Atmos. Chem. Phys. Discuss., 8, S2945–S2947, 2008
www.atmos-chem-phys-discuss.net/8/S2945/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal temperature range over Europe between 1950 and 2005" *by* K. Makowski et al.

Anonymous Referee #2

Received and published: 23 May 2008

Review of Diurnal temperature range over Europe between 1950 and 2005 by K. Makowski et al.

General comments

This study investigates the observed changes in diurnal temperature range over Europe in recent decades. The study is based on various "Observed" datasets (ECA, GEBA, aerosols sources). The main finding of the paper is the suggestion that the long-term trend of annual DTR has reversed from a decrease to an increase during the 70s and 80s in western and Eastern Europe, respectively. The authors then attribute the shift to the variations in sulfur emissions and related short-wave changes. The paper is about the right length. It is clearly written and the scientific approach is

fair. The methodology is appropriate and complements previous studies performed by the authors on global dimming issues. The authors provide a detailed analysis of their results, but they have to be much more cautious about their final interpretation which is much too conclusive given the caveats outlined below. The authors should consider an extension of their study by using other observed temperature datasets and even model data. The final discussion must also include a detailed description of the limitations of their study.

Major points:

1. The temperature dataset: the authors have only used the ECA dataset to perform the DTR trend analysis. The main problem with that dataset is the quality (or even existence) of the data homogenization process. This point is crucial when estimating trends and trend reversal on short periods. Another related problem is the small number of stations for some countries. I do not think you can say anything about a specific country with only 2 stations. Even when you have more stations as in the case of France, there are still large uncertainties. I have recalculated your diagnostics using a very carefully homogenized temperature dataset (91 stations) for France. Qualitatively, it gives a shape DTR variation similar to yours. However in this case, none of the least-square fits is statistically significant at the 95 or 90% level. The cut between linear and quadratic trend is also not that clear. If I subsample this data in 25-station sets, I bet I can find a pretty wide distribution of trend values. So I don't think you can really say that the country trends have reversed in a particular year (not even sure you can attribute it to a specific decade). I am not sure either that presenting the results by countries is that necessary, really. Indeed, I know that the ECA dataset is the only European observed station dataset which is freely available, so I am not saying that you should not use it. I am just saying that it would much more convincing to show agreement (or lack of) with other results using other available gridded datasets such as CRU, Hadghcnd, or even ERA40 (which indeed have also their own and different limitations).

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2. The detection and attribution issue: what you are trying to do is basically a detection and attribution study. But you are not considering model data (for instance IPCC AR4 simulations, the CMIP3 dataset) and signal to noise issues which could strengthen your results. As for the attribution part, I do agree with you that the global dimming to brightening shift is certainly an important factor. But I don't think it is the only one (you also stress this point at the beginning of the paper but do not discuss it later on) and for some regions it might not be the most important. One has to be aware that DTR data show large changes in variability even for periods with small changes in aerosols. For instance, it can be shown that summer minimum temperature is much less sensitive to large-scale circulation (LSC) than maximum temperature. So low-frequency LSC changes can also induce large changes in DTR variability and trends. You also discuss soil moisture influence for Eastern Europe at the end of the paper, but it could be an important player elsewhere as well. In a formal attribution study, you would have to demonstrate that other mechanisms are unable to explain the observed DTR changes. I am just saying here that you have to be a little more cautious in your conclusions. Another result that has to be much more investigated and discussed is the understanding of the various lags (between the supposed forcing, sulphur emissions, and the response, DTR trend reversal) for the different regions. Why should there be a lag of several years if your mechanism is the dominant one?

Minor points:

1. Page 7053, end of first paragraph: remove or change last the sentence as you are making the unjustified assumption that the climate system responds linearly to the short-wave and long-wave forcing.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7051, 2008.

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Interactive Comment