Review of Bidou et al, ACP, 23/02/26

This study investigates the effect of low-level wind shear on characteristics of deep convection by using a set of LES experiments initialised with different mean wind profiles. Strengthened low-level shear is shown to have a positive feedback on multiple measures of convective intensity. The overall tone of the article is primarily descriptive: as the authors themselves state (L284), they “mainly [focus] on different diagnostics of convection rather than deeply analysing the physical mechanisms”. This is perfectly acceptable – however, my primary criticism of the article is that this topic has been explored extensively in the literature, with similar LES studies stretching back to at least the 1980s. The present article acts as a coherent review and demonstration of known mechanisms in a single setting; however, it does a generally poor job of appropriately referencing prior work and identifying the analysis here that is novel. My impression is that only the TKE results are novel. A thorough literature review, and clearly-identified discrimination between known and new findings, are both required to make this article suitable for publication. I recommend at least Major revisions be undertaken.

Major comments

Literature review: Essential prior literature in this field, particularly regarding work from the present decade, is either missing or not cited extensively enough. In general, the article feels a little outdated in its implicit knowledge of the field, ignoring important modern development and context. That several such papers have come from French groups which are also part of CNRS makes this all the more baffling! A thorough review away from the article is required, which should then be translated into a more complete Introduction and Conclusions, and improved references for findings that are re-affirmed in the Results section. Here are some key elements to work on:

- Effect of low-level shear on entrainment. On L160 the authors end their analysis of Fig. 6 by stating “This is consistent with the previous hypothesis of shear reducing entrainment dilution of the convective cores”. This hypothesis was first posed, and verified in LES simulations, by Mulholland et al, JAS (2021). It has further been verified in km-scale explicit simulations (Maybee et al, GRL (2024) & QJRMS (2025)). Some of these articles are cited, however not for this key physical mechanism. The Mulholland paper, in particular [I am not an author], uses a very similar framework to this article, varying initial state shear for a set of LES experiments, to rigorously investigate the relationship between entrainment, shear and convective intensity. This must be much more clearly acknowledged throughout this article.
- I was very surprised, and disappointed, to find that the authors have neglected recent articles [with which I was not involved] led by Sophie Abramian from the group at the Laboratoire de Météorologie Dynamique; in particular, Abramian et al, GRL (2021); and Abramian et al, JAMES (2023). These papers use LES simulations to verify physical hypotheses regarding the orientation of squall lines relative to low-level shear, and the contribution of shear-modulated dynamical controls (entrainment) to changes in extreme rainfall in increasing-shear environments.
- The authors do make reference to some theories for understanding the role of shear in convective organisation, such as the classic RKW theory. They have neglected a more recent key theory whereby shear modulates the low-level inflow into convective systems and thereby convective characteristics (Alfaro, JAS (2017)). This theory

offers testable metrics relating to maximum updraft velocities and precipitation; its predictions were independently tested in LES experiments initialised from observed soundings by Bickle et al, JAS (2022). It would be particularly interesting to consider an analysis of the Alfaro metrics in the context of the different up/downstream TKE values.

- There is now an extensive body of literature surrounding shear controls on deep convection in satellite observations/reanalysis and km-scale convection permitting models. Such work provides an important motivation for the kinds of experiments conducted in this article, since they can inform understanding of new modelling systems or observations. Some reference to this context would improve the Introduction and its motivation of the article. Consider e.g. Klein et al, ERL (2021); Chen et al, GRL (2023); Maybee et al, GRL (2024); Muetzelfeldt et al, JAS (2025). And from CNRS, Roca et al, JGR-A (2025)!

Supercells: The authors describe the high shear experiments (W20_S144 and W15_S106) as generating supercells. How have the systems been diagnosed as supercells? I see no presentation of quantitative evidence that suggests supercells, with Fig. 5 showing systems simply with coherent organised updrafts. As stated in Markowski and Richardson (2011), "*…consensus now seems to favor a dynamical criterion in place of a longevity criterion for classification of a storm as a supercell. The widely accepted dynamical criterion is the presence of a persistent, deep mesocyclone within the updraft. A mesocyclone is a region of vertical vorticity with a characteristic width of 3–8km and magnitude of O(10^{-2}) s^{-1}.*"

Please could the authors demonstrate that the storms labelled as supercells meet such a dynamical criterion? If a deep mesocyclone cannot be detected or demonstrated in these systems, I would ask that all references to supercells be removed from the article.

Analysis methods: The present article does include a Methodology section, which describes the MesoNH model and the experiments that form the data for the paper. However, no details are given of any of the analysis methods used throughout the Results section. This undermines much of the Results section, as the nature of what is being shown is ambiguous. In particular, details should be provided regarding:

- Identification of updraft cores. Is there an area threshold on a contiguous ascending domain at a set level, is this all w>0 points, etc? I cannot fully interpret some figures as I am unclear on this point.
- Entrainment profile calculations. The bulk plume method mentioned in context of Fig. 6 should be briefly described, explicitly highlighting the key technical details which underpin the Results. It was disappointing to have this relegated to a citation when these results play an important role.
- Organisation indices and 2D cloud masks. How exactly are these calculated? What are "void sizes"?

I note that some details here may be in the appendices. Please move them to the main text so that readers do not need to refer to appendices, these methodological details are important for interpretation of the results.

Other comments

- L21 “all studies agree on this” – this language is quite loose, and some studies do not agree on the effect of shear on rainfall. I suggest rewording.
- L35 “Entrainment itself is strongly modified under shear” – important missing citation, Mulholland et al, JAS (2021), see Major comments.
- Introduction – missing references as in Major comments. But also, I was surprised that no mention is made as to the relationship of shear to observed species of deep convection and their organisation. In particular, where do Mesoscale Convective Systems, the archetypes of organised convection and shear beneficiaries, fit in?
- Section 2.2 – It may be worth highlighting your experiment naming convection explicitly to the reader? While reading the results it took me a little while to twig that the labels display the mean winds and shear for each experiment.
- Figure 3/L107 – See Major comment; how did you identify “updraft cores”?
- L117 “those two” – too informal and clear, please state the experiments explicitly.
- L119-120 “producing a few large updraft cores with very strong vertical velocities….These supercells correspond to the maxima shown in…” – see Major comment, how have you diagnosed the systems as supercells? Where is the evidence of strong vertical vorticity?
- Fig. 6b – please reformat the correlation values, this many decimal places is entirely meaningless. I suggest 2s.f. The Figure caption should really be more detailed in describing how the lines/r^2 values relate to the marker shapes, too.
- L156 – Correct “bulk plum” -> “bulk plume”. More importantly, I am not familiar the method for computing the provided entrainment/detrainment profiles and as a general reader would expect such detail to be provided. See Major comment.
- L193-194 “Shear may cause the environmental wind to exceed the storm motion, which could explain a higher TKE upstream of the convective cores” – See Major comment, please discuss this in the context of the theory of shear modulated of low-layer inflow posed by Alfaro, JAS (2017).
- Figure 11 – The caption only describes a single line in the top plot of each panel. What is the bottom line plot? And what are the different lines within each plot? The figure looks very pretty, but it’s extremely hard to pull a concrete scientific message out of. The panels are also quite similar between simulations. I would strongly suggest finding a better method to display this information; my suggestion would be box or violin plots, which will being less aesthetic are more quantitative. You could group different experiments’ distributions at common timesteps, and choose six key timesteps, to convey the same data but in a clearer form.
- L216/Fig. 12 – What exactly is being plotted here? By context it is the same as Fig. 11, which is virtual potential temperature in the first vertical level, which is not the same as the surface. Yet the text refers to “cool[ing] the surface more efficiently”. The first vertical level is not the same as the surface? Are you plotting surface or skin temperatures?
- L230 – suggest adding Biagioli and Tompkins, JAS (2023) to reference list regarding difficulties of quantifying organisation.
- L244 “we derived 2D cloud masks from our 3D fields” – see Major comment; how?

- L247 “Tendencies appear on these graphs” – suggest removing and rewriting this sentence, as it is extremely unclear what meaning is being conveyed.
- L249 – See Major comment; what are void sizes? You must be more detailed when introducing new methods and metrics.
- Fig. 14d – I strongly recommend removing this panel, it serves no purpose beyond showing that an arbitrarily chosen metric conveys little information, and thus detracts from the rest of the figure. If you wanted to replace the panel, maybe the closest thing to a “single indicator of organisation” (L240) in the sense of its position as a standard metric is *Iorg* (Tompkins and Semie, JAMES (2017)). There are many issues with this metric, however it is designed for and frequently used in LES settings such as your own. Plotting this could be a nice way of providing a clearer panel that should synthesise information conveyed in the other three.