

# ***Interactive comment on “WRF-Simulated Low-Level Jets over Iowa: Characterization and Sensitivity Studies” by Jeanie A. Aird et al.***

**Anonymous Referee #2**

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## **1 General**

The paper presents an overview of LLJs in Iowa in winter and spring. The paper is well written, although a bit difficult to follow in some places. My main criticism is related to goal 1 and 3 (see line 76-80) of the paper:

## **2 Major comments**

- Goal 1: to define a climatology one has to use at least a year of data and preferably more (to capture all relevant mechanisms). The usual definition of a clima-

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tological period is 30 years. Also in the context of wind energy the turbine life time is generally >20 years. It is quite likely that also summer time jets are quite abundant, if not more, than during winter and spring. For example in the cited paper of Baas et al. (2009), most LLJs were observed during summer. If you don't use a full year of data the paper is just a case study and in that case I don't think it contains enough novelty to publish the results.

- Goal 3: I agree with the paper that the detection could depend on resolution, but I was expecting to see a proposition of a method to help diagnosing the jet independent of resolution. At least something better than linear interpolation should be tested (see comment below).

### 3 Minor comments

- I7: I find it a bit confusing that the abbreviation LLJ is both used to indicate singular and plural. Maybe better to use LLJ for singular and LLJs for plural?
- I27: This is usually referred to as baroclinicity, please add that term
- Table 2: It is not really clear to me whether these criteria are used separately or together. If they are not used together, you should put them in separate tables.
- I87: To represent a real climatological study one should at least cover all seasons.
- I231: This discussion would be much more interesting with some more physical interpretation. If you plot geostrophic wind speed and thermal wind speed in Fig. 5 it becomes clear if this mechanism plays a role here.
- I261: The explanation of this figure confusing and had to read this section several times to understand what was being plotted in Fig. 7. I am I correct that for group 2, approx. 60

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- I294: "differs markedly" -> I can hardly distinguish any differences in Fig. 9. It would be more clear with a difference between the two plots, but also then I would probably not call it a marked difference. It seems it would be 1-2
- I328-330: This description is not very clear to me, maybe an equation would be better. So you normalize the wind speed profile by a maximum value in each grid cell and then calculate a frequency using the variable threshold and subtract those two frequencies? But then a difference of 0.1 is quite big, so it might be worth putting some more emphasis on that result in panel a?
- Fig. 10. This analysis requires the authors to use a simple polynomial fit or something similar to extrapolate the low-resolution case. Using a linear extrapolation in the points of the wind profile clearly does not reflect the non-linear behaviour of a LLJ profile.
- Conclusion: I was expecting to see some discussion on which method would be better or could be more suitable in certain conditions. The paper could benefit from a discussion section at the end of the results.

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