

Interactive
Comment

Interactive comment on “Will climate change increase ozone depletion from low-energy-electron precipitation?” by A. J. G. Baumgaertner et al.

Anonymous Referee #1

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This paper addresses an important and interesting question, namely whether climate change could change the role of EEP-produced NO_x on stratospheric ozone. In general, with a few exceptions mentioned below, their presentation is satisfactory and their basic result is credible, although I think they could have added some more “meat” to the paper to give a more complete picture. I have some specific suggestions; I don't think they're hard to implement, but would require a bit more analysis and plotting.

The paper limits itself to a single test of this hypothesis- using the single year of 2003 and a single hemisphere (SH) as a template for EEP. Ultimately, this appears linked to the availability of MIPAS data for this year. Is this correct? If so, that should be made

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clearer.

But in general, other scenarios really should be considered. For example, the work of Randall has shown that NO_x in the present-day NH stratosphere generally experiences less EEP effects than in the SH, although for a couple of unusual years (i.e. 2003-04), effects can be significant. Indeed, the authors' most recent work, last year in ACP, showed an ability to assess EEP effects in the NH. What about in the year 2100? I recognize that simulating the unusual SSWs that have recently occurred is likely to be problematical, but any study of B-D changes should address both hemispheres. Who knows, perhaps with a speed-up of the B-D circulation, the current NH-SH asymmetry in EEP significance would be reduced? That would be significant and the authors are well poised to address that question.

Second, to validate their 2100 circulation and associated B-D speed-up, the authors limit themselves to a single figure, Figure 6, which only goes up to 1 mb. But this is insufficient since by their own admission, the ozone changes are also partially due to cooling of the stratopause. So the figure combines two effects, making the isolation of transport changes impossible. I have three specific recommendations here.

1) To validate their circulation changes, they should show a passive tracer- for the stratosphere, this means CH₄ or N₂O. They should show this field for present day and for year 2100.

2) Then, also, the vertical domain should be extended up to at least 0.1 mb, particularly since their previous paper showed results as high as 0.1 hPa. They then should add a mesospheric tracer such as CO (which they showed in their 2009 paper).

3) Finally, I strongly urge them to abandon the annual average and break their analysis up into seasons. Or at least, if they want to keep it simpler, NH and SH winters.

More minor comments

1. Page 3, bottom paragraph. The authors should explain here the climate change

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scenario since many people will not have access to the Nakicenovi reference.

2. Page 4, top. The “another two simulations for present day conditions”. . . . What’s the difference between those simulations and what they published in ACP last year (Baumgaertner et al , 2729-2740)?

3. Page 4, bottom paragraph. Listing the MESSy submodels by their specific routine names is only really helpful if you are reading a manual with the code in front of you. (is it really helpful to know that their cloud routines are called CLOUD?) Better just describe the parameterizations clearly with the associated references. Even worse, on the top of page 5, they give the routine name (MECCA) without any description at all!

4. Page 5, end of Section 2.1. Is that really a web site? It doesn’t look like one and my browser was unable to find it. Please check.

5. Page 6, the bottom and page 7, top. The description of the Nakicenovic A2 scenario doesn’t read well. What does “fragmented technical change” mean? I know that this phrasing is copied almost word-for-word from the IPCCreport. The problem is that it is not self-explanatory and meaningless to readers not familiar with the IPCC process (most of them).

6. Page 7, end of 2.3 and also for subsequent figures and discussion. Is micro-mol/mol the same as ppmv? If so, are we to understand that they are assuming CO2 more than doubles during the next 90 years (its about 392 ppmv now: <http://co2now.org/>). That sounds overly-extreme! If micro-mol/mol is not the same as ppmv, is there a reason they can’t convert to more familiar units?

7. Figure 5: I think there is a critical typo here: should not one of the boxes say “EEP NOx off”? They all say “on”.

8. Assuming, my hunch about Figure 5 is correct, I would appreciate more diagnostics than simply Figure 7. I suggest showing some of the components of the effects of B-D change on NOx. Thus the EEP NOx effect for year 2100 and separately for the present.

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Then the third panel could be the difference, which if I understand Figure 5 correctly, is the current Figure 7.

9. Finally, the authors should recognize that the acceleration of the B-D circulation with changing climate is a theoretical result that is not fully validated. Thus, Engel et al (Nature Geoscience, 2, 28 – 31, 2009) have not found changes in the SF6 lifetime that one might expect. I suggest this be acknowledged and Engel et al referenced rather than presenting the B-D change as completely validated.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9895, 2010.

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