximum described in the previous paragraphs is the SJ wind maximum, isn't it ?

12) Line 402 and in the rest of the paper. At which level is the SJ wind speed defined ? Is it a given isobaric surface ? Or is it case-to-case dependent ?

13) Line 410: please insert (Fig. 4a) just before ", indicating"

14) Lines 457-458: why does this information important ?

15) Figure 5: At t= 70-75h, before the ascent, the percentage of CSI is quite high. What does that mean ? Is it relevant for the ascent ?

16) Line 504: I do not understand "the direct consequence"

17) Caption Figure 7: "Fig. 5a" –> "Fig. 6a"

18) Caption Figure 8: "Fig. 5a" –> "Fig. 6a"

19) Lines 505-510: paragraph to be reworded by diluting the information (see main comment 2) above).

20) Lines 527-531: how are all these conditions implemented in terms of maths ?

21) Line 662: please be more explicit to say why t299 case is an outlier.

22) Lines 671-673: A large part of the descent is azimuthal as well, isn't it ? These lines are difficult to follow

23) Line 715: I think the static stability issues are never described in the present paper. One or two sentences would be welcome.

24) Line 725: the suggested link is not clear to me (see my main comment above)

reviewed by Gwendal Rivière

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