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Atmospheric Chemistry and Physics Discussions

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

K. Makowski et al.

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We thank the reviewer for the helpful comments. A: indicates the beginning of our reply. If the same issue was raised more than once we tried to answer at the best position possible in the text.

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.

A: We rephrased our final summarizing sentences in the abstract and the conclusions to a more cautious version as suggested by the reviewer: "Consequently, we conclude that the long-term trends in DTR are strongly affected by changes in incoming shortwave radiation, presumably largely influenced by direct and indirect effects of aerosol from sulphurous emissions." "Consequently, we conclude that the long-term trends in DTR could be mostly determined by changes in emissions and the associated changes in incoming solar radiation."

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.

A: We included: "The trend analysis is limited by the lack of a standard homogeneity procedure and by the limited number of available measurement sites and their spatial distribution." into our conclusion section.

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.

A: We fully agree with this statement, to provide a fair and transparent interpretation we

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included the Appendix B giving in most cases information on the spatial coverage and representativeness. Also we would like to comment that because DTR is very sensitive to changes in instrumentation, calibration and location, it is an often used measure to track inhomogeneities in maximum, minimum and mean temperature. Therefore we are confident that the majority of our carefully selected sites are reliable. However, as a consequence to the high demands of quality we sometimes ended up with a small subset of all the available stations. For the European mean we are absolutely sure that the amount of stations is sufficient to be representative for the overall area. If still some stations might contain inhomogeneities we do not expect those to have a strong influence on the mean of 168 sites.

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).

A: We are already in the process of widening our study to different products, amongst those the grid version of the ECA dataset, the CRU dataset and Hadghcnd. Actually we are waiting for an update of John Caesar from MetOffice to have the Hadghcnd available until 2005, so far it is only available until 2000. CRU has some problems concerning the last two decades in Central to Western Europe as well as the former USSR area. From the mid 1980s the number of stations giving DTR information dropped from 500 to about 100; and increased shortly after to about 300 but dropped again towards the year 2000 (Mitchell and Jones 2005). Regarding our reversal period coinciding with this rapid change in data availability we have to be most cautious about the usage of CRU data as you mentioned. We did not think of using the ERA40 data but we will definitely consider it, thanks for this helpful advice. Concerning the model data we are planning to analyze the Ensemble data; however, we assume that including this into the present study would exceed the suitable amount of information for one paper.

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 lowfrequency 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.

A: We included the following section at the beginning of the discussion, according to the reviewers suggestions: "The extent of the DTR is determined by many different factors, such as surface solar radiation or sunshine duration, cloud cover connected with changes in large scale circulation or aerosols, soil moisture and water vapor content of the atmosphere. Change in water vapor for example leads to an asymmetry in the DTR (Stenchikov and Robock 1995) by changing longwave and shortwave downwelling

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fluxes. A continued increase in water vapor due to anthropogenic influence would lead to a slightly reduced downwelling shortwave and increased downwelling longwave radiation at the surface. This would consequently lead to a continued reduction of DTR which, however, we did not observe in the investigated area, and therefore do not consider water vapor as a major factor influencing DTR in Europe. Soil moisture plays an important role by damping the DTR as energy is consumed by evaporation during the daytime and released by condensation during the nighttime. However, according to Robock and Li (2006), long-term changes in soil moisture are coupled to changes in solar radiation and tropospheric air pollution respectively at least on regional scale in Russia and the Ukraine, where long-term records of soil moisture data are available. For the inter-annual variability of DTR the total amount of cloud cover as well as the cloud optical properties play an important role again by altering longwave and shortwave downwelling fluxes (Karl et al. 1993). Clouds alter DTR mostly by damping the daytime maximum via a strong reduction of surface solar radiation, while the influence on the nighttime minimum seems to be rather small (Dai et al. 1999). Apart from local convection, long-term changes in cloud cover can be connected to large scale circulation patterns and aerosols. However, the correlation between DTR and cloud cover in Europe for the period 1910 to 1990 is only 0.35 according to Dai et al. (1997). For the long-term influence of changes in large scale circulation Sanchez-Lorenzo et al. (2008) show that in Western Europe on a seasonal scale, circulation may have an influence on the long-term development of sunshine duration, which can be used as proxy for surface solar radiation (Stanhill and Cohen 2005). Still for the overall annual mean long-term trend in sunshine duration, they identified changes in surface solar radiation from anthropogenic aerosol emissions as a more likely explanation. To sum up, factors influencing surface solar radiation and factors that are influenced by surface solar radiation seem to account for the most of the changes in DTR in Europe. Consequently, we consider changing surface solar radiation as a major cause for the different types of DTR development."

We are currently working on a study which shows that correlation coefficient between

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DTR and solar radiation (at the surface) is about 0.87 for Europe. A comparable result has also been shown for China (0.88) by Liu et al. in 2004. Whether the cause for changes in both DTR and solar radiation can be attributed to aerosols, circulation or cloud optical properties is out of our scope. However, we consider the influence of large scale circulation as an important mechanism, which we will try to account for in the future. Referring to the reviewer's comment on soil moisture, there is just not enough long-term data elsewhere in Europe except for the European part of Russia and the Ukraine. The issue of trends in soil moisture and evaporation is currently addressed by a member of our institute using the available observational data from Fluxnet and the global soil wetness project together with land surface modeling.

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?

A: The problem with comparing estimates of emissions and measurement temperature is fairly simple. The estimates are given on national basis, which omits the fact that emission producing areas are point sources which are usually grouped together. Thus, if we use temperature data from rural and industrialized areas we will always have a significant uncertainty. The published estimates mostly do not take the transboundary transport into account, if they do so the information is derived from modeled data which again produces uncertainty. Unfortunately the available estimates also do not agree too well; the uncertainty of the estimates can be seen by comparing the two different estimates by Mylona 1997/Vestreng et al. 2007 and Lefohn et al. 1999 now shown in Fig. 5 and Fig. 6. Measurement data for sulfur emission to verify the estimates is only available for very few sites and if it exists it lacks mostly sufficient temporal coverage.

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So, connecting DTR for a wide area and emissions is a very demanding task which we are not capable of yet. We will try to address this issue in future model studies which will include sophisticated aerosol treatment from the ECHAM5-HAM model. We included a new Figures 5 and 6 showing the qualitative agreement or disagreement of DTR and emission. In addition, we included sunshine duration as an approximation for surface solar radiation to provide more information on the forcing. Also we rewrote parts of section 4.1 to address the reviewer's remark on the time lags.

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.

A: We deleted the sentence as suggested by the reviewer.

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

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