Atmos. Chem. Phys. Discuss., 15, C8053–C8055, 2015
www.atmos-chem-phys-discuss.net/15/C8053/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observational evidence of temperature trends at two levels in the surface layer" *by* X. Lin et al.

Anonymous Referee #2

Received and published: 14 October 2015

This is an interesting analysis of temperature variations at two heights in the lower atmosphere. While the time series is rather short, there still are some useful results. Some of these results are speculative (such as the implications for estimating global warming and enhancing climate model "predictions") and others are obvious (effects of increased wind speed). Even so, the central point that the height of temperature observation can alter the characterization of temperature trends is an important one to consider and explore. Overall, I recommend publication but with a number of important revisions.

General comments:

(1) The authors have not done much beyond a simple land-cover classification into

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

"grassland" and "cropland" to characterize the local environment of the stations. In particular, a decrease in surface roughness could be largely responsible for the changes seen here. Similarly, changes in irrigation practices would alter soil moisture, albedo, roughness, etc. The correlation analysis helps to get at some of this, but all of these processes are connected through nonlinear feedbacks. As a result, bivariate correlation is not able to demonstrably deconstruct the causes or interactions.

(2) The analysis that uses the MODIS land-cover classification doesn't address the central issue regarding land-cover. It's not the land-cover itself but any change in land cover that could be causing the differential trends. Also, "cropland" that transitions from wheat to corn during this time would have a different response from that that transitions from corn to wheat (in seasonality of the growing season, albedo, surface roughness, etc.). This part of the analysis is rather weak and doesn't add much to the conclusions.

(3) Near the end of Section 3.2, the authors briefly suggest that estimating trends at a single height introduces "an uncertainty that has not yet been accounted for in the use of surface temperature trends to diagnose and monitor global warming". While this is technically correct, it's also a straw-man argument since we already know that we don't have "certainty" with regard to trends at every level in the atmosphere at all locations. What do these results specifically say about uncertainty in monitoring global warming? For instance, is the uncertainty likely to be in one direction or the other? Based on the limited results here, the 1.5m air temperature trend would be an underestimate of the trend at slightly higher levels in the surface layer. Lumping all of this into "uncertainty" doesn't add to the understanding of the issue.

(4) Throughout the manuscript, the authors refer to the lapse rate as a response to turbulent energy exchanges. But it's also the case that the lapse rate is a determinant of turbulent energy exchange. This nonlinear interaction should lead to a more nuanced interpretation of the role of changing lapse rates in the near-surface environment.

Specific comments:

ACPD
15, C8053–C8055, 2015

(1) Abstract: air temperatures at 1.5m are only used over land – much of the global average temperature is from SST measurements. (2) Abstract: “flat terrain” – please quantify. (3) In a number of places (e.g., in the figures), the author say “surface temperature” when they should say “air temperature”. Surface temperature (over land) is yet another different variable with likely different trends from near-surface air temperature (at different heights). (4) Section 2.3: partial derivatives are used where difference operators make more sense, as the variable is discretized and only available at two heights. (5) Section 2.3: “missing data was retained”? (6) Section 2.3: why use percentiles of wind speed to define calm and windy. The 5 windiest could be relatively calm conditions during a particularly calm month. This also results in a different definition of calm and windy in each month, which isn’t that useful in this context. (7) Section 2.3: need a reference for Eq. 5. (8) Section 2.3: Why is potential evapotranspiration referred to as “reference” evapotranspiration? (9) Section 2.3: not sure that that long section on adjusting the t-test is needed – just say that the effective sample size is used to correct for temporal autocorrelation (and give a reference). (10) Section 3: “suggesting a decrease in air humidity” – this doesn’t make sense as the decrease is Td is shown. Do you mean caused by a decrease in humidity? (11) Figures 2 and 3 are virtually illegible and do not add much to this manuscript. (12) Figures: Should make clear that most of these are spatial averages. (13) Figure 11: why were these data smoothed? Also, trend estimates should be given even when not significant.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 24695, 2015.