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Interactive comment on "A high-resolution unified observational data product of mesoscale convective systems and isolated deep convection in the United States for 2004–2017" *by* Jianfeng Li et al.

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This article details the development of a highly useful convective climatology for the eastern two-thirds of the continental U.S. Considerable effort is expended explaining the datasets used, potential sources of error and mitigation strategies, and the complex processing steps involved. Future applications for the dataset are described. I am concerned about the design of the algorithm not separating organized from unorganized isolated convection, potentially leading to impacts in the precipitation intensity calculations. Pending responses to my comments enumerated below, I recommend

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acceptance bending major revisions.

Major comments:

1. The IDC category is potentially lumping unorganized isolated convection in with highly organized isolated convection such as supercells. I can certainly see the authors' points that MCSs are larger systems than all IDC, and thereby should result in larger transport, circulation, and accumulated precipitation impacts (e.g., Lines 80-82), but citation of a few studies in the literature to that effect would be useful so as to not argue from intuition alone.

Regarding severe weather, however, including tornados and large hail, MCSs are not the primary generators. Instead, supercells are (e.g., Wurman et al. 2011 BAMS). Furthermore, supercells are increasingly recognized as producers of heavy and extreme rainfall (e.g., Hitchens and Brooks 2013 AR; Smith et al. 2001 JHM). The conflation of two dynamical storm classes when evaluating the impacts of IDCs has potential impacts on the authors' discussion of precipitation intensities (e.g., Figs.3, S9; Sections 3.2.1, 3.2.2). The precipitation intensity distribution for IDC events is in all likelihood a bimodal distribution, containing output from isolated non-supercells and supercells (e.g., Hitchens and Brooks 2013 AR).

Can the authors examine the IDC portion of their climatology to determine if there is indeed a bimodal distribution captured within? If not, why do the authors think that the FLEXTRKR algorithm failed to capture heavy/extreme rain events from supercells?

Is it possible to add an additional class to the FLEXTRKR algorithm to detect supercells specifically? How likely is it that supercells will be classified as IDC within this climatology? Supercells do frequently grow upscale into an MCS (e.g., Reif and Bluestein 2017): in such a situation, would the entire storm track be classified as an MCS in this climatology? How would these "misclassifications" impact the results?

2. I had trouble following exactly how the SL3D algorithm output was incorporated into

the climatology. Given its introduction at the start of section 2.2 I had first assumed its classifications were important to the FLEXTRKR algorithm in identifying the CCFs and PFs. After reading I'm no longer convinced it is used in that effort at all, but instead just incorporated into the climatology after the fact. How are the five SL3D categories, listed in lines 221-222, used to identify the CCFs and PFs (if they are)? If not, how are those categories used? On a display note, how does the data shown in Fig. 2e correspond to the five categories listed?

3. How is rain rate calculated? Is it based on assumed Z-R relationships from the GridRad data (which introduces a host of problems), or is it calculated by subtracting accumulated Stage-IV rainfall at successive hours? If calculated by subtracting rainfall accumulations, that has the unfortunately side effect of evenly distributing rainfall over the full hour, lessening peak intensities that occur over shorter periods of time. Given the temporal resolution of the datasets used here, that issue can't be avoided, but the authors should include a few sentences qualifying their discussion on precipitation intensity (beyond the issues already noted in major comments #1.)

4. Figures S1 and S5 are key to understanding the descriptions in the text and should be moved to the main article.

Minor comments:

* Lines 39-45: This is a nice summary of the wide-reaching impacts of deep convection. On a minor note, should multiple citations within one reference be provided in chronological order?

* Line 49: "deep convection associated thunderstorms" –> deep convective thunderstorms

* Line 201: Is the "nearestdtod" method essentially nearest neighbor? A brief one-line description would be helpful.

* Lines 229-231: A few sentences describing, theoretically, how CCFs and PFs differ,

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and what kind of features each of these is intended to represent, would be helpful. It wasn't always clear to me why essentially two separate datasets were being developed.

* Lines 290-293, Fig. 2: I was only able to understand the descriptions of the pixel-level information by reading the caption of Fig. 2. I'd move the description in the caption into the text and expand lines 290-293 by referencing each subfigure individually.

* Lines 345-346, 349, 356, 358: Six different proxies for convective intensity are used in a small section: convective precipitation area, convective 20-dBZ echo-top height, area with column max reflectivity >= 45DBZ, max 30-dBZ echo-top height, max 40-dBZ echo-top height, mean convective 20-DBZ echo-top height. Why are all these different proxies are being used – do the underlying results differ? I'd find it easier to read if the convective intensity results were all discussed in the frame of one proxy.

* Lines 363-370: While Section 3.2 is a good application of the climatology product, it isn't "a detailed examination of the 3D evolutions of MCS/IDC events."

* Line 391-393: Instead of using "stratiform" as the name for all precipitation not associated with MCSs or IDCs, I suggest the phrase "non-convective". "Stratiform" is confusing as there is stratiform rain within both MCSs and IDCs.

* Lines 451, 460, 466: Discussion of Figure S9 happens before that of Figure S8.

* Lines 565-567: Can the authors elaborate how each dataset is incorporated into the CCF/PF and CCS criteria listed in Section 2.2?

* Line 613: Instead of individual NEXRAD radar data, could the Multi-Radar Multi-Sensor radar dataset be used? (https://www.nssl.noaa.gov/projects/mrms/)

* Line 633: Section 3.2.2 –> Section 2.2.2

* Section 4.4: I appreciate the authors' testing of the MCS and IDC definition criteria and discussion of that criteria's impact on classified precipitation. I would recommend the authors urge caution of future researchers using this dataset to examine transport

or large-scale circulation impacts without conducting their own, similar analysis.

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