

Interactive comment on “Severe summer heat waves over Georgia: trends, patterns and driving forces” by I. Keggenhoff et al.

Anonymous Referee #2

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This article shows a climatological analysis of extreme temperature events in Georgia. It has the merit of analysing a poorly studied region; in this sense I encourage its publication of ESD. Nevertheless, I have a few fundamental problems with this paper.

In the first place the article is not well written. The amount of material presented is huge and a real effort of synthesis is in order, along with an editing of written English. The methods and the variables used are not well described. Sometimes you have to look for the information. For example, the domain of averaging of the predictands is only found in a figure caption, and actually one wonders if it's spatial means or vector fields until reading the caption. But there are many examples like this.

Second, the article lacks focus. Is the paper a study on the definition of the heat-waves, on the trend, or on the dynamical/physical forcings? From time to time there

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are unsupported sentences, examples: pg 2291 line 5: "implying strongest heat-health impacts here" there is no study on health impact. In the conclusion, line 21 pag 2298, predictability is mentioned, while there is no consideration on predictability.

Third, and most important, the methodology has some problems, it may well be that I misunderstood a few things, which is in relation to the lack of clarity of the paper that I mentioned above. I agree on the comments on the trends that are formulated by the other anonymous referee, but I have another fundamental problem: I don't see in what the CCA analysis is adding anything to the composite analysis. The CCA is performed between large scale 2d fields and the small field of station data in Georgia. In essence, it boils down to a series of CCA between a vector time series and an essentially scalar one. When I say essentially scalar, I mean that the region where the stations span is so small with respect to the scale of the other maps that all the stations basically vary together. (this would be a good test to do, in fact). Incidentally, note that I had to search for a while to understand whether Tmean95p is a scalar or vector series, this is in relation to my point about the clarity of the paper. Now, a CCA between a vector and a scalar series resembles pretty much to regression, so that it is not very surprising that the CCA patterns and the composite look the same! In addition, I don't see how showing composites of all those fields is necessary. The 500mb winds are in equilibrium with the 500 mb Z field, for example. so is the subsidence in case of high SLP. In summary I think that the methodology used is over dimensioned, and there is too much material presented. I invite the authors to consider exactly the results they want to show, and select the material to give a proof of that. Still on methods: the article first shows trends in a definition of HW, and then looks for dynamical. physical mechanisms to explain HWs in Georgia. Are the time series in the second part detrended? If not, since most of the study is based on correlations, there is a risk that the trend pollutes the results.

Minors, language etc:

Acronyms should not be defined in the abstract. section 3.2.2. no need to repeat "as

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shown in tab3. . ." In general all section 3.2.2 is tedious to read. Main points should be put in evidence, need of synthesys. What is the difference of panel b an c in fig. 8 The article does not really give a proof of the stefano et al mesoscale mechanism is at play here. Or it is not clear.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2273, 2015.