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Discussions

Interactive comment on "Day-time identification of summer hailstorm cells from MSG data" *by* A. Merino et al.

Anonymous Referee #1

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This paper offers an interesting methodology for objective identification of hail producing storms over central Spain. It is very unclear to me if this methodology would be applicable to any other area outside the Middle Ebro Valley as it has only been trained with observations over this region. My primary issue with the document is the authors understanding and treatment of the satellite data in the convective cloud mask (CM) and hail mask (HM). The authors seems to take a lot of satellite observations, combine the channel information together in strange ways, and then arrive at a set of parameters that offer the highest weight in HM and CM. For example, the authors have derived a 8.7 & 1.6 micron combination that has not really been published in the literature. They make misleading statements on the relevance of the 8.7 micron channel.

In addition, they somehow use cloud property retrieval information throughout the pa-

per and do not introduce this dataset nor do they explain how the cloud property retrievals are "extracted" for this domain. They consistently try to relate their SEVIRI channel or channel combination data to the cloud retrievals to better understand the SEVIRI channel data. If one is trying to find clouds that are cold, highly reflective, and contain small ice particles, why not just use the retrieval information in the algorithms instead of the SEVIRI channel data. With this approach, you're sure to isolate the desired pixel types instead of "guessing" with the SEVIRI channel data. The authors also use LWP in their analysis as if it were an independently retrieved parameter, when in fact, LWP is a derived quantity that is a function of OT and Re thus it does not contain any unique information.

Lastly, the datasets used by the authors are not very well described and the Figures could use improvement. They mention use of training data that seems to dwindle in sample size as the paper progresses, finishing with a very small validation sample size (78 events). They do not show a figure with the HDT output and the locations of hail fall. For many readers, this visual representation of algorithm output is most useful as this is what a forecaster would be seeing on their terminal. I'd expect to see this for more than one event, not just the 12 August 2011, 1400 UTC case. This shouldn't be too hard to find for the authors given their strong validation statistics.

The bottom line is that, after reading this paper, I was left with more questions than answers which should not be the case with a publication-ready paper. Thus I feel that it needs major revisions. My specific comments are included below:

Line 2, page 5455, how is this statement relevant to the paper? You do not have any satellite simulation work in this paper.

Line 17, I think you should be precise here and say that SEVIRI is an advance over the Meteosat First Generation imager, since Meteosat may have more sensors than just the imager.

Line 12, page 5456, one can compute updraft speeds only during the developing phase with cloud top cooling, not mature phase. Once the storm becomes mature, there are very small temporal oscillations in IR temp, but this definitely does not mean that the storm has a weak updraft.

Line 1, 5457, I would insert the word “can” prior to “have long-lived cold rings”. Your current statements suggests that all storms have rings.

Line 3-5, 5457, you state that none have provided an unsupervised hail detection algorithm. While this may be true, several of the studies you cite in the beginning of this paper (and one that you’ve neglected, i.e. Dworak et al. 2012 (Wea. Forecasting)) have shown strong relationships between their products and hail falls. The way you’ve phrased your argument would suggest that your method is the only possible way to objectively recognize hailstorms which isn’t very fair to the hard work of the authors above.

Line 26, 5462, I suggest you insert 15 min ahead of temporal resolution for this and all other occurrences

Table 2, some of the parameters you’re using in combination with each other do not make sense. What exactly does “interaction” mean when you’re examining the combination of two variables? When I see (Channel 8.7μmÂůChannel 1.6 μm) does this mean that the two parameters are multiplied by each other? Subtracted? Regardless, for the 8.7 um channel discussion is invalid. You say that the 8.7 channel permits distinction between different cloud top phase. This is true, but only when used in combination with the 10.8 micron channel (i..e 8-7 – 10.8 BTD). With this BTD, one is taking advantage of differences in the refractive index of ice in the two channels. The 8.7 alone does not offer the information you state, nor has some sort of “interaction” between 1.6 and 8.5 been published as far as I know.

Line 4, 5464, you never state how you acquired the VISST product, what its characteristics (spatial/temporal resolution) are and how you are “extracting” these properties? I see some acknowledgement at the end of the paper but I think this should be more

prominent in the paper body since this VISST data is used quite often in the paper. You also discuss the LWP, OT, and Re parameters if they were all independently retrieved datasets, when in fact, LWP is a function of OT and Re so LWP would offer no unique information here. You should consult with the VISST algorithm developers to better understand their products.

Line 10, 5467, you should explicitly state that you’re using the reflected component of the 3.9 micron channel and indicate the methodology used to compute this reflected component.

Line 13, 5468, you should be more precise when stating that radiances from two satellite channels with broads weighting functions informs one on the “water vapor concentration”. The term concentration implies a quantitative retrieval which cannot be done with the satellite information here.

Line 25, 5468, you say that cloud thickness is related to Optical Thickness (OT), are you referring to the geometric thickness, i.e. height difference between cloud top and base? If so, your statement is not necessarily true in that one can have very high OT for low stratus clouds given that they are very reflective and spatially uniform, but have fairly low geometric thickness.

Line 21, 5472, you say that high 0.8 um albedo is required for hail storms. Could you provide a physical explanation as to why this parameter has more value than the 0.6 um channel?

Line 25, 5473, a storm need not have a overshooting cloud or V-shape to produce hail. This should be clarified. In fact hail tends to fall upon collapse of an overshooting region.

General question, you devote a lot of effort to relating various SEVIRI channel information and some strange channel combinations to retrieved cloud microphysical parameters from NASA LaRC VISST. The NWC SAF produces cloud property retrievals in real time, why not just use their retrieval fields (or the LaRC VISST) for your work instead of the individual channel information and combinations?

Another general question, page 5474, how could one ever have high optical thickness and strong updrafts (inferred from NIR) without a lot of upper tropospheric water vapor? It would seem that your analysis of microphysical cloud properties items 1-3 are not necessarily independent of one another.

Section 6.1, it seems that the sample size of events becomes smaller and smaller as the paper progresses, you start with 700 events for the CM, then 200 events for the hail training, then 78 events for the verification. Why such a small verification database?

Line 5, 5477, it is unclear how the HM is transitioned into a probabilistic product in the HDT, this should be clarified.

Figure 6, the locations of hail fall at this time should be included on the figure.

General question, how do you account for variations in visible and NIR reflectance as a function of solar zenith angle in your HDT product?

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 5453, 2013.