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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of optimized mixing heights on simulated regional atmospheric transport of CO_2" *by* R. Kretschmer et al.

Anonymous Referee #2

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This paper addresses an important problem atmospheric scientists dealing with inverse flux estimation problems are confronted with, i.e. the influence of errors in simulated boundary layer mixing heights on those estimates. This is a problem well recognized by the community but not yet sufficiently addressed in a rigorous manner as done in this manuscript. This publication is thus a very welcome contribution and it is timely given the advances in the setup of regional carbon cycle observation systems such as ICOS in Europe.

The paper is generally well written and the methods are adequate and rigorous to the extent I was able to verify. Although the authors have worked on the same topic previously, this paper takes the analysis further by applying the method to a range of observational data sets (ground based measurements in Cabauw and Heidelberg

and aircraft profiles from the IMECC campaigns) which provides further challenges to the method as compared to the setup with synthetic data in the previous study. These additional challenges are discussed in detail. I recommend publication after addressing a few general remarks and a number of minor issues.

General remarks

Although the paper presents an interesting method to generate improved mixing height (MH) estimates as input for transport simulations, it has some important drawbacks that are not sufficiently acknowledged. Although the transport models may have vertically misplaced MHs, those MHs are internally consistent with the simulated vertical profiles of wind, temperature, humidity and other variables. Artificially changing the MH without adjusting other meteorological variables (notably the wind profile) will necessarily lead to inconsistencies, and such inconsistencies are likely contributing to the problems described in Section 4.2 where the STILT simulations based on MYJ versus YSU PBL parameterizations diverge even more after adjusting the MHs to more or less the same heights. Artificially displacing the MH may, for example, result in a situation where air parcels previously located in the free troposphere in a regime with strong wind speeds due to reduced drag, are suddenly located inside the PBL and thus able to interact with the surface. Signals from distant sources that would not reach a surface site within a short time given the low wind speeds in the PBL, could now reach the site more quickly due to fast transport aloft which is no longer decoupled from the PBL as it should.

Modern data assimilation methods such as 4D-VAR or Ensemble Kalman filters could be used to incorporate radiosonde observations for improved MH estimates similar to the method presented here, but with the advantage of not disturbing the internal consistency of the model (or at least much less). The above mentioned deficiencies of the method and the alternative of data assimilation should be better highlighted in the paper.

Nevertheless, the presented method is valuable and carefully implemented in this

study, and it will be interesting to compare the results obtained with this method with those from (more expensive) data assimilation in the future.

Apart from this general remark that can be addressed easily e.g. in the discussions section, I only have a number of minor comments, mostly on spelling errors and typos.

Minor points:

P4629, L7: What are “continental point observations”?

Introduction: It should be stated more clearly that while the previous study by Kretschmer et al. (2013) was based on synthetic data, this study investigates the effect of MH optimization on real observations.

P4632, L3: “and compare” -> “and compared”

P4632, L5-L10: Should be reformulated. First the problem of prior flux uncertainties is mentioned while the next sentence explains that simulations with two different PBL parameterizations were used. What is the connection between these two?

P4634, L8: “Cabauw to one” ->” Cabauw one”

P4635, L24: “providing for an” -> “providing an”

P4638, L10: “.. physically consistent, for our purposes..” -> “physically consistent. For our purposes”

P4638, L13 and L15: “(Seidel et al., 2012)” -> “Seidel et al. (2012)”

P4639, L2: Here and other places kriging is in lower cases, while at other places Kriging is in upper cases. Please unify throughout the manuscript.

P4639, L9: Why do you say “KED solves for the weights ..”? Isn’t this a general feature of Kriging not specific to KED?

P4639, L9: “such that interpolation” -> “such that the interpolation”

P4639, L17: Shouldn’t it be “linear function” rather than “linear combination”?

P4639, L20: Why are the coefficients a and b without asterisks here?

P4639, L20: “by the ordinary least squares” -> “by ordinary least squares”. You should probably mention here that this is done by evaluating the simulated MHs at observation points.

P4639, L20: “and thAn in a second iteration” -> “and thEn, in a second iteration,“

P4640, L15: “in distances classes” -> “in distance classes”

P4641, L6: Why did you perform a weighted linear regression here? To get rid of the biases? Should be explained more clearly.

P4641, L12-13: I didn’t understand this sentence. What do you mean by “true variability”?

P4641, L27: “edinburgh” -> “Edinburgh”

P4642, L1: “eulerian” -> “Eulerian”

P4642, Eq 8: In my view it should be f(xi,yi,tm | xr,tr), since the right hand term is a scalar product of two functions at the locations (xi,yi,tm) not at (xr,tr)

P4643, L7: “effect MH errors” -> “effect of MH errors”

P4643, L8: What do you mean by “side effects”? Please explain.

P4643, L10: “extend” -> “extent”

P4644, L7: To my knowledge EDGAR does not provide any time factors. Please provide more details.

P4645, L5: “to isolate impact” -> “to isolate the impact”

P4645, L14: Explain how CO loss by reaction with OH is simulated. With prescribed 3D/4D OH field or just a constant OH value? What about CO production from VOCs?

P4647, L13: Looking at table 2 it seems that the bias increases rather than decreases.

P4647, L14: “Random errors slightly decrease”. Relative to what? Where or how do I see that?

P4647, L25: “The result were” -> “The results were”

P4648, L8: I would even say that CO2 is not a suitable gas to diagnose MHs due to its fluxes being both positive and negative. As a result, CO2 often shows no vertical gradient at certain times of the day around the transition from positive to negative fluxes while other gases do.

P4648, L23: “and than selected” -> “and then selected”

P4650, L28: Isn’t the receptor located “above” the MH rather than “below”?

P4651, Section 3.3.2. Another potentially complicating aspect in a city is the urban heat island effect which alters the diurnal cycle of meteorological variables in a way not represented in the model.

P4652, L3: “to some extend” -> “to some extent”

P4654, L5-8: How do you deal with potential biases in the MACC CO data? My own experience with this data set is that it can have very large biases (though larger in winter than summer).

P4655, L14: Typo, “tweo” P4656, L5: “in CO2” -> “on CO2

P4656, L6: “correct” -> “correctly”

P4657, L4: Probably you mean “key” rather than “critical”

P4657, L17: “due to errors MH” -> “due to errors in MH”

P4658, L10: “build” -> “built”

P4658, L14-L15: Something is wrong with this sentence.

P4659, L22: “the reasons this effect “ -> “the reasons for this effect”

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4627, 2014.