

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

**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.

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 poten-

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tially 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.

It's not clear to this reader why the authors expect a strong global correlation between isoprene emissions and the ENSO index. I would expect the correlation to be much stronger over particular regions of the world. Have the authors looked at regional correlations?

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

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.

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?

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