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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Connecting smoke plumes to sources using Hazard Mapping System (HMS) smoke and fire location data over North America" *by* Steven J. Brey et al.

Anonymous Referee #2

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Dear Authors,

Thank you for a well-written concise manuscript describing your very interesting experiment. Leveraging the thousands of hours of analyst labor manifest in the NOAA hazard mapping system for science purposes is a very worthy goal. The basic climatological analysis of smoke influences over the US could not readily be performed without these HMS data.

I consider your study worthy of publication, but your results are only semi-quantitative and in some cases potentially subject to large errors, because of weaknesses in the input datasets. You will need to at least include a discussion of these potential errors

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and hopefully some analysis to approximate their magnitude in your final paper.

Land cover is the most egregious example: while the US landscape has not been radically reshaped over the past 25 years, this is not an excuse to use a truncated version of a dataset based on 25-year-old AVHRR data. The Hansen et al. 2000 paper with basic validation results for this dataset is a good place to start, it says “Comparisons of the final product with regional digital land cover maps derived from high-resolution remotely sensed data reveal general agreement, except for apparently poor depictions of temperate pastures within areas of agriculture” (http://www.tandfonline.com/doi/abs/10.1080/014311600210209).

I do not think that your results would see large qualitative changes if you used a more modern dataset such as the North American Land Change Monitoring System (https://landcover.usgs.gov/nalcms.php) but I would expect much better answers from that dataset in areas such as the discrimination of cropland and forested land in the Southeast US (the best dataset for that purpose would be the Cropland Data Layer [https://nassgeodata.gmu.edu/CropScape/]). I am not recommending that you redo your entire analysis with a different land cover dataset (though it might not be that difficult to do so). However, you should include this in your discussion of uncertainties.

You are also using the HMS analyst-generated fire detection data, and for those data there is a published validation: Schroeder et al. IJRS 2008 (http://www.tandfonline.com/doi/abs/10.1080/01431160802235845) . This paper indicates good quality of the fire data, and does not point to significant source of error except to note that like all fire detection systems, small fires are much harder to detect and will be systematically underrepresented in the output products. A recent paper by Hu et al. JGR 2016 (http://onlinelibrary.wiley.com/doi/10.1002/2015JD024448/abstract) describes how these errors manifest as both drastic underdetection of individual fires and as imbalances in fire detection rates by ecosystem. The agreement in the coarse seasonality of North American burning between HMS and GFED data, while encouraging, does not rule out significant biases at the scale of your regional analysis.

You cite the Rolph et al. (2007) paper about the NOAA Smoke Forecasting system, but you need to include the information on uncertainties from that paper in your discussion. That paper found very weak agreement between HMS smoke plumes and smoke transport model results, and while that paper was formulated as a validation of the transport model using HMS plumes, it remains that there is no published validation of the HMS smoke plume extent data, and it is likely to have both large uncertainties as well as some systematic biases due to discrimination of smoke being easier over some areas and seasons relative to others. The Rolph paper is a good place to start to formulate a discussion of how potential errors in the HMS smoke extent data could affect your results.

This last area is one where I will recommend additional analysis. Your current manuscript includes this analysis (page 2): “10% of these plume days are days where ground level PM2.5 is one standard deviation above average summertime concentrations.” That 10% was for Minnesota stations; you cite a figure of 30% for Washington and Oregon stations. This is a good basis for a test of the skill of your method; however, because you are using ground monitors, the additional uncertainty of the vertical profile means that no conclusion can be drawn from these results. I recommend repeating this analysis using AERONET stations in the Western and Eastern US, to determine whether the presence of HMS-diagnosed smoke corresponds with significantly elevated aerosol optical depth relative to the seasonal mean values. This analysis would build confidence in the unvalidated HMS smoke plume extent that is the core of your study.

Good luck with completion of this study, and I look forward to its publication, but I hope to see an expanded discussion of the uncertainties in your analysis that will assist readers in drawing conclusions from these unique comprehensive datasets.

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