

Interactive comment on “Effects of climate-induced changes in isoprene emissions after the eruption of Mount Pinatubo” by P. J. Telford et al.

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Received and published: 11 June 2010

We thank both referees for their constructive comments and proceed to provide detailed replies. The major difference between our ACPD paper and this draft we submit to ACP is the addition of some extra simulations to test our sensitivity to uncertainties in isoprene oxidation chemistry. We feel that the relative insensitivity of our results to these changes helps to indicate the robust nature of our conclusions that variability in isoprene emissions driven by changes in climate affect the oxidising capacity of the atmosphere.

Anonymous Referee #1

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Received and published: 19 March 2010

The authors investigate the effect that climate changes in the early 1990’s (including the Mt Pinatubo eruption) may have had on atmospheric composition via changes in isoprene emissions. This work makes use of the UKCA model with the SDGVM vegetation model. The authors suggest that decreases in isoprene emissions following Pinatubo may have led to elevated OH and an increased sink of methane. The article is succinct and the topic quite interesting. I have only a few suggestions to improve clarity.

1. Page 6874, line 22: Specify the year for the SST and sea-ice cover from HadISST

The HadISST data is always a time series from 1989 to 1997. We agree that this is a slight inconsistency when it comes to fixing the meteorology, but as the nudging constrains the model quite strongly we don’t believe this is a big effect. We have clarified this in the text.

2. Page 6876, lines 1-2: “UKCA integration” is a bit ambiguous – please specify if the nudged ERA-40 meteorology used to drive the isoprene emissions as well.

Yes. This has been clarified.

3. Page 6876: Has the dynamic vegetation simulation been evaluated? How realistic is the distribution compared to satellite-derived LAI over the early 1990’s?

We examined the AVHRR vegetation (Pinzon et al (2005), Tucker et al (2005)) and there appear to be decreases in vegetation in eastern Brazil around where we model them in 1992. However we note that there are uncertainties associated with these measurements in the Amazon basin, so even if there was some disagreement with this data wouldn’t necessarily invalidate our conclusions.

4. Section 2: The Pinatubo forcing should be clearly described in the text. My understanding is that the authors have included only the meteorological impacts

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included in the nudged meteorology, but have neglected the effect of changes in both stratospheric sulfate and ozone on radiation (which could also impact isoprene emissions). This should be clearly stated.

This is nearly true. We have clarified this to avoid such misunderstanding. The main change is produced by nudging to ECMWF analyses. There is also some forcing from the SSTs, although as we note above we believe this is less important than the nudging changes. We also include the changes in optical depth from the aerosol changes (though not from the ozone changes). However the photolysis is from an off-line scheme and is insensitive to changes in the SW flux. The emissions model does see the changes in SW flux after the eruption from both the aerosol and the ozone (see Telford et al 2009), although not the changing ratio between direct and diffuse fluxes.

5. Page 6878, lines 17-24: Clarify if the calculated correlations are temporal, spatial or both.

The correlations are spatial averaged over 1992. We have clarified this in the text.

6. Section 4.1: The authors might consider adding a definition sentence here – it can be confusing for the reader to separate “climate” and “emissions” when isoprene emissions are climate-dependent. One might assume that a fixed-climate run (Metfix) would imply that isoprene emissions would be constant, which is not the case here.

We have rephrased this to try and avoid this confusion.

7. Page 6881, lines 6-7: typo? I believe the authors mean that Base-Metfix represents the effect of interannual variations in climate.

That is what was intended and the text has been corrected to reflect this.

8. Page 6881, last paragraph: The issue of isoprene oxidation at low NO_x is a very important topic. I recommend that this be discussed very briefly in the introduction as well – the knowledgeable reader will look for the context of this

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study early on.

We have performed sensitivity studies for 1992 with a simple parameterised OH recycling scheme included [it involves the addition of the reactions $\text{ISO}_2 \rightarrow \text{OH} + \text{MACR} + \text{HCHO}$ and $\text{ISO}_2 \rightarrow \text{OH} + \text{MGLY} + \text{HACET}$]. We find that although OH is greatly increased in places, with tropospheric columns increasing by up to 100% in regions low in NO_x and high in isoprene, the changes that we show in Figure 5 are very little affected. We would not claim that the mechanism that we include is necessarily correct, but we do feel that the robustness of our results to its conclusions increases our confidence as to the magnitude of the impact of the changes in emissions on OH. We have expanded our sensitivity discussions to talk about these changes.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6871, 2010.

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