

[Interactive
Comment](#)

Interactive comment on “Development and optimization of a wildfire plume rise model based on remote sensing data inputs – Part 2” by R. Paugam et al.

Anonymous Referee #3

Received and published: 6 July 2015

The paper discusses the problem of determining of the injection height of smoke from large-scale landscape fires, which is both interesting and urgent. The author group includes well-known names in the fire research area, so I started the reading with high hopes. Unfortunately, the paper appeared disappointing in several senses described below. This is highly surprising since I am familiar with many papers of some of the manuscript authors. After spending a lot of time digging in this one, I had to accept that they did not pay enough attention to it.

Major comments

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



The paper is astonishingly difficult to read. This is a huge manuscript (totally over 80 pages of the ACPD format) with long sentences, whose meaning is by no means easy to comprehend. The reader gets this already in the abstract, which is long and chaotic. I had to read it several times to finally decipher the simple message: the paper is extending an unnamed (why unnamed?) model with a mass conservation equation and a new entrainment algorithm and compares it with remote-sensing observations to find the model coefficients. The authors spent two pages to say it. I was particularly baffled by the last sentence, which took 9 lines and contained no message whatsoever. The paper continues this style throughout the whole manuscript, which is chaotic and combines several iterations of the same with absence of vital information. For instance, the “steady-state fire requirement” is discussed at least thrice, whereas the basic information on meteorological data used in the analysis is missing, except for a remark that it came from ECMWF. Poor style and organization of the paper are enough to suggest rewriting the whole manuscript in a reader-friendly way.

The methodology selected by the authors seems to have several large issues. The exercise itself is straightforward: the authors put a few new equations in the existing model, which brought about six new parameters that have to be identified through the fitting to the observation data. The only dataset of sufficient volume is the MINX database derived from MISR retrievals, which has now about 13,000 fire plumes ($\sim 10,000$ if “poor” retrievals are removed). The authors took that one and . . . removed 99.7% of the data following arbitrary and vaguely explained criteria, finally ending up with 38 cases! . . . of which they further removed two cases just because the model failed to meet them. Well, having 36 points and six dimensions of freedom in the system is a red light, which means that the approach is wrong.

The data mis-management had numerous far-reaching implications. The main ones are: (i) evident over-fitting of the model to the data noise since RMSE of the model is much less than the uncertainty of the MISR data themselves, (ii) complete loss of large classes of fires, i.e. the model is not even deemed to work for them, (iii) fantastic steps

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

in the analysis, such as a correlation coefficient made for three dots, (iv) statistical significance neither calculated nor considered, (v) absence of an evaluation dataset turned the whole evaluation section into a hand-waving, (vi) fire plume climatology got no ground: extrapolation of 36 fires to a few tens/hundreds of thousands observed by MODIS is going much too far.

Specific comments.

P.9819, L.18-19. There is always a space for improvement but simply wiping out all existing models and parameterizations as “unsatisfactory” without even formulating the criterion to be satisfied is not an acceptable style.

P.9819, L.18-25. Is that all? I recall about a dozen of papers discussing the impact of injection height, either directly working with it or mentioning the issue in connection with sensitivity studies and modelling efforts. Some of these studies are later even quoted by the authors – and yet not included here. Two pages later, the authors make a U-turn and mark-out three approaches, which should be mentioned here.

P.9819, L.25. What is “improving large scale transport relatively locally”?

P.9819, L.27. Freitas et al, 2007 is a 1D model, not a parameterization.

P.9822, 9837 and in other places. The whole concept of steady-state fire sounds ill-fated to me. None of fires is ever in such condition: changing wind, fuel type and density, evolution of the fire front position and shape, its interaction with landscape topography, etc, all these parameters are never stable. The task of back-tracing of the fire intensity is interesting and challenging but I have a feeling that it should be addressed up-front rather than pushed under-the-carpet by assuming that some fires are more “steady” than others with little reason to do so.

P.9834, L.12-13. Why? If the fire is not related to the plume then why to include it?

P.9834, L.18-22. I did not understand the value of re-extracting the MODIS FRP data instead of using these very data already picked in the MINX dataset. MINX project

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

has this extraction done with much more care than the approach suggested by the authors: 20km around the plume reference point easily picks the fires not associated with the plume but also can miss the needed fires if the plume long enough. This step is worsened the quality of the dataset, not improved it.

P.9837, L.21-26. A very strange move. About 90% of fires are ignited by humans, either on purpose or accidentally. Having removed the agriculture-areas fires, what to do with the deforestation fires? Or with those in urban-rural interface? Conversely, I would suggest that these fires are the ones to be used rather than ignored: MISR makes its observations before but close to midday local-time, i.e. regularly. So, the regular deliberate fires constitute a dataset with “known features”, which can be taken into account and correlated with MISR. Why do the authors think that randomly ignited natural fires (even assuming that they are deciphered out of MINX dataset) are any better?

P.9838, L.5-8. I did not understand the procedure of derivation: the MINX dataset does not have any wind profile. The whole procedure described in this paragraph is unclear, both from scientific and technical points of view. Since the authors removed three quarters of the observations using this criterion, a much better ground and description is needed. In fact, this is one of corner stones of the problems of the paper: the authors formulate a vague criterion, which has severe consequences, but do not make any attempt to justify the choice. In this case, the problem is difficult because the MISR wind and height retrievals are correlated, and careful analysis is needed to understand the data with and without wind correction, to estimate the related uncertainty of the plume height, may be, to filter out some retrievals with evident problems (but not 75%, of course), etc. An important aspect is the quality (or, rather, existence) of the reference point: modelled global meteorological fields form a shaky ground for any sharp action with the data. The representativeness issues may be overwhelming.

P.9838, L22. Another strange criterion. The authors have previously rejected the MINX fire clustering, which uses the actual plume edges to associate the fires, replacing this

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

procedure with a 20km circle, which has no physical ground. Here is another arbitrary requirement claiming 30km area to be free from fires. Plumes cannot interact at such scales and resolution of MODIS and MISR are both an order of magnitude higher, so retrievals of those clusters cannot interact with each other either.

P.9839. This is baffling! Having initially over 13,000 plumes (the MINX project database today), the authors left 39 for the model development and evaluation. Should we really accept that the whole dataset is unusable? The remaining 39 cases are not sufficient for any feasible application because both statistical significance and extrapolation possibility of the results will be negligible.

P.9842, L.15. This must be a joke!! Do the authors really think that correlation can be meaningfully computed for 3 points??

P.9847, L.2. Being within 4.5 m from the MISR observations (error of 20m2) means that there is a huge over-fitting to the noise in the data: declared accuracy of MISR is 200m, if I recall correctly. The independent studies show even larger uncertainties. Whatever is much better than this is nothing but over-fitting. Not surprising though: 36 points for 6 dimensions to catch is a guaranteed over-fitting.

P.9847. And here is another problem: one cannot kick out the data just because the model does not fit them. This is not acceptable.

Section 5.2 has little about the actual performance evaluation. Physical reasoning can be used in discussion but evaluation requires directly comparable quantities: one observed, one predicted. The section does not have them because the authors have disqualified 99.7% of MISR data following arbitrarily picked criteria criticized above. Out of curiosity, how does the scatter-plot for the whole MINX dataset looks like? The authors heavily refer to Sofiev et al works – there such plots are presented, with consideration of “poor” MISR retrievals in one case and having them removed in another.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9815, 2015.