

Interactive comment on “Hailstones across the Greater Sydney Metropolitan Area” by A. A. Rasuly et al.

Anonymous Referee #1

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The paper “Hailstones across the Greater Sydney Metropolitan Area” by Rasuly, Cheung and McBurney, presents a 25-year hailstone climatology over Southern Australia. The Authors aim to assess some spatial and temporal characteristics of severe hailstorms and to sketch a possible long term trend.

The topic is interesting and fits the scope of NHESD, but the paper is rather poorly written, with several redundancies and irrelevant comments, and need a substantial revision before can be considered for publication. I try to summarize my major concerns about the paper in the following, where page number refers to the “printer friendly” document.

First, the title refers to the Greater Sydney Metropolitan Area, but in the paper, the data

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are from the Greater Metropolitan Severe Thunderstorm Warning Area, that includes Sydney area, but also other regions with different characteristics, as it is demonstrated in the paper.

Secondly, there is unnecessary emphasis on the use of GIS. It seems all the simple operators and queries used to derive the results do not really need the full use of a GIS, and the same results can be obtained with simple programming. Moreover, is not interesting here how the analysis is performed, since no new operators or algorithms are introduced. For instance, to the goal of the paper, the long sentence at the beginning of section 3.2 is redundant and should be summarized as: “25 years of data are analyzed”. Similarly, at the end of the same section, the last paragraph repeats obvious procedures for data analysis, and should be canceled. The GIS is not a technique, but an environment that offers tools to elaborate the data.

A fundamental information missing in this manuscript is the total number and spatial distribution of the hailpads used, which is essential to understand the relevance of the study. In figure 1 or 2 (I suggest to merge them) a map showing the location of the ground stations is mandatory. In addition: which quality control is performed on the data? which are the “analytic stages” mentioned on line 8 (page 6979)?

Some concepts are repeated many times across the manuscript. Few examples: the time span of the dataset is mentioned on sections 1, 3.2, 4.1 (5 times), 4.2 (2 times) and 5; the number of hailstones events is reported on sections 4,1 (2 times) and 6; the use of GIS on sections 1, 3.1, 3.2 (x3), 4.1, 5 and 6 (x2). The Authors should carefully review the manuscript trying to cancel all redundancies and repetitions.

Throughout the paper, there is some confusion around terms as “hail”, “hailstone”, “hail event”, “hail day” and “hail storm”. Please clearly define these terms at the beginning and stick to the definitions in the paper.

At lines 13-15 on page 6980 it is said that “the polinomial fitting” makes recognize the positive and negative hail years: as a matter of fact, this is recognized by simply

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looking at the numbers, the fitting does not add anything. What is the meaning of the polynomial curves reported in figures 10 and 14 to connect data points? Usually, experimental data are fitted by a simple relation to search of a dependency (trend) between dependent variable (in these cases hail frequency, size and NMRC) and independent variable (time, in this case). The plotted curves have no meaning, I suggest to plot only the data points. The same applies also for the dotted curve in figure 11b: it is not a trend line (why a 6th-degree polynomial is used? With coefficients ranging from 10^{-4} to 10^{15} . . .), the Authors should clarify which is the meaning of this curve.

In describing equation 1 the values of the defined parameters should be reported, with the study performed to set them.

The discussion of the diurnal cycle of hail events is redundant: figures 3 and table 3 tell the same thing, but with different time intervals. It is easy to understand the numbers in table 3 just looking at figure 3. The information about the mean hailstone size can be added to figure 3. The same applies to the discussion of seasonal variability: figures 6 and 8 and table 4 are redundant (all information can be shown in just one figure or table), such as the discussion in section 4.1 and 4.2, that is repeated for months and seasons.

The need of separate section 5 (Discussion) is not clear. In the first part, some of the results, already redundantly commented in previous sections, are repeated (lines 11-26). Then, obvious arguments are proposed, without any quantitative reference to the data presented. A sentence like "the effect of "built-up" areas upon the distribution of hailstones is quite real" cannot be reported in a scientific paper. It is real or not? Since the Authors use GIS, it would be easy to compute the frequency of hail occurrence (and size) over built-up areas. The same applies also to the impact of the coastal areas and orography that should be quantitatively assessed. The last part of the section reports the result of previous studies on the large scale settings favorable to development of hailstorms, that are out of the analysis reported in the paper. I suggest to cancel this section.

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In the "concluding" section the second "final outcome" mentions "appropriate temporal models": what temporal models are used here? In the last item: how the Authors infer "spatial movements of hailstones" (or hailstorms? or hail events?) with the used dataset?

Minor comments

In section 3.1 the sentence on lines 16-20 is unclear, please rewrite.

Table 1: how are these events selected? Please replace "supper" with "super"

Lines 11-13 on page 6977: this sentence repeats the same thing.

I find figures 4, 5, 7, 9, 11a, 12 and 13 difficult to read: the symbols used are not easy to distinguish and the character are too small. I suggest to use different colors and larger fonts.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 6973, 2014.

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