From: "Karl E.Taylor" <taylor13@llnl.gov>
To: mmaccrac@usgcrp.gov
Subject: to mask or not
Date: Tue, 17 Aug 1999 16:30:58 -0700
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Mike,

I thought maybe I could contribute a few comments to your concern over using a common coverage mask for surface and MSU temperatures. (Copy of your relevent paragraph copied below.)

Whether or not to mask depends on the question being addressed. If we wanted the best estimate of global mean MSU temperatures, then clearly we wouldn't want to mask. The issues we address, however, are largely based on an expectation (from models and observations) that over large portions of the globe strong vertical coupling tends to lead to large positive correlations between surface and lower tropospheric temperatures. There is a further (model-based) expectation that any warming trend at the surface should be slightly amplified higher up in the troposphere. These expectations seem to be contradicted by the MSU data (at least for global mean trends).

Masking makes most sense if there is in fact strong coupling between the surface and troposphere. Suppose the CO2 warming signal were one with relatively strong warming over land areas and weaker warming over ocean. Suppose further that we only had surface temperature measurements over land, but had MSU retrievals over all the globe. Also assume a case of perfect coupling (1K rise in local upper air temperature for every 1K rise in local surface temperature).

In this case the unmasked global mean MSU temperature increase would be less than the "global" mean surface temperature increase, falsely indicating a damping with height of the CO2 signal. If we masked the MSU temperature (sampling only over land), then the global means would be computed over the same area as the surface temperature and the MSU temperature change would equal the surface temperature change, indicating no damping of the response with height. This second conclusion would be the correct one. Note, however, that the true global mean temperature change (both at the surface and aloft) would be best estimated using the MSU unmasked data (under the conditions of this hypothesized case).

Under different conditions, and again depending on what question is being addressed, it might be best not to mask the MSU data. In our paper we wanted to determine whether the apparent discrepancy between the MSU trend (very small) and the surface trend (positive, and larger) could be explained by coverage differences. This makes sense since models seem to indicate that the trends should be comparable. One explanation for the discrepancy is that in models true global means had been considered until now, whereas in the data the MSU mean was computed from global coverage, but the surface changes were computed from data covering about 70% of the globe. In our study both model data and observations were treated with the same mask so we rule out different sampling as a full explanation for the difference between surface and MSU temperature trends.

Hope this doesn't confuse things further.

cheers,
Karl

Mike wrote (in part):

I think one needs to be very careful about this coverage argument--basically becuase the atmosphere can move anomalies around compared to the surface. One would just not expect therir spatial patterns

to be the same, so taking a common spatial maskwill not resolve this (even if it seems plausible). To illustrate, take an extreme example of there only being sfc msmts for the equatorial eastern Pacific (the El Nino region). There, the MSU and sfc temp go in opposite directions for quite plausible physical reasons. Doing a mask and comparing for that small region would make no sens and give negative correlations, etc. Now, in that sfc obs cover most of the globe, the problem will not be so severe, but it persists (it was for this reason that I was suggesting extrapolating to the global value for sfc temp based on changing coverage--not sure how to do that however). In any case, I believe taht MSU and sfc should only be compared, if at all, for the globe as a whole.