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Atmospheric Chemistry and Physics Discussions

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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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 #2

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The authors attempt to quantify the effect of volcanic aerosol emitted by Mount Pinatubo on emissions of isoprene and subsequent atmospheric chemistry via the hydroxyl radical. This is one of the first studies using the new UKCA chemistry model. This is an interesting study which merits publication in ACP. However, there are a number of issues that need addressing before it should be accepted for publication.

Is OH recycling addressed in the isoprene chemical mechanism? If it is not, the authors should comment on potential implications of this recycling on their results.

See comment to referee 1.

The authors state that changes in UV due to the volcanic aerosol are included in the calculation of SW radiation but not in the calculation of photolysis rates. This is potenltially an unnecessary simplification. At the very least, I think it would be useful to give the reader some indication (qualitative or quantitative) of how this simplification affects their results.

We proceed in this manner because of the nature of the photolysis code in this version of UKCA, which is based on offline tabulated rates. We agree that this misses some important changes around the time of the eruption (see Bekki et al 1994). We have added a discussion of this in our results.

It's not clear to this reader why the authors expect a strong global correlation between isoprene emissions and the ENSO index.

The global correlation has been noted in the literature (eg see Muller et al 2008, Lathiere et al 2006) and we believe that it arises because El Nino events tend to produce warmer conditions globally and, as we note in the text, temperature appears to be the most significant driver of emissions changes.

I would expect the correlation to be much stronger over particular regions of the world. Have the authors looked at regional correlations?

We can pick regions with stronger correlations, for instance the correlation between ENSO and the isoprene emissions over south and central America exhibit a stronger correlation than the global one. However even in this region there are areas where ENSO increases the emissions and areas where it decreases them. This is complicated by the effects of the eruption as well, reducing warming and enhancing drying.

The authors state that one of the most striking exceptions to isoprene emissions occurs over western Amazonia where emissions increase in 1992. The authors don't fully explain this, even though they have a model which can be diagnosed. The authors state that establishing the exact causes of emissions is difficult due to the complicated nature of the algorithm. This reader notes that it is difficult but not impossible. Using a simple correlation is low-level data analysis using which rarely improves understanding. The correlation of 0.21 is unlikely to be significant – changes in temperature explain 4 percent of the isoprene emission variability!

Using a simple definition of the Pearsons correlation coefficient the correlation of 0.21 is significant with the numbers of points studied. As we note in the text the correlation is larger in certain regions(eg N America 0.69). The global correlation is reduced because there are some regions where, due to reductions in biomass and reductions in the soil moisture activation factor in the emissions model increases in temperature reduce emissions. As you note therefore a simple global correlation is only of limited use and we employ it merely to highlight the main driver of emissions changes.

Other factors such as drought, affecting LAI and isoprene emissions, are neither fully described by the vegetation model (rooting depths accurately described?) nor fully explored by this study.

Although, as with all such global vegetation models, the SDGVM makes many simpli-

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fications it has been extensively tested (see Beerling and Woodward 2001, Woodward and Lomas, 2004). Of course we cannot exclude the possibility that the response to drought are exaggerated as there are limited data sets to evaluate these changes with. However, as we note in the text, there were field studies at the time (eg Milton and Dean) that observed dramatic changes in biomass.

The soil moisture activation factor in the MEGAN model does attempt to describe the impact of drought on the emissions through the closing up of the stomata.

Is there a notable change in the seasonal distribution of LAI in 1992/1993 compared to previous years? Additional analysis is clearly required.

There are some seasonal changes to vegetation and emissions, but the reduction in both is, on general, throughout the globe and the year.

In section 4.2, the authors state the impact of changing isoprene emissions on the OH burden is of the same order of magnitude as the change meteorology. In the next paragraph they state that any conclusions cannot be assured to be quantitatively accurate. This reader is left wondering whether the results are quantitatively robust given the acknowledged simplifications. Consequently, I suggest the author revise the wording associated with the size of the impact in the abstract and conclusions.

We note that we already emphasised the uncertainties from emission and oxidation of isoprene in our conclusions. We accept that we neglected to mention this in our abstract and have rectified this.

The discussion about the impact on ozone and aerosol is brief and dismissive. The global impact is small but what is the regional impact? Is this worth showing?

The local impacts on ozone, in places, are significant though still small (up to 2% in the Amazon basin), especially as the changes occur in regions with low ozone con-

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centrations, reducing the impact of the changes on the global burden. In the version of the UKCA model used in this study secondary organic aerosol from the isoprene is not included, so we can only speculate on the changes in aerosol concentrations. We note that these changes would be interesting to examine with the more complete UKCA model that includes these processes (Mann et at, GMD in preparation) which could also incorporate the effects of changes in the turpene emissions which are also produced by the emissions model.

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