Atmos. Chem. Phys. Discuss., 14, C672–C676, 2014
www.atmos-chem-phys-discuss.net/14/C672/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraining CO_2 emissions from open biomass burning by satellite observations of co-emitted species: a method and its application to wildfires in Siberia" *by* I. B. Konovalov et al.

Anonymous Referee #1

Received and published: 24 March 2014

The paper deals with an interesting problem of determination of CO2 emission from wild-land fires using satellite observations of CO and PM in fire plumes. The first solve two inverse problems determining PM and CO emission using observations of the CO columns and AOD and a dispersion model linking those with emission. Then, a simple rescaling using literature coefficients is applied to obtain CO2. The paper is monumental and covers a wide range of problems, which have to be addressed to reach the declared goal of CO2 emission. It made it difficult but interesting reading.

Despite the large amount of highly sophisticated work performed, the authors have not provided sufficient ground for several elements of their approach, which affected the results and conclusions. I also noticed one evident error in validation of the results. Therefore, I will be able to support the publication only after the weaknesses are properly addressed.

General comments

Firstly, the problem addressed is of very high uncertainty since it involves two inverse problem solutions to estimate the amount of consumed dry biomass from the satellite observations of AOD and CO column, followed by a scaling to CO2 emission using hugely uncertain factors reported in literature. Each of these steps brings errors. A particular problem is that CO and PM constitute minor fractions of fire smoke, whereas CO2 is its major component. Hence, the approach suggested in the paper tries to constrain the major component of the plumes by observing two minor ones. One can never obtain good accuracy with this.

Both CO and PM fractions in smoke refer to poor-combustion conditions and therefore are correlated. Odds are high to have their error correlated too (see detailed comments below). These are bound to dramatically limit the accuracy of the estimates and essentially eliminate the added value of the two inversions, even if the inversions themselves are "perfect".

Secondly, the paper faced the problem reported by practically all related studies, including earlier works of some of the authors: whereas the CO emission factors deduced in bottom-up and lab studies meet the top-down assessments, the results for PM show about a factor of 3 under-estimation in the bottom-up inventories (with root cause probably being the low emission factors). This mystery is not yet resolved, i.e. simultaneous use of CO and PM literature-based emission factors must include some workaround. It is absent in the paper and, sadly but expectedly, the authors got about 3-fold difference between the mean estimates derived from CO and from AOD inversions (table 2). The

authors noticed the problem but waved it out. In particular, they stressed (p.3130) that the uncertainty ranges of these estimates are overlapped. This, however, is not convincing because, firstly, the uncertainty ranges themselves are very poorly known and their tiny overlap can be simply a coincidence. The authors themselves note that their error estimates are rather over- than under-stated, which suggests even higher odds for the difference being formally "statistically significant". Secondly, the overlap, even if exists, refers more to huge uncertainty ranges (up to a factor of 5 and even more) than to indeed closeness of the estimates. Since the authors are interested in absolute CO2 emissions, which are given in table 3 with 3-digit(!) accuracy, a factor of 3 difference between the outcome of the CO- and AOD-based retrievals is hardly acceptable. Once again, the root cause for this, to my mind, is that the literature-based PM emission factors must be used with the highest care until the problem is resolved. I have not seen much criticism on CO emission factors and assume that they (so far) represent consensus among the researchers. The authors discuss the issue (pp.3132-3133) but somehow ended up with a conclusion that this difference is insignificant. In view of the above, I disagree.

Finally, the validation section 4.2 re-uses the same observations as were used for emission optimization. Such re-use of the fitted measurements to evaluate the fitting results is absolutely not acceptable. This is especially true because the authors analyze the very parameters heavily affected by the fitting (mean values, biases, RMSE) and ignore those less influenced (correlation coefficient, for instance). Why the authors didn't withhold half of the data from the fitting? The amount of observations is bound to be more than sufficient for that. In the current form the section 4.2 has no value, except for in-situ comparison, which leaves the study practically without any validation. A rigorous workaround to save the paper would be repeating the fitting with half of data withhold but I understand that it may be too painful. One can consider additional periods with strong fires, may be, in other years, although this is not completely painless either.

Specific comments

Introduction

P.3102, l.21-23 I did not understand the division between wildfires and "other types" and the following lengthy but pointless and confusing wording. Why not simply "emissions of CO2 and other species from wildfires are available from. . ."?

P.3107, l.23. GFAS emission estimation involves direct scaling to GFED totals as part of the procedure, as mentioned in p.3138. This deserves a clearer explanation here too.

P.3113, l.10 I did not understand the reason for such brute-force approach to minimization. Why not to take some standard minimization routine? Just three dimensions of optimization should not be difficult. Problems may arise only if the data scatter is very large resulting in poor convergence. But then the uncertainties of the brute-force minimization will be large too. Explanation is needed here.

P.3115, eq 7. The threshold level notation o is easily mixed with the number 0. The notation should be changed.

P.3119, The eqs.11,12 hold only in case of independent estimates, as the authors stated in p.3120. However, both CO and PM emissions refer to burning quality and type of fire (flaming – smoldering). Since the uncertainties in both CO and PM emission factors partly (largely?) originate from uncertainties in the combustion conditions, they become correlated too. The authors ignore it without even trying to check for error covariances. The statement in p.3120 line 19 goes unsupported and doubtful: there is no self-evident reason to believe that.

P.3121 last line. Factual support is needed. How comes that the CO chemistry and secondary aerosol formation from non-fire sources has no impact on the study outcome? I would accept it for grid cells / days, where / when the fire-induced smoke is dominant. But the authors included all cells with fire contribution > 10%, i.e. up to 90% of the pollutants can be from other sources (eq 7, parameter o). For such cells the

uncertainties in anthropogenic emission are bound to have strong impact.

P.3125, l.14. I did not understand: was MEGAN run online or CHIMERE received precomputed inventory?

P.3129-3130, table 2. Now the problem comes. It is explained above in “General comments”, here I just have to second the statement of p.3130 l.1-5: the combination of CO and PM retrievals using the literature dry-matter-to-CO and –to-PM conversion factors has inherent problems, which questions the value of the whole exercise. A possible way out is to use CO-based emission estimates of CO2 keeping PM-based values as a sensitivity study.

P.3134, l.1-10. This is the major problem. The wrong statement and an evident crude error in the approach are covered by hand-waving (“would hardly help . . . if emission is wrong”). See the general comments above.

P.3138 l 3-10. A very long and self-contradicting sentence collecting several arguments for and against independence of the GFAS and GFED datasets. Please restate.

P.3141, l. 5. This is a confusing sentence. It should clearly separate the CO/PM model-based inversion to emission fields, which are then simply re-scaled to CO2 using literature data. Note that no evaluation is provided for the last step.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3099, 2014.