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Interactive comment on "Contribution of emissions to concentrations: The TAGGING 1.0 submodel based on the Modular Earth Submodel System (MESSy 2.52)" *by* Volker Grewe et al.

Anonymous Referee #2

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This new model development by Grewe et alii continues the development of tagging tracers in various ways to attribute environmental degradation to specific emissions (or possibly other actions). The method is reasonable but certainly not a unique or singularly correct approach to derive the environmental damage for a given set of actions. Grewe has made some very specific assumptions about how to partition a key species like tropospheric O3 into a unique sum of causes. The choices made are plausible, but there are readily available other methods (e.g., the 'perturbation method', line 50, which goes back decades before the Grewe references listed here indicate). I tend to concur with the other RC1 review in that a diversity of approaches can always teach us something. Thus, with some chemical model revisions and with a recognition that

tagging does not just give us the "contribution", the model should be published

There are several serious problems with this GMDD paper as written that might be cleared up with major revisions affecting both the (i) chemical modeling and (ii) the authors' choice to describe the tagging method as the true 'correct' method and thus discussing the errors, for example, in the perturbation method. There is an additional (iii) potential problem here in that the extensively expanded TAGGING here may still lack the full chemical coupling across species and regions that has been demonstrated in perturbation experiments with fully coupled models. There are some very interesting results here, but the paper needs to be a bit better balanced and informative.

(i) The chemical model has some clear problems in language or concept. For example, CH4 does NOT make NMHCs. The production of NMHCs (e.g., C2H6 etc) comes from the sources. CH4 is a major source for H2CO in the remote troposphere but that species is not listed here and is not an NMHC. At best it might be a VOC. The NMHC reactions in the table make no sense.

Another mistake appears to be the lack of photolysis of O2 as an important source of O3 in the tropical upper troposphere. This is well established and I can only take it that the old photolysis lookup tables used here have cut off this process in the troposphere? Otherwise the explanation for tropospheric O3 sources does not makes sense. Putting in N2O emissions is interesting, but I do not see where it is then listed as a source of tropospheric NOx (∼0.1 Tg-N/yr)? Moreover, the N2O-CH4-O3 system in the stratosphere is quite complex and controls the net trop influx of O3 and NOy. That system is also established as a coupled perturbation, but is not accurately represented in this tagging model.

Almost all modern scavenging algorithms for species such as H2O2 and HNO3 and others follow the rainout and washout AND re-evaporation of these species in layers below the original uptake. The method of tagging here would seem to be inadequate to deal with this.

(ii) The tagging method as best I understand is built to achieve a zero sum in that a certain level of, say O3 concentration, is partitioned into the different sources so as to sum correctly. This is clearly a personal choice, since I would prefer the linearized tangent approach in which the differential evaluated at the current atmospheric state is used to calculate the change in O3. This of course will not lead to a sum of all the components being zero because the chemistry is non-linear over the range from zero to full industrial emissions. This has a similar issue with CO2 and radiative forcing attribution, since the RF of CO2 goes as the log of its concentration. In this case we have the "which came first?" or "straw that broke the camel's back" problem. Recent increase in CO2 has a lower impact than earlier ones. For many of us the only fair way is to do the perturbation experiment (flat slope at high CO2) and then realize that the sum of all tangent additions if scaled to zero would not be equal to the current state. This is the same problem with the attribution of say biomass burning, and there is no single correct answer. This paper needs to realize and carefully explain the arbitrary choices made.

There is a worrisome statement in the introduction that somehow demonstrates the absurdity of the different approaches. The idea that tagging is the 'correct' answer is just incorrect. It is indeed one of the answers, but not necessarily the best: "For example, the change in ozone due to a 100% reduction in road traffic emissions is smaller by a factor of 5 than the contribution of the road traffic emissions to ozone." The tagging method is obviously defined here to be "the contribution of ", but as a policy maker who wants to know what happens if I reduce road traffic, I would prefer a 100% reduction as the correct answer, or I might choose the 5% reduction times 20. Clearly if I reduce road traffic by 5% or by 100%, the perturbation run, not the tagging run, is the correct value. Does the tagging method do any better than a series of 5% reductions scaled across different emission sources? Moreover, one would not be interested in attributing the background basic atmospheric state (e.g., lightning emissions) in a proportional basis with those of industry, since the background state is not billable for damages as an anthropogenic perturbation is.

The authors continue to use the English word contribution as a code word for their own specific method for dividing up species concentrations. Lines 530-535 argue that the perturbation method "underestimates this contribution" by a factor of 2. I would assert that the "tagging-contribution" is larger than the perturbation by a factor of two and that one of the causes may be the lack of full tagging. The other reason is the apparent need to tag everything including background processes in a similar way to pollution sources. Personally I am not sure that the treatment of the background atmosphere by tagging is done well (e.g., the stratosphere) and thus the partitioning here may be specific to the assumptions made.

Also the authors really need to explain their $\frac{1}{2}$ factor throughout the equations (starting with eqn 3). It is far from obvious since a simple Taylor expansion would not give $\frac{1}{2}$. Please help us out. If it does not apply to small perturbations but only when trying to ensure that the sums are balance, then explain.

(iii) There are clearly identified global chemical coupling patterns that reach across species and regions (strat vs. trop). They are readily identified in models through perturbation simulations. For example, these early chemical feedbacks of tropospheric OH-CH4 and the N2O-NOy-O3 in the stratosphere have been demonstrated to work across many models in various IPCC model comparisons. These are important because they affect the lifetime of a perturbation and hence the attributable damage of emissions. They are most surely in the full MESSY model. From the couplings of this tagging method, I do not believe that these fundamental couplings are present in the tagging model. If you could demonstrate that both of these feedbacks can be derived from the tagging then it would be convincing. Otherwise it shows that tagging really cannot include the dominant chemical feedbacks of the lower atmosphere. This lack of full coupling in the TAGGING model means that one cannot be sure what chemical feedbacks are not included.

Other issues: (iv) The method is described as low cost and non-intrusive, but the only global example given is for T42 resolution (2.8 deg). This is very low resolution for current global models, yet this is only a GMD paper to establish the development of the model. OK, but can you run at T159 (1.1 deg) for example with all the memory requirements for the tagged tracers to be transported? I had thought that tracer transport was one of the dominant costs of high-res CTMs. Indeed, line 564 seems to indicate that you already have memory limitations at T42.

(v) What was the STE flux of O3 and NOy as a function of latitude and season. This would seem to be very important since the background atmosphere shares the attribution in this scheme. Please denote.

(vi) The idea that the Mediterranean Sea contains "pristine areas" anywhere is at least humorous – thanks.

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