

Interactive comment on “Identifying homogenous sub-periods in HadISD” by R. J. H. Dunn et al.

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Received and published: 28 April 2014

In general, this paper documents valuable work and I do not see anything to give me any serious grounds for concern over the substance of the analysis. My two major comments relate to making what has been done clearer to the reader, and to place the work in context with some further discussion of potential causes for some of the inhomogeneities.

Major comments

1. The widespread use of the term ‘adjustment’ to describe the magnitude of an inhomogeneity creates a certain amount of confusion, as no adjustment is actually carried out in this paper (as described at the end of p4). While I appreciate that the authors have defined this at p5 line 6, I still think it would be helpful to the reader if an alternative term could be found (perhaps ‘magnitude’ or ‘size’ of the inhomogeneity).

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2. There is no real discussion of possible causes of inhomogeneities, how these might differ between variables, and in particular how the method of construction of this data set introduces possibilities for inhomogeneity types which might not exist in some other data sets – for example, at p8 line 11-15, an obvious potential cause of inhomogeneity in both mean and diurnal range of temperature is a change in reporting schedule of the station, either explicit, or implicit through the use of daylight savings time (see also comments below on diurnal range). Wind, dewpoint and pressure all have their own homogeneity issues, too, and some citations of relevant papers would be useful (in the case of wind, some of the particular difficulties are discussed in Jakob 2010, Aust. Met. Oceanogr. J. 60. 227-236 - http://www.bom.gov.au/amoj/docs/2010/jakob2_hres.pdf). It might also be worth a brief discussion of data quality issues that might have implications for homogeneity if they occur too frequently – two common ones (from my experience) are dewpoints tracking dry-bulb temperatures as a result of dried out wet-bulbs, and double conversions from SLP to MSLP leading to spuriously high values of the latter.

Minor comments

P7 line 18-20 – note that Trewin (2013) actually used annual and seasonal, not monthly, mean values of maximum and minimum temperatures in the detection of inhomogeneities in the ACORN-SAT data set. (I'm assuming the point which is being made here is that maximum and minimum are being used, as opposed to mean or diurnal temperature range).

P8 line 15 – if I've understood this correctly, in this context the diurnal range is the difference between the highest and lowest hourly observation of the day, and the monthly average is taken as the average of the daily diurnal ranges? This definition, if correct, would be different to the more commonly used definition of diurnal temperature range as the difference between the daily maximum and minimum temperature – this needs to be clarified in the text.

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P9 line 1 – what would result in a station being unable to be processed by PHA? (presumably an excessive amount of missing data – anything else?).

P10 line 12-13 – the station numbering system is not described anywhere in the paper. It would be useful to list where these stations are (perhaps in a footnote). Also affects line 6 page 12.

P10 line 15-17 – is there any indication of why medium-length stations might have fewer change points than shorter ones? (does PHA have any known issues with over- or under-detection of change points near the start/end of a time series, for example?).

P10 line 29 – ‘wing’ – I’m more used to seeing this described as ‘tail’, but if ‘wing’ is also a standard term I can live with it. Also appears elsewhere.

P11 line 10 – presumably the same data completeness criteria apply for these variables as for temperature – if so it would be useful to say so.

P12 lines 24-25 – ‘The stations which were not processed by PHA cluster’ – does this apply for all three variables considered in Fig. 11, or just for some of them?

Interactive comment on Clim. Past Discuss., 10, 1567, 2014.

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