

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 #1

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This manuscript primarily details an attribution analysis of the relative contribution of biomass burning smoke originating from the North America region to smoke observed over predefined areas within the continental US. The authors employ NESDIS HMS data and forward trajectory modelling using HYSPLIT to achieve their analysis and results. A smoke transport climatology is presented, which outlines the key smoke producing regions and their influence over themselves and other neighbouring receptor regions.

The narrative is generally well written and logically organised, with clear figures and diagrams – particularly the visual analytics style graphic at the end which provides a

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nice overview of the smoke pathways. The analysis of the smoke climatology follows a reason approach and the results are presented clearly.

In my opinion the main deficiencies are in the input datasets used for the analysis as outlined in the comments below, and a lack of discussion on the potential margin of error in the results. Overall, I think the results are of interest to the community, and would recommend publication after the following comments have been addressed.

1. The HMS is used operationally for smoke forecasting but clarification is needed why this is considered a more suitable choice of dataset for this paper over other established ones like GFED for example, which is compared in the paper and widely used in many studies. Since the HMS product is considerably subjective as it is based on analysts manually adding points for various situations as outlined in section 2.1, the consistency of the product needs to be put into question as a suitable dataset for such analysis. There are also limitations on available years of suitable HMS data. The subjectivity and inconsistency of this operational dataset also limits its usefulness for future analysis.

2. The smoke plume analysis done operationally by the HMS analysts also have a large element of subjectivity and it would be useful to cross check this with another dataset (as was done with the comparison between the HYSPLIT points and GFED). One possibility would be AOD for example, from satellite observations as well as AERONET stations.

3. Units of time (i.e. hours of smoke) are used for the analysis though it would have been better to use derived smoke emissions instead which would take into account land cover characteristics, fuel loading etc. Just using duration alone seems to be a self-imposed limitation when comparing with the amount of smoke observed. Some explanation to better justify this approach would help.

4. The land cover map using data from 1992-93 is considerably old and it is difficult to see why a more updated map wasn't used since there are various newer maps available out there. Unless it could be shown that there weren't significant changes

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in the land cover over the 15 years or more to the analysis years of 2007-14 (quite unlikely), the results involving land cover classification are hampered by using the old map dataset.

Specific comments:

1. P1, line 16 & P11, line 30: ...'HYSPLIT'...
2. P2, line 26 -35: There have been attribution studies conducted in other parts of the world using Lagrangian models with chemistry. Emissions information are available from global inventories including GFED, GFAS, FINN for example, so I would disagree that a modelling approach is unsuitable, because it would clearly be more comprehensive and could include full plume dispersion (compared to trajectories), deposition, attribution of secondary smoke particulates etc.
3. P3, line 3: What is meant to “trigger” a smoke forecast?
4. P3, line 26-29: Please clarify how the accuracy statistics stated here were determined.
5. P3, line 30-31: Repeated phrase - “HYSPLIT points in proportion ... smoke observed”.
6. P4, line 5: On the relationship between HYSPLIT points and smoke quantity – is this purely based on duration assigned by the analyst or is land cover taken into account? Do all HYSPLIT points emit the same amount of smoke? This also raises the question again of why hours of smoke are being used instead of derived emissions.
7. P7, line 6: Is the difference in magnitude actually due to comparison between ‘SPDH’ hours and actual C emissions rather than “varying emission factors for different ecosystems”?
8. P9, line 30-31: Just a comment that a modelling approach would better allow altitude specific analysis to be conducted e.g. at surface level where air quality is of concern to

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the population.

9. P11, line 2: It would be good to provide some explanation on why the trajectories run using the EDAS data are nearly identical and if this is something expected given the higher resolution of the meteorological input.

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