Response to Reviewer #1: Dr. Tomeu Rigo

Thank you for your careful and thorough reading of this manuscript and your thoughtful comments and suggestions. Our responses follow your comments (in *Italics*).

General comments:

The authors present a new methodology for identifying mesoscale convective systems based on the combination of three different sources: satellite imagery, weather radar volumetric mosaics and rainfall charts obtained from the merging of radar estimation and rain gauge values. The work results interesting but there are some points that should be solved before its accepting. One is the large number of typos associated with the table and figures references (what is S1, S2, …?).

Reply:

Thanks. Table Sx or Figure Sx means figures or tables in the supplement. There is a supplement attached to the main manuscript.

Second one is conceptual, and more important to me: when the authors define isolated deep convection, they do not refer in any case to supercells. Besides, while the limitations of the methodology about the spatial and temporal scales are minimized in the case of MCS (because of their extent and duration), the part of isolated convection does not look like solved as clearly. I think that the authors should try to explain better the limitations (if exists) about this issue or, at least, explain why the results are not affected by this point.

Reply:

Thank you for your comments. When we defined IDC, it was actually non-MCS convection events, consisting of many different types of deep convection, as we explained in Lines 295 – 300 in the revised main manuscript. Supercell is another topic. The well-known characteristics of supercells include hook echoes, bounded weak-echo regions, and the presence of strong rotation updrafts (Lynn, 2002; Naylor et al., 2012). Rotation recognition (or rotation-related variables, such as vertical vorticity, low-level-shear, azimuthal shear, etc.) is necessary for the automatic identification of supercells (Lakshmanan and Smith, 2009; Lynn, 2002; Smith et al., 2012; Stumpf et al., 1998). The source datasets used in this study do not contain Doppler velocities or environmental wind fields and cannot be used to calculate rotation. Besides, rotation is not involved in the current FLEXTRKR algorithm, making FLEXTRKR unable to recognize supercells. Here, we need to emphasize that there are no direct relationships between MCS/IDC and supercell. Both MCS and IDC events may show supercell features sometimes during their lifetimes. The advantage of our data product is that it provides detailed information of each MCS/IDC track, such as the location, time, and a series of convective feature characteristics (area, echo intensity, echo-top height, etc.) of the track. Users of the data product can further identify supercell features of the track by incorporating rotation datasets.

A limitation in the identification of IDC is related to the temporal resolution. The 1-hour temporal resolution is enough for MCSs, as they are generally large and long-lived. However, some IDC events associated with weak convection can be shorter than 1 hour, which can not be resolved by our data product. We discussed the temporal resolution limitation in Section 4.3 (Lines 658 – 683 in the revised main manuscript). We found that, from the perspective of precipitation, the IDC events with PF lifetimes shorter than 1 hour might be much less important than those IDC events identified in our data product, and the missing of the IDC events with PF lifetime shorter than 1 hour should have little impact on our results.

Finally, the number of results is excessive and, in my opinion, deviates the attention about the main objective of the research: the application of the new methodology. On the contrary, they do not compare their results with other methodologies, which are easy to find and can verify the lines provided by the current manuscript. In the next lines, the authors will find more detailed comments regarding some other points.

Reply:

Thank you for your suggestions. The manuscript's primary purpose is to introduce a new data product useful to the Earth science community, which is the aim of the ESSD journal. The methodology to produce the data product is just part of the data product introduction. The uncertainties, validation/assessment, and potential usage and applications of the data product are also parts of the data product introduction. We indeed show some results in Section 3. However, those results are straightforward and can be easily obtained from the data product. The primary purpose of Section 3 is to validate the quality of the data product and confirm that the data product is consistent with our definitions and general knowledge. Section 3.2, about precipitation characteristics from different sources, is also an example of the potential applications of the data product. Therefore, we'd like to keep these results except that we have revised some discussions about convection intensity in Lines 374 – 383 and 387 – 393 to make the sentences more understandable. We agree with you that we should add some comparisons between our results with other studies, further validating the data product. We have compared our results with other studies in Lines 346 – 349, 366 – 368, 385 – 386, 435 – 437, 442 – 445, 498 – 500, 509 – 517, 558 – 560, and 569 – 572 in the revised main manuscript.

Abstract

Acronyms (MCS, IDC, FLEXTRKR), references (Li et al., 2020) and web pages (http://dx.doi.org/10.25584/1632005) are not frequent and preferable not included in abstracts. Do you consider strictly necessary for the understanding of the text to maintain them? In my opinion, at least the last sentence should be removed

Reply:

Thank you for your suggestions. We have deleted "FLEXTRKR" in Line 17 in the revised main manuscript since the acronym was only used once there, but we'd like to keep "MCS" and "IDC" in the abstract as they were used several times. Using the acronyms can simplify the abstract. For the last sentence, the doi link and reference is a requirement of the ESSD journal.

Introduction

Although Doswell et al (1996) is still one of the reference paper in this field, there are *many more recent research manuscripts that are noticeable to include in the L46-61 paragraph, for instance:*

- *Brooks, H. E., Doswell III, C. A., & Kay, M. P. (2003). Climatological estimates of local daily tornado probability for the United States. Weather and Forecasting, 18(4), 626-640.*
- *Taszarek, M., Allen, J. T., Púčik, T., Hoogewind, K. A., & Brooks, H. E. (2020). Severe Convective Storms across Europe and the United States. Part II: ERA5 Environments Associated with Lightning, Large Hail, Severe Wind, and Tornadoes. Journal of Climate, 33(23), 10263-10286.*

Reply:

Thank you for your suggestions. Brooks et al. (2003) investigated the climatological distributions of tornado occurrence over the contiguous United States. Taszarek et al. (2020) investigated meteorological environments associated with lightning, large hail, severe wind, and tornadoes. Both studies used severe weather hazards as proxies of thunderstorms, confirming the relationship between the weather hazards and deep convection. We have cited both papers in Lines 50 – 51 in the revised main manuscript. Besides, we have also cited another study from Koehler (2020) in Line 50 in the revised main manuscript, which examined the climatological distributions of lightning flashes and thunderstorm days over the contiguous United States.

L62-70: "deep convection" is repeated five times in the same paragraph. Please, modify the text using other options

Reply:

Thanks. We have deleted "deep convection" in Lines 64 and 70 and made some changes to the paragraph. Now those sentences are as follows.

'The crucial roles of deep convection motivate the need for more accurate and comprehensive datasets to improve understanding and modeling of this process and its impacts. To this end, datasets with information on the location and time of occurrence, intensity, and other properties of deep convection are necessary to understand and quantify its impacts on the hydrologic cycle, severe weather hazards, large-scale circulations, etc. While field campaign data can provide detailed information on deep convection properties, they are limited in space-time coverage for statistical analysis. A corresponding reliable long-term dataset is undoubtedly useful for model evaluation and development (Prein et al., 2017; Yang et al., 2017).'

L75-76: when you introduce IDC, are you including supercells? If the answer is yes, can you confirm that all the sentences that following this are true? In special, I disagree with the points about the higher rain rates, larger echo top heights, and greater ice masses.

Reply:

Thank you for your comments. The definitions of IDC and MCS in Rowe et al. (2011) and Rowe et al. (2012) are similar to our study, based on precipitation feature (different from PF in our study) major axis length and aspect ratio between the major axis and the minor axis. In their studies, IDC is defined as a track with precipitation feature major axis length < 100 km and aspect ratio less than 5:1; while an MCS is defined as a track with precipitation feature major axis length > 100 km. In short, their definition depends on the size and shape of convective areas but not rotation, which cannot be used to identify supercells, as we mentioned above. Since the results of Rowe et al. (2011) and Rowe et al. (2012) were only based on the North American Monsoon Experiment (NAME) in the summer of 2004, to be more accurate, we change 'can' to 'may' in Line 77 in the revised main manuscript so that the results are not representative of all cases.

Again, lines 80 and 82 depend on if you consider supercells or not in the IDC database

Reply:

Thank you for your comments. The conclusion that MCSs might be associated with more favorable environmental conditions is based on the results of Rowe et al. (2012). Their definitions of IDC and MCS have been described above briefly. French and Parker (2008) compared isolated supercells and MCSs, but their MCSs also showed supercell features. It is consistent with our previous point: supercells can be isolated or embedded in MCSs. We cannot separate supercell in our data product based on the algorithms and source datasets used in this study. To be more accurate, we have changed the sentence as follows (Lines 81 – 87 in the revised main manuscript).

'Rowe et al. (2012) also suggested that the enhanced rainfall from MCSs might be associated with more favorable environmental conditions, such as higher convective available potential energy (CAPE) and wind shear. CAPE and wind shear can impose different impacts on the initiation and evolution of IDC and MCSs (French and Parker, 2008).'

L103-104: "We produce the data product"?

Reply:

We have combined the two sentences into one as follows (Lines 104 – 113 in the revised main manuscript).

'The data product is developed by applying an updated Flexible Object Tracker (FLEXTRKR) algorithm (Feng et al., 2018; Feng et al., 2019) and the Storm Labeling in Three Dimensions (SL3D) algorithm (Starzec et al., 2017) to the NCEP (National Centers for Environmental Prediction) / CPP (the Climate Prediction Center) L3 4 km Global Merged IR V1 brightness temperature (T_b) dataset (Janowiak et al., 2017), the 3-D Gridded NEXRAD Radar (Gridrad) dataset (Homeyer and Bowman, 2017), the NCEP Stage IV precipitation dataset (Lin and Mitchell, 2005), and melting level heights from ERA5 (ECMWF, 2018).'

Source datasets and algorithms

L120-121: "We only use the hourly Tb data in the FLEXTRKR algorithm discussed below, as all other datasets are only available at an hourly interval" Do you think that this time resolution could have any influence in the results?

Reply:

We discussed the limitations of the temporal resolution in Section 4.3 (Lines 658 – 683). We missed some short-duration (< 1 hour) convective events due to the 1-hour resolution of the data product. If all source datasets were at a resolution of 30 minutes, we would identify some IDC events shorter than 1 hour. Then, the data product would be somewhat different from the current one. Suppose we only used the half-hour T_b data but still used the hourly precipitation and reflectivity datasets in the FLEXTRKR algorithm. In that case, the results might also be different, but the difference should be much smaller than the former case with all half-hour source datasets. The reason is that T_b is only used to identify CCS in FLEXTRKR, and the confirmation of MCS and IDC still needs precipitation and reflectivity data.

Figure 1: Maybe you should include a small map of the whole American continent and a box for the zoomed area shown in the current caption

Reply:

We have added another subplot in Figure 1 showing the whole North American continent and a red box indicating the data product domain. Please see Lines 218 – 226 in the revised main manuscript.

L202 (and many more): you cite "table S1" in the text, but I was not able finding this table in your manuscript.

Reply:

Table Sx or Figure Sx means figures or tables in the supplement. There is a supplement attached to the main manuscript.

Para 283-288: according to these lines, maybe you should change the label of "IDC" category.

Reply:

Thank you for your suggestions. At the very beginning, we tried to use "non-MCS deep convection," which is the exact meaning of these convective events. But considering that we plan to add hurricanes on the basis of the data product in other studies, we need to assign a specific name to them. We also thought about the term "quasi-isolated deep convection," which Bigelbach et al. (2014) used to separate stronger air mass thunderstorms, multicell clusters, and supercells from weak convection and MCSs. However, "quasi" means "apparently but not really," which can not represent those isolated thunderstorms in our data product. Finally, we decided to use "isolated deep convection" following the idea of Rowe et al. (2011) and Rowe et al. (2012), which used isolated convection to distinguish smaller convection events from MCSs, similar to our data product. Since we mentioned the limitation of the term "IDC" in the manuscript, we'd like to keep it after careful considerations.

In figure 2(b), it seems that there are more categories that the maximum number of the legend. Is this it?

Reply:

Yes. There are too many small clouds, and it is hard to assign a distinct color to each cloud. It is better to assign a constant color to those smaller clouds to show those larger clouds clearly.

Results

This section results too much extend and hard to follow (because its density and the large number of interesting results). However, I miss the comparison of your results with other works such:

Fritsch, J. M., R. J. Kane, and C. R. Chelius, 1986: The Contribution of Mesoscale Convective Weather Systems to the Warm-Season Precipitation in the United States. J. Climate Appl. Meteor., 25, 1333–1345, https://doi.org/10.1175/1520-0450(1986)025<1333:TCOMCW>2.0.CO;2.

Or the cited:

Haberlie, A. M., and W. S. Ashley, 2019: A Radar-Based Climatology of Mesoscale Convective Systems in the United States. J. Climate, 32, 1591–1606, https://doi.org/10.1175/JCLI-D-18-0559.1.

Geerts, B. (1998). Mesoscale convective systems in the southeast United States during 1994–95: A survey. Weather and Forecasting, 13(3), 860-869.

Then, my suggestion is reducing the results to the most interesting one (for instance, the percentage of contributing rainfall for each type) and comparing with the others works. This is also because the goal of the paper is to present the methodology, but not the *"climatology". Then, the authors could have the opportunity of publishing the climatological results in another manuscript.*

Reply:

Thank you for your suggestions. We have compared our results with other studies in Lines 346 – 349, 366 – 368, 385 – 386, 435 – 437, 442 – 445, 498 – 500, 509 – 517, 558 – 560, and 569 – 572 in the revised main manuscript.

As mentioned above, the primary purpose of the manuscript is to introduce a new data product. Those results in Section 3 are used to validate the quality of the data product and confirm that the data product is consistent with our definitions and general knowledge. Section 3.2, about precipitation characteristics from different sources, is also an example of the potential applications of the data product. Therefore, we'd like to keep these results except that we have revised some discussions about convection intensity in Lines 374 – 383 and 387 – 393 to make the sentences more understandable.

Uncertainties of the data product

About lines 540-549: there are many more radar errors that can affect NEXRAD or other network, e.g. beam blockage, false echoes related with EM interferences, solar interferences, volumetric conus influence, among others. Have they considered or they can appear in the volumes?

Reply:

When we talked about potential low-quality observations, we meant a summary of those small errors well-mitigated by the quality control approach from Homeyer and Bowman (2017) (http://gridrad.org/software.html), including false echoes related to electromagnetic interferences and solar interferences.

We think you meant the cone of silence when you mentioned ‘volumetric conus influence.’ The cone of silence and beam blockage is related to the missing radar data discussed in section 4.2 (Lines 647 – 657 in the revised main manuscript), as no data are available under the impact of the cone of silence and beam blockage. According to the explanation of Homeyer and Bowman (2017), many errors in the native NEXRAD level-2 observations that propagate into the Gridrad 3.1 data can be largely reduced through the quality control approach, but not completely. Therefore, there are still uncertainties in the radar reflectivity data we used for SL3D and FLEXTRKR. That is why we think that using three different datasets in the FLEXTRKR and SL3D algorithms could improve our identification of MCS and IDC than using a single dataset.

L566-567: “we identify the most robust MCS/IDC events satisfying all the criteria based on the three datasets” Which percentage of data satisfies the whole set of criteria?

Reply:

Annually (2004 – 2017), we identified 802,633 tracks based on CCS (brightness temperature T_b), and finally, we obtained 45,800 MCS and IDC tracks. Therefore, 5.7% of the CCS tracks satisfy all the criteria.

About Stage IV: do you think that geo-statistics contribute to the error, at the time of *generating the final product?*

Reply:

We think you meant the 'erroneous precipitation' hours in Lines 618 – 624 in the revised main manuscript. We are not sure where the error is from. We contacted the NCEP Stage IV team for help, but they did not know the root cause of the issue either. We suspect that maybe NEXRAD Level-II data (reflectivity, radial velocity, etc.) were not well-filtered and processed in Stage I (the first step of Stage IV). Then the errors were transferred to the following steps to generate Stage IV. Geo-statistics may cause the spread of the errors to larger areas.

L584-586: The sentence "Most grid cells in the US have less than 2% missing hours, which should have a negligible impact on the data product." Is, at least, debatable. According to figure 5, it is difficult finding pixels with more than 170 hours of rainfall per year (combining both maps). This is less than 2% of the yearly hours (8760). If most of those missing hours coincide with a part of the rainfall period, the results changing notably. Please, explain better this point.

Reply:

Thank you for your comments. However, Figure 7 (the old Figure 5) cannot be compared with Figure S10 (the old Figure S12) directly, as Figure 7 and Figure S10 use different criteria. Firstly, according to the caption, Figure 7 shows the number of hours per year when any MCS/IDC events produce > 1 mm hourly accumulated rainfall in a grid cell, while Figure S10 is about missing values (not related to precipitation amount thresholds). If we exclude the limitation of > 1 mm hourly accumulated rainfall and count the number of hours when any MCS/IDC clouds overpass the grid cell, we can obtain Figure R1.

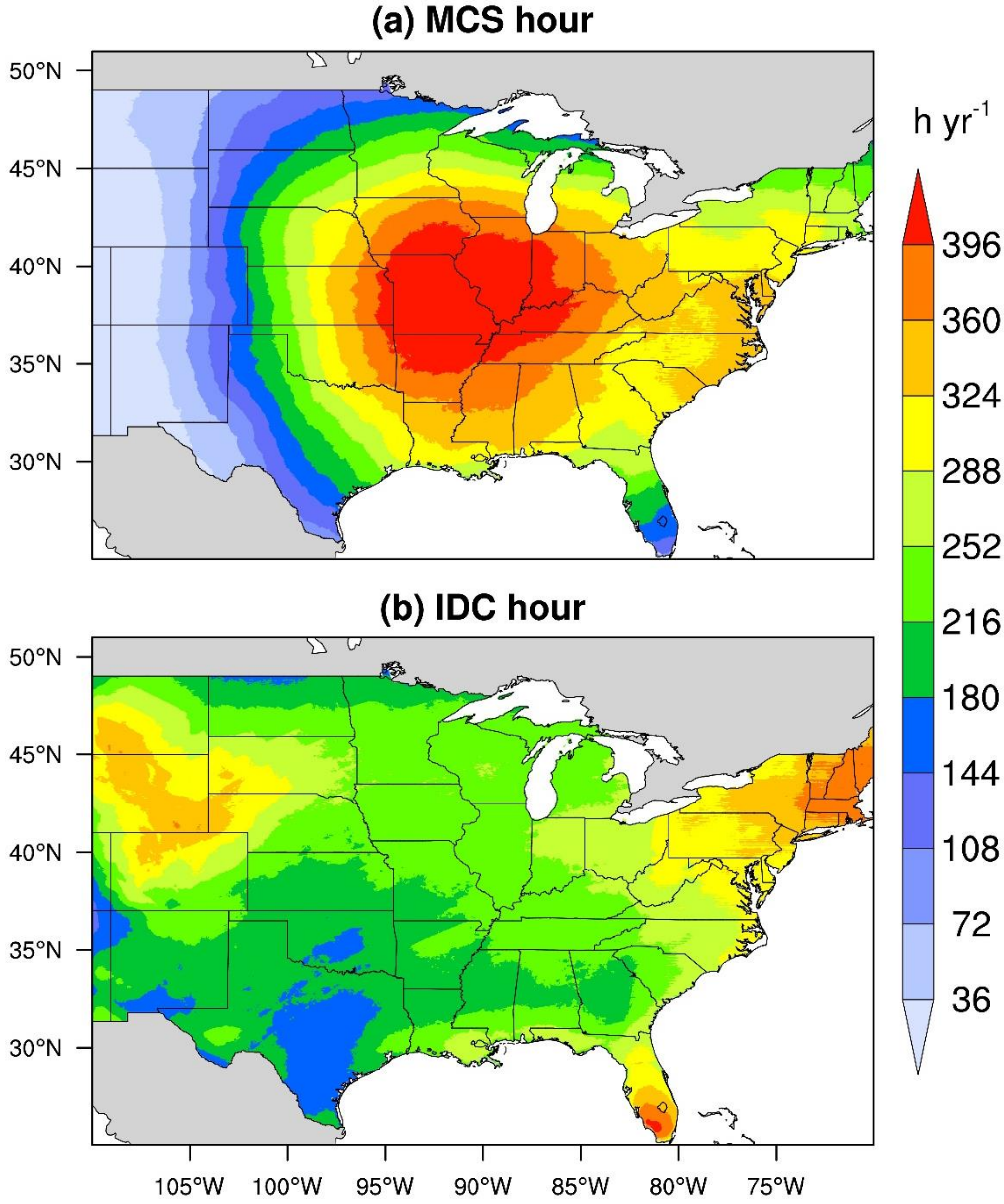

Figure R1. Spatial distributions of annual mean MCS/IDC hours for 2004 – 2017. (a) is for MCS, and (b) is for IDC. The annual mean MCS/IDC hour of a grid cell is the number of hours per year when any MCS/IDC clouds overpass the grid cell.

Secondly, Figure 7 shows the results satisfying all the criteria based on three types of observations, not just precipitation. That is to say, we excluded those 716 hours not passing the T_b criteria in Lines 632 – 635 in the revised main manuscript in Figure 7. However, when we plotted Figure S10, we included those 716 hours in the denominator to exclude the impact of missing T_b on the missing precipitation calculation. If we exclude those 716 hours and only count the number of hours with missing precipitation in the rest hours with T_b satisfying the criteria in Lines 632 – 635, we can obtain Figure R2, which can be compared to Figure R1. Now we find that most grids only have about 40 – 60 missing precipitation hours per year, while MCS/IDC hours in Figure R1 are mostly above 400 per year, much higher than the values in Figure R2. Anyway, we agree with you that the 40-60 missing precipitation hours can still affect the MCS/IDC results if all these hours coincide with a part of the rainfall period. However, that is an extreme case and unlikely to happen. We find that missing precipitation generally occurs continuously in space. For example, from 2004-03-03T07:00:00Z to 2004-03-03T12:00:00Z, all precipitation data are missing over the data product domain. Therefore, they only affect our identification of MCS and IDC during a short period and should have little impact on our final climatological results from the perspective of time length.

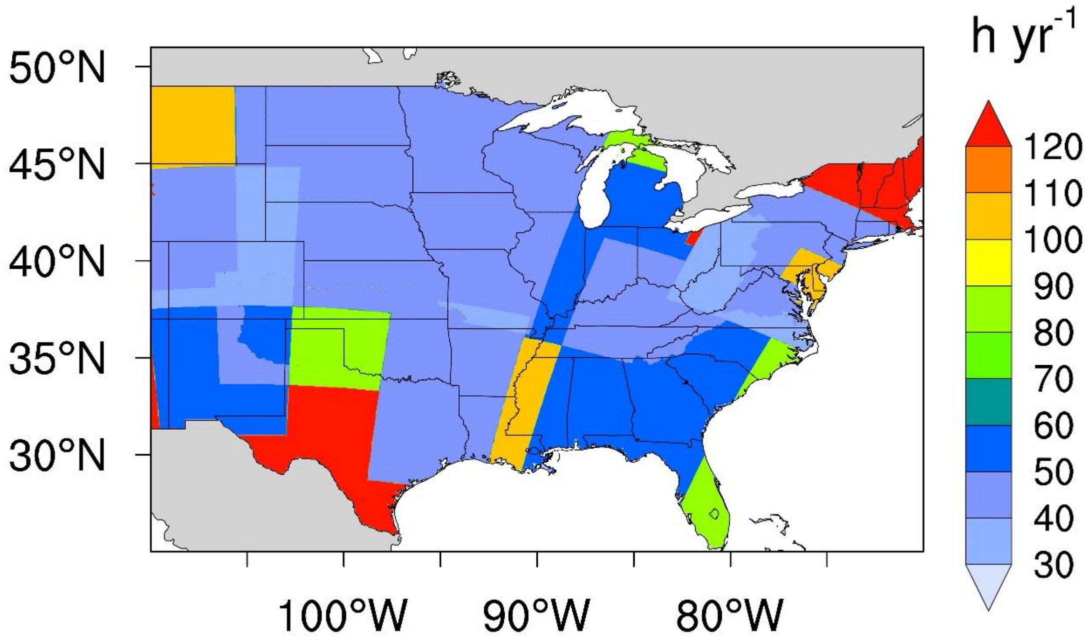

Figure R2. The distribution of the number of hours with missing precipitation per year between 2004 and 2017. Those 716 hours not satisfying the T_b criteria in Lines 572 – 576 are not included in the calculation.

L647: What is for you "most of the important MCS"?

Reply:

Here, we meant MCSs with the most precipitation, which can be derived from the sentence in Lines 706 – 707 in the revised main manuscript: 'The fraction of MCS precipitation only increases by 6% (from 45% to 51%), compared to the almost doubling of MCS number (from 454 to 857).' It means that the increased MCSs contribute little to the total MCS precipitation. To be more accurate, we have added 'with heavy precipitation' in Lines 708 – 709 in the revised main manuscript. The sentence now is as follows.

'The fraction of MCS precipitation only increases by 6% (from 45% to 51%), compared to the almost doubling of MCS number (from 454 to 857), suggesting the MCS definition in the original data product is capable of capturing most of the important MCSs with heavy precipitation.’

References:

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