

Interactive comment on “The scale of typhoon RUSA” by N. W. Kim et al.

Anonymous Referee #1

Received and published: 16 October 2006

Review in connection to the paper ‘The scale of Typhoon RUSA by Kim, Won and Chun

General: The paper describes the hydrological impact of typhoon RUSA on the Korean mainland. Due to the fact that this storm certainly was a major event a publication is certainly justified in principle. However, none of the employed methods is sufficiently explained (so that the reader could appropriately judge the results). Also, the geographical names must be located in some overview map. Finally, much of the used concept should briefly be introduced and the data material must be substantiated (see major comments). Finally, some of the crucial points should be worked out more clearly so that the ‘message’ of the authors comes more clearly across. For this, in the ‘detailed comments’ a number of questions are formulated. My overall disposition is therefore ‘major revisions’ before the paper can be published.

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Major Comments:

1. Many of the geographical names, regions and rivers are most likely unknown to the average HESS reader. For example, on p. 3151 Taebaek mountains (l. 20), Jin Island (l. 23), Samcheock (l. 25), Jeonam province (l. 27), then on p. 3152 Chungnam, Chungbuk, and Gyeonggi province(s) (l. 4), Sumjin River (l. 6) etc. etc. A map, like Fig. 2, but including all the relevant names is absolutely necessary to understand all the descriptions on how the event evolved.

2. The underlying data set is to some extent described section 4.2. However, only the number of stations is given and their sources as well as how many have been excluded due to some 'general reasons'. What is necessary to judge the analysis is in addition (and in an early section, i.e. before the Figures with the isolines appear) is the following: how many stations (to judge the quality of the isolines), what is the observation frequency and type (automatic stations or hand readings etc.), possibly manufacturer and observation principle, duration of their records (are these stations in operation over the last 100 years, or since 10 years, say?)

3. The major methods employed in this study should briefly be explained: How are return periods (p. 3153, l. 20) determined (method, possible uncertainty, statistical reference); how is probable rainfall defined. Also the DAD analysis and the PMP analysis seem to deserve a short introduction, i.e. on what principles the analysis is performed, what the required input is and again a word on possible uncertainties.

Detailed Comments:

Page 3148:

l. 10/11: no abbreviations in the abstract (in any case, for the first usage of 'DAD' or 'PMP', also in the body of the text, at least the words should be spelled out.

Page 3149:

l. 5: 'was up to 879.5 mm': per 24 hours (I presume). Also the other precipitation

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amount (in this and the following paragraphs), usually need an indication mm/3h or mm/24h etc.

Page 3150:

I. 6: 'The upper air, above the North Pacific, which lay from east to west in (the) Korean Peninsula': how can the 'air lay from east to west'? I don't understand at all.

I. 8: how can the 'movement of velocity' be blocked? Aren't the air masses moved, or the core of the typhoon or whatever (but the velocity?).

I. 20: are there any references (in the open literature) for this description of the typhoon?

Page 3153

I. 6: 'that there are large rainfall deviations EVEN in the ': First of all: you probably mean spatial variability (not deviations). Second, why 'even'? What is the reason for expecting no large spatial variability in a region with a lot of rain? (third, where is Yeoungdong anyway? Couldn't find on Fig 2).

I. 14: after this paragraph I wonder whether the authors have any comment on the 'twin peak' structure, especially in Fig. 7.

I. 17: Among the 8 observation stations of Fig. 5: I see only 4 (in Fig. 5c). Figs. 5a,b have only one.

Page 5154:

I. 3: AWS needs to be spelled out (automatic weather station, I presume).

I. 5: what is 'too large'? Can the authors give a threshold? What is a 'different distribution'? How judged? Any objective criteria?

I. 6: 'Such examples can be seen': Does this mean that the authors chose 'excluded stations' in Figs. 5-8? IF this is the case this certainly should be avoided.

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Page 3155:

I.1: 'represents comparisons': for those not entirely familiar with this type of analysis, it is absolutely unclear what is displayed here and what it should mean. Can the authors explain?

I. 11: 'order of magnitude' should be replaced by 'ranking'

I. 20: 'using [an]established procedure': does this mean that the given values are NOT according to an established procedure of WMO? (I suspect that the procedure as described in MOCT 2000b indeed differs from the WMO procedure. If this should be the case, this should clearly be stated (before discussing the results) and a reasoning should be provided for having chosen this non-standard approach in the first place.

I. 21/24: replace mb by hPa.

Page 3156:

I. 26: estimated PMP exceeds the PMP diagram within \tilde{E} . I don't think the notation 'PMP diagram' has been introduced so far. So, it shouldn't first appear in the Conclusions.

Table3: Given are periods (years) and not frequencies (would be years to the minus one power).

Table 5: Order of magnitude should probably read 'rank' (an order of magnitude is whether a value is rather 1 or 10 or 100 \tilde{E} .)

Fig 1: What is the moisture vector? Should be defined in the text.

Fig. 5: Chupungnyeong reads Chupungnyeung in Fig 2. Which is correct?

Figs. 5-8: The ordinate must be labeled in appropriate units (mm/h).

Figs. 5c-8c: The symbols (especially in Fig 5c) cannot be distinguished so that it is not possible to see which line corresponds to which location.

Figs 8/9: No labels given in the inlets. Also no labels on the abscissa an ordinate (so:

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what is plotted here? what are the units?). Possibly a problem of my acrobat reader.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3147, 2006.

HESSD

3, S1196–S1200, 2006

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