Geosci. Model Dev. Discuss., 5, C885–C888, 2012
www.geosci-model-dev-discuss.net/5/C885/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "PORT, a CESM tool for the diagnosis of radiative forcing" *by* A. J. Conley et al.

Anonymous Referee #2

Received and published: 9 November 2012

Review of "PORT, A CESM Tool for the Diagnosis of Radiative Forcing" submitted to GMD by A. J. Conley, J.-F. Lamarque, F. Vitt, W. D. Collins, and J. Kiehl

The paper by Conley et al. introduces PORT a new tool to calculate radiative forcing based on the radiation code in CESM-1. Such a tool would be very useful for a wide community and thus the paper is of potential great value. However, the current manuscript lacks some structure and some points need clarification, and most importantly it lacks a broader discussion of possible errors for aerosols and short-lived climate forcers. This is important as users may not always be aware of these caveats.

My recommendation is: Accept with major revision.

A discussion about how PORT may work for aerosols must be added. For aerosols

there are potentially large correlations between aerosol distribution and optical properties and the distribution of clouds. The aerosol distribution is affected by vertical mixing and wet scavenging both closely related to clouds, while the optical properties are affected by swelling related to the relative humidity, again related to clouds. I would propose the following tests to be performed.

1. Use CESM-1 and sample sulphate aerosols every time-step and also every 73 time step and compare the results (similar to the 2xCO2 test). Run PORT with the same sampled physical state as was the case in the CESM-1 run used to generate the sulphate samples. With this setup clouds, vertical mixing, humidity etc. should be consistent. 2. Use the sulphate sampled every 73 time step as above but with a physical state sampled from a different model year(s) in CESM. This would show how large error could be when PORT is used for fields derived from other models (e.g. as in the ozone simulations based on the Stevenson et al. paper)

To follow up on this it would be very useful to have the normalized forcings for the ozone experiments (Table 2). The discussion of the ozone results as it is now adds very little information about the performance of PORT. In the Stevenson et al. paper the results using PORT have been compare to the radiative forcings using two other schemes. Some of that information could be used here to discuss the properties of PORT.

Page 2695, line 13-15. Based on the discussion above, I am not convinced that this conclusion is generally valid. If prooved OK, this should be reflected in the abstract.

Page 2695, line 16-17. The experiment described in the text show that ozone RF is linear in the vertical (i.e. RF by tropospheric and startospheric change separately adds up toe the total RF). However, the statement oin the text indicates that it is linear in time (i.e. in total burden) which might be quite true, but it is not shown here.

Abstract: Too short and does not reflect the properties of PORT.

Figure 1. The explanation of this in the caption and in the text should be improved.

Add a sentence to describe how the perturbation (as given in figure 1 is derived). Figure 1 should also show how CAM4 data such as clouds, humidity, etc enter the framework. From Section 3 I read that CAM is run offline first and the physical state of the atmosphere is sampled at given time intervals and used as input to PORT. I suppose the text in the parenthesis in line 22 on page 2689 is meant to explain this, but it needs to be elaborated. All this is well explained in section 4 and a cross reference to section 4 should be added.

Minor comments:

Page 2689, line 1. I suppose the not only CFC but also HFC and HCFC are included. I suggest replacing chlorofluorocarbons with halocarbons. Is nitrate aerosols included in the code?

Page 2691, eq. 4. Is the T_m factor a miss-print, should be T_p? Otherwise it must be defined.

Page 2691, eq. 7. The M-factor in the last term Q(T-M*T_sa, c_p) is not needed. If M=0 then the whole left hand side of the eq. is zero, and if M=1, it doesn't matter in the term Q(T-M*T_sa, c_p). Page 2692, section 3. The discussion about the robustness of the sub-sampling is limited to the 2xCO2 case, while the introduction (page 2689, line 1) state that the code includes also short-lived components with more heterogeneous distribution and aerosols with large SW effects. Also these cases should be discussed in section 3, i.e. Table 1 needs to include also experiments with ozone and e.g. sulphate and black carbon aerosols. One might expect that the sub-sampling error is quite a bit larger for these cases.

Page 2692, section 3. Recent studies by Shindell and Co-workers (e.g. Shindell, Atmos. Chem. Phys., 12, 7955-7960, 2012 and references therein) have indicated that the radiative forcing from short-lived components in latitude bands can be used to derive information about the latitudinal variation in the temperature response. In light of this it would be good also to see the error due to sub-sampling for annual mean RF

in 30° wide latitude bands.

Page 2692, line15: The sentence: "Additional analysis (not shown) have indicated the low biases associated with our chosen 73-step sampling" does not give much meaning. What is this sentence telling beyond the text above?

Page 2694, line 22. It took me some time to understand what the three TARGETS were refrerring to (I suppose it is ozone changes in the troposphere, stratosphere or both, line 14). Please make it clearer.

Figure 4 versus Table 1. The relative error given in Table for the global annual average is about 5x10-5 for the LWTOA which is about equal to the maximum error for a given day and latitude in figure 4. I would suspect that the local error due to sub-sampling should be considerable larger than the global annual average?

Interactive comment on Geosci. Model Dev. Discuss., 5, 2687, 2012.