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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Attribution of ozone radiative forcing trend to individual NO_x sources" *by* K. Dahlmann et al.

Anonymous Referee #2

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In this paper, Dahlmann et al. attempt to quantify the contributions of various NOx sources to the radiative forcing due to ozone changes in both the troposphere and the stratosphere. They use a chemistry-climate model to calculate the forcing due to the following processes from 1960 to 2018: lightning, biomass burning, soils, industry, road traffic, ships, air traffic, stratospheric N2O degradation, and O2 photolysis.

General comments.

I rate the paper as poor. The authors have an incomplete understanding of how the concept of radiative forcing is applied in scientific studies. The paper adds little to our understanding of the contributions of NOx sources to ozone forcing.

Radiative forcing is typically defined as the change in radiative balance due to a per-

turbation in some atmospheric constituent, solar insolation, or surface quantity. Anthropogenic activity indeed imposes a perturbation on atmospheric constituents, and the authors are on safe ground when they compare the radiation fields of pairs of simulations with and without the anthropogenic emissions of industry, road traffic, ships, and air traffic. But lightning, biomass burning, soil NOx emissions, stratospheric N2O degradation, and O2 photolysis are all processes that have been ongoing for millennia. Calculating the total radiative impact of any one of these natural processes, as the authors have done here, tells us little and is not interesting. What would be interesting is whether any of these natural processes have changed (or are expected to change) and to what degree these changes have altered the radiation fields and thus climate. The authors begin to examine the issue of change in natural processes, but they do not go very far in this direction.

In the section "Additivity," the authors appear to calculate radiative forcing due to ozone by comparing the radiation fields of a pair of simulations, one with all the ozone production processes turned on and one without all these processes turned off. (This is, at least, my understanding). In my view, the authors' approach is akin to comparing a pair of simulations with and without any CO2 present in the model, an exercise with some pedagogical value but not helpful in understanding changes in current climate. The authors do not convince me that such a calculation for ozone has importance.

It is not clear what value the examination of stratospheric processes adds to the paper. Is the photolysis rate of O2 in the stratosphere expected to change? Certainly N2O abundance in the stratosphere is changing in recent decades, but this issue was not addressed.

The paper could be salvaged by (1) looking at the impacts of the changes in natural emissions over time, (2) examining trends in the different forcings for significance, (3) examining the seasonality of the forcing, and (4) dropping the discussion of stratospheric ozone forcings. Figure 8, which shows the trends in NOx emissions, ozone production efficiency, and radiative forcing efficiency for the troposphere over the 1960-

2019 time period, would be the focus of this revised paper.

Most references in the paper are outdated, from the 1990s or before. Problems in spelling and grammar appear at the rate of 1-3 per paragraph.

Specific comments.

Abstract. Here and throughout the paper the author need to be clear about how they are calculating radiative forcing: exactly what perturbations in ozone production have they imposed on their model? For example, the text says, "Lightning . . . causes the highest specific RF [radiative forcing]," but it is not made clear that this is forcing relative to a case with no lightning.

Page 16133. In the introduction the authors need to make clear how their work builds on the work of others. The most recent paper cited here is Stuber et al., 2001. More recent relevant papers, such as Unger et al. [2008] and Fuglesvedt et al. [2008], are not brought up till much later in the paper. Papers examining the relationship between ozone forcing and climate, such as Hansen et al. [2005] and Mickley et al. [2004] are neglected.

Page 16134-16136. The sources of the all NOx emissions, past and future, need to be supplied. More details about the natural emissions should be supplied. Also, I was surprised that the chemistry model omits non-methane hydrocarbon chemistry. The authors need to provide a quantitative estimate of how this omission affects their results.

Page 16137. "An average annual cycle of each ozone field . . . has been taken." Are these monthly means? It would have been interesting to look at seasonal effects of the NOx emissions, since ozone concentration has a strong seasonal cycle over mid-latitudes.

Page 16139. "The reason for a higher ozone production efficiency of lightning and air traffic is the higher amount of UV radiance at higher altitudes." Actually the reason for

the high OPE is greater dilution of NOx at high altitudes.

Figures. Captions for all figures should be stand-alone. Where quantities are calculated (and not observed), that should be noted. Global annual averages should be identified. In Figure 2, NO2 should be N2O. Figure 4 is not necessary, as it appears to present calculations from another work.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16131, 2009.