

***Interactive comment on “Characterization of wildfire NO<sub>x</sub> emissions using MODIS fire radiative power and OMI tropospheric NO<sub>2</sub> columns” by A. K. Mebust et al.***

**Anonymous Referee #2**

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The presented study uses NO<sub>2</sub> retrievals from OMI in combination with the MODIS FRP product to derive NO<sub>x</sub> emission coefficients from wildfires in Nevada and California. Recent studies have shown promise in using FRP together with satellite retrievals of atmospheric composition for better constraining fire emission estimates. Most of these studies have focused on aerosols and as such the presented work will make a valuable contribution to this field by providing estimates for NO<sub>x</sub>. The paper is well written and the methods and results well presented. I also acknowledge that the authors make an attempt to provide a characterization of the uncertainties.

I recommend this paper for publication if the following points can be addressed ade-

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quately.

(1) In line 220 the authors state that errors in wind speed and direction are difficult to address and hence were neglected. Have they explored how the results change when using different data sets (e.g. NCEP Eta North American Analysis) or a different vertical level? What impacts would they expect when emissions are injected at higher altitudes such as could be the case for especially forest fires?

(2) Line 416: what is this error estimate of 10-20% based on?

(3) I am worried about the large difference between the emissions coefficients derived in this work compared to previously reported values. Could the derived values actually be used and trusted in any way? Or is the intention of the authors simply to present their method and the underlying issues? In this case, could they make recommendations for what is needed and feasible to improve the approach?

(4) In the conclusions one possible explanation that is given for the large discrepancy between these and previously reported values is that NO<sub>x</sub> emission from California fires area lower on average compared to other regions and fires studied. Given that the OMI and FRP are global data products, would this not be a hypothesis the authors would be able to test?

(5) Could SCIAMACHY NO<sub>2</sub> data be used to test if and to what degree the discrepancy between these and previous estimates is due to a bias in the OMI NO<sub>2</sub> retrievals?

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