



EGUsphere, referee comment RC2
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Comment on egusphere-2022-345

Anonymous Referee #2

Referee comment on "Statistical modelling of air quality impacts from individual forest fires in New South Wales, Australia" by Michael A. Storey and Owen F. Price, EGU sphere, <https://doi.org/10.5194/egusphere-2022-345-RC2>, 2022

The study modeled PM_{2.5}, measured at air quality stations in NSW Australia, from 1500 historical individual fires as a function of fire and weather variables. It combines fire satellite data, air quality data and ERA5 gridded weather data, and forest models to predict PM_{2.5} concentrations. The models provided the identification of the main drivers of PM_{2.5} from individual fires, including fire area and meteorological parameters such as Planetary boundary Layer Height, temperature, wind speed, and relative humidity. The results are important for a better understanding of the impacts of individual fires, both wildfires and hazard reduction burns, on ambient PM_{2.5} concentrations, and the influence of fire and weather conditions. The manuscript is overall well written, and I have a few comments and suggestions:

Introduction: Line 85, I don't know if it's clear to a reader from another country that NSW is a state of Australia, I suggest presenting some information regarding it here.

Introduction: Line 97, "proximity to human populations" does not affect "PM_{2.5} output", as the phrase suggests. Proximity affects the population exposure, I suggest rephrasing.

Introduction: I missed a brief discussion on using random forest models. Are there previous studies using this approach? Why are these models suitable for this work? There are limitations?

Methods - Fire Data, Line 155-167, The choice of buffers and thresholds seemed a bit arbitrary. Was there some sensitivity test? Any justification for choosing these values (distance of 150 km, foliage projective cover > 125, buffer by 2.5 km)?

Methods - PM2.5 Data, Line 191, The PM2.5 definition ("particulate matter < 2.5 μm diameter as micrograms per cubic meter of air")

could have been presented at the beginning of the manuscript.

Methods - PM2.5 Data: The authors do not mention any issues related to data validation or missing data. This happened? If so, what was the strategy to resolve this?

Methods, Line 212: Please check the writing: "modelling to account for account for seasonal"

Results, Figure 3 - I didn't see the unit of measurement on the x-axis of the first graph (PM2.5). Is this $\mu\text{g m}^{-3}$?

Discussion, In general, when analyzing the influence of fire conditions and weather conditions on PM2.5 dispersion, the work does not consider the role of the formation of secondary aerosols from directly emitted precursor gases throughout plume transport. Can secondary PM loading (from smoke precursors) under- or over-estimate the modeled PM2.5? Which predictor variables might have a greater relationship with this effect? It would be nice to include some discussion on this in the manuscript.