

Interactive comment on “Large-scale transport into the Arctic: the roles of the midlatitude jet and the Hadley Cell” by Huang Yang et al.

Anonymous Referee #2

Received and published: 11 October 2018

In this paper, the authors analyze three different tracers from the CCM1 models in order to understand transport of tracers that are emitted in the NH to the Arctic. They find that transport from NH midlatitudes depends on whether sources are ocean or land-based and, in the case of land-based sources, on the climatology of the Pacific jet. They explain that this is because the jet is associated with the Hadley cell edge and associated meridional flow. Generally, I find this paper to be well written and clear. The result that the realistic CO and CO50 are not closely related is an important one, I think. It emphasizes the need to analyze the output of real chemical tracers and compare to observed tracer data. The differences between NH50 and CO50 are also important.

While the physical mechanisms proposed by the authors are plausible, the supporting evidence is based on correlations of decadal averages within a very limited number of

C1

models rather than a clear causal indication of the process. It is clear that the number of models that can be examined is not the fault of the investigators, but given these constraints, it is not clear that any strong conclusions can be reached using these methods. In particular, the number of independent pieces of information is unclear. WACCM and CAM-1 seem nearly identical in the scatter plots. The EMAC-47 level model and EMAC-90 level model are definitely not independent, and considering the frequent grouping of these with their SD runs in the scatter plots, it would be useful to know how many independent data points these four represent. Since, as the authors note, the correlations can be easily influenced easily by which models are included the choice of weighting the single ACCESS run as equivalent to one of the EMAC runs, for example, could change the results significantly. I would expect that the mechanisms the authors point to would work on shorter timescales than decadal, so it is strange that the small dots of the annual averages do not fall closer to the relationships that are calculated. Perhaps an annual average is too short for the process to be valid, but why should a decadal average be necessary? Some filtered time series showing that this process is valid within a model and not just between models would be helpful, as it would help convince the reader that this physical mechanism is correct. Maybe GEOS has a longer record that could be used? And if the relationship only holds between models (i.e. the explanation of the jet and the tracer concentration only explains the difference between models, not the physical processes within the models), then it just implies that biases in models between transport of tracers and large scale circulation near the tracer sources are correlated.

The statistical methodology is problematic. The correlations are done using least squares, which necessarily holds one variable as dependent and one as independent. This is why all of the “fits” where there is no correlation are horizontal lines. Since there is no fundamental reason to expect that either quantity should be an independent variable, this method is not sufficient. Either the fit needs to be done with the x and y reversed (in $y=ax+b$) and that slope should be plotted as well, or a reduced major axis regression, with each quantity scaled by its variance, should be employed. With small

C2

r values (e.g. Figure 3), no fit line should be shown at all.

The analysis of the differences between GEOS and GEOS SD is fine but since GEOS is shown to have quite different behavior from the other models in Figure 9, the conclusions drawn from this comparison are not obviously going to apply to other models.

My final concern is that analyzing transport in SD runs seems inherently problematic, since it is unclear whether they are actually conserving tracers. I assume that, since the authors are analyzing these, they have reason to believe that it is not a problem. A discussion of the extent to which the SD runs do or do not conserve mass and tracer concentrations would be helpful—I'm not sure if such a study already exists or if the authors could do some analysis of their own with the GEOS-SD model.

I wish I could provide more constructive feedback. Process-based analysis of the tracer transport in individual models seems necessary to demonstrate that the plausible mechanisms shown by the weak correlations are in fact the causal mechanisms. It seems like the authors could do this with GEOS, at the very least, and they could potentially get output from at least one other model to do the same analysis to demonstrate consistency. If the authors can address these substantial issues, I would be willing to review a revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-841>, 2018.