Review of 'Idealised simulations of cyclones with robust symmetrically-unstable sting jets' by Ambrogio Volonté, Peter A. Clark, and Suzanne L. Gray

Overview:

Idealized simulations of cyclones are analyzed here with particular focus on the cause of the low-level wind jets. It is concluded that the strong winds resulted from a sting jet. The diagnostics used to analyze this case have been used previously in other published cases of sting-jet cyclones by these authors. There is a rather significant problem with the model simulation initial condition that will require all the simulations to be performed again. The interpretation of the results is a little superficial in places (see detailed comments) and it is unclear why the focus is on dry symmetric instability when moist instability, synoptic-scale forcing, and frontal forcing cannot be ruled out; further diagnosis will be required. The literature cited is incomplete, ignoring previous contributions by other authors, neglecting other cases from the literature that are inconsistent with their results, and not citing contradictory statements from the authors' own research.

Overall, therefore, I find the argument plausible but unconvincing. More precision is needed, or, at least, more caution. Further calculations of the other factors leading to sting jets is needed. While interesting model simulations undoubtedly exist here, I believe that the degree of revision needed constitutes at a minimum 'major corrections', if not 'reject and resubmit'.

Major comments:

L47 Given that the authors had emphasized the importance of moist symmetric instability in sting jets in their previous publications (e.g., the sting-jet precursor depends strongly on the occurrence of CSI) and in L314–315 ("moist processes occurring in the cloud head have a primary role in the evolution of the cyclone in which the SJ occurs and are instrumental in the SJ generation mechanism"), this emphasis on the dry instabilities only is unclear to this reader. Figure 5 shows that between (1) and (2) that CSI, SI, and II are all equally important. Moist instabilities need to be considered equally, and the focus of the manuscript needs to be changed accordingly, requiring substantial rewriting.

Figure 1 The layer between 400 and 300 hPa appears to be absolutely unstable (i.e., potential temperature decreasing with height). Abrupt and nonuniform gradients of static stability occur within the stratosphere, as well. With the emphasis on the role of instabilities in the cyclone, initializing a model with such a large region of instability should raise a concern. Other initial conditions for idealized cyclones do not show static instability in the upper troposphere (e.g., Fig. 3 of Thorncroft et al. 1993; Fig. 3 of Schultz and Zhang 2007, DOI: 10.1002/qj.87; Fig. 1 of Coronel et al. 2016), so why the authors chose such an unusual set of initial conditions is unclear. Even the set of initial conditions from Polvani and Esler (2007, their Fig. 2) – which the authors claim their "initial base state...is inspired by" and is in return inspired by that of Thorncroft et al. (1993) – has smooth potential temperature gradients throughout the stratosphere and no instabilities in the troposphere. (With such large differences in the initial base states, in what way were your initial conditions "inspired"?) The initialization of such a deep layer of absolute instability then raises the question of whether the model is initialized with any moist instabilities, an analysis of which is lacking in the present manuscript. The model simulations should be redone with any dry or moist instabilities absent in the initial conditions.

Figure 4 and its accompanying text In Clark and Gray (2018, p. 954), the authors write about a modeled sting jet in which "the acceleration amounts to no more than about 2 m/s/hr, but acts over a very slow descent (over more than 12hr); so these trajectories only loosely resemble SJs in observed systems." The air in Figure 4 descends over a 12hr period (86–98 h) and accelerates from 20 m/s to a maximum of 38 m/s (1.5 m/s/hr). Therefore, to be consistent with the authors' previous publications, the trajectories within this simulation should be described within the present manuscript as "loosely resembling a SJ in observed systems".

L341 Can the authors clarify within the text what they mean by "irregularities"?

Section 2.1 Maybe I missed it, but can the authors state how the lower boundary condition was modeled? Is it flat land or ocean? How is the temperature of the surface specified, and is it fixed over time or allowed to vary? How are heat and moisture fluxes handled at the surface?

L383 The trajectories for the sting jet are selected from a height of 805 hPa. Given that a sting jet is a surface expression of a region of strong winds (and hence the near-surface damage potential), it is unclear why the height for selecting sting-jet trajectories is so high. Should these not be selected much closer to the ground, or at least immediately above the boundary layer rather than a height of about 2 km above the surface?

L434 The authors report that there is no clear cooling signal due to evaporation or sublimation. However, in Clark and Gray (2018, p. 948) they write that "it is not clear that this [sublimation of ice] differs dynamically from CSI." Given that they've chosen not to focus on CSI in the present paper, yet Figure 5 shows 20–30% of descending air parcels have CSI, how do the authors reconcile these apparent contradictory statements and model results? Is CSI the dynamical equivalent of the sublimation of ice, as they previously wrote? And, is CSI/sublimation present (or important) in the simulations described within this paper or not?

L476–479 and throughout the manuscript The authors state the number of trajectories unstable to dry mesoscale instabilities is "substantially smaller" than that of a previous case and therefore concludes that "the release of mesoscale instabilities such as SI and II takes part in the dynamics of SJ speed increment and descent". These two statements would seem to be contradictory. Moreover, these statements also contradict, for example:

- L8–9: "A substantial amount of SI...is released along the SJ during its descent...."
- L519–520: "mesoscale instabilities...play an active role in the evolution of the SJ...."
- L667–668: "it is difficult to assert a strong relationship between...SJ maximum speed and degree of SI."
- L683–684: "there seems to be overall some evidence that weakly enhanced SJ strength is associated with increased SI".

Given the degree of inconsistency among these various statements within the manuscript, is the word "robust" in the title of this manuscript appropriate?

Thus, more clarity and consistency on the degree and importance of the dry (and moist) instabilities in relation to the acceleration of the SJ is needed throughout the manuscript.

Section 3.2 Here it is concluded, "There seems to be overall some evidence that weakly enhanced SJ strength is *associated* with increased SI, but clearly other processes are occurring in the different cases to complicate behaviour." It's not clear to me how I've learned anything useful from this analysis. First, weak and vague words ("seems to be", "some evidence", "associated", "other processes", "complicate behavior") obscure the meaning of this sentence and the actual results. Greater precision is needed when writing such important conclusions.

Second, given that the authors admit that synoptic-scale and frontal-scale circulations are in part responsible for this descent (L618–619) and that the initial strength of the jet stream, and hence of the cyclone, has changed in these simulations, then the authors cannot rule out that the magnitude of the forcing has increased and is the major contributor to the differences in the accelerations of the jets across these sensitivity experiments. The analysis within the present manuscript does not tell us what is causing the acceleration to be strong in these regions. Schultz and Sienkiewicz (2013) discuss this issue in their paper (see p. 604). At a minimum, the authors would appear to need to calculate the synoptic and frontal forcing to determine if changes in these can explain their model results. Simply concluding that "several environmental factors modulate this relationship, making it difficult to disentangle the net effect of instability release" (L765–766) undermines the basis of their study. Such a sentence would appear to be a weak concluding statement when such factors could be calculated and examined. After all, the purpose of a scientific paper should be to shed light on these factors for the benefit of the readers, rather than be defeated and conclude such a problem is intractable.

L16–17, but throughout the manuscript Are you comparing with other analyzed cases of sting jet cyclones here when you say that the sting jet in your case is robust? If so, then the literature is not so clearly uniform on this issue of instabilities in cyclones. Some model simulations of real cyclones with sting jets (e.g., Smart and Browning 2014; Brâncuş et al. 2019) found little to no instability associated with the sting jet. What does the absence of instabilities in observed cases mean for the authors' conclusions? The authors should express that ambiguity more clearly throughout the manuscript because idealized simulations, as informative as they are and therefore commonly used, do not often represent reality. More care needs to be taken to avoid overgeneralizing the results of this study.

Minor comments:

L25–26 Schultz and Browning (2017) (DOI: 10.1002/wea.2795) argue that one cannot identify a sting jet from the surface observations alone and should be cited here.

Section 1 It would seem appropriate to cite the comprehensive review of conditional symmetric instability (as well as other instabilities) by Schultz and Schumacher (1999) (DOI: 10.1175/1520-0493(1999)127<2709:TUAMOC>2.0.CO;2) somewhere in the introduction.

L46 I suggest that the paper by Schultz and Sienkiewicz (2013) (DOI: 10.1175/WAF-D-12-00126.1) is cited here as this is the first paper I know of that has discussed the importance of frontolysis.

L81 Brâncuş et al. (2019) (DOI: 10.1175/MWR-D-19-0009.1) and Eisenstein et al. (2019) (DOI: 10.1002/qj.3666) also considered the importance of these instabilities and should be cited here.

L427 There are many other papers on modeled sting jets that could be included here - all consistently showing that the descent here is consistent with that in previous studies. Thus, it is not correct to say that the results found in this case are the same as in other cases and cite only the Volonté et al. paper. Whether or not you do this, I suggest you reference the paper by Slater et al. (2017) (DOI: 10.1002/qj.2924). The Slater et al. paper considers the same case study as the Volonté et al. (2018) paper and has higher descent rates and accelerations to that being cited here. The authors would argue that the model used to analyse the Slater et al. cyclone has insufficient resolution to allow a sting jet to form if associated with any mesoscale instability, but it does have the resolution to produce descent and acceleration due to frontolysis.

L434 There are many other papers on observed and modeled sting jets that could be included here - all consistently showing that cooling is minimal (e.g., Smart and Browning 2014; Coronel et al. 2016; Slater et al. 2017; Brâncuş et al. 2019). On the other hand, Eisenstein et al. (2019) found cooling was much more important in their case. This diversity of results should be discussed.

L614–615 Schultz and Browning (2017) (DOI: 10.1002/wea.2795) argued that the wind maximum of a SJ needed to exit the cloud head and accelerate, and should be cited here.

L704–705 In addition to the citation to Coronel et al. for recognition of the importance of the synoptic-scale forcing, I suggest adding a sentence citing the paper by Schultz and Sienkiewicz (2013) (DOI: 10.1175/WAF-D-12-00126.1) as the first paper I know of that has discussed the importance of frontolysis as a forcing mechanism for sting jets.

L720 There are many other papers on modeled sting jets that could be included here - all consistently showing that the descent here is consistent with that in previous studies. Whether or not you do this, I suggest you reference the paper by Slater et al. (2017) (DOI: 10.1002/qj.2924). The Slater et al. paper considers the same case study as the Volonté et al. (2018) paper and has higher descent rates and accelerations to that being cited here.

L731–732 and throughout the manuscript Not all sting jet cases are associated with dry instabilities. You should cite relevant literature by other authors that show other cases with negligible amounts of these instabilities (e.g., Smart and Browning 2014; Brâncuş et al. 2019). Consider other statements within the manuscript that should be similarly reworded with additional caveats and citations.