Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-577-RC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric CO_2 inversions at the mesoscale using data driven prior uncertainties. Part 1: Methodology and system evaluation" *by* Panagiotis Kountouris et al.

Anonymous Referee #2

Received and published: 25 October 2016

This paper describes calculations of CO2 fluxes for Europe based on inversion from synthetic concentrations. It serves as preparation of a second part where observed concentrations are used. The title announces that "data driven prior uncertainties" will be used. But there is a substantial issue with this. It is important to note that the paper has a precursor in Kountouris et al. (2015) where prior flux errors are estimated based on comparison of model results and real (eddy correlation) flux observations. There, remarkably small flux error correlation lengths of up to 40 km are found (see page 6 line 7 in the present paper). When this is imposed on the prior flux error matrix, this leads to "exceptionally small" (line 9) estimates of the error in the continental integrated prior flux. Apparently, this constitutes a problem: in the end, the authors decide to

Printer-friendly version

Discussion paper

use a much larger correlation length (of 566 km on average, see page 12 lines 3-7), which is based on an investigation of model-model residuals (page 11 lines 19-21, and Abstract). Unfortunately, this means that the "data driven prior uncertainties" claim in the title no longer holds. This also undermines the innovative pretention expressed in the title An interesting innovation is the use of an extra "bias" term in the flux, consisting of a "known" spatial flux field multiplied with an unknown time series to be determined by optimal fitting. This avoids the artificial inflation of errors to obtain an acceptable result. Maybe, more could be said about its proposed physical interpretation (which is now indicated very briefly on page 13 in lines 11-12).

In conclusion, the paper represents little real progress (that is not to say that a lot of technical good work was executed to arrive at this stage), in particular with the synthetic inversion results contradicting the title.

Minor comments Page 2, line 5: "it is used in such a way" → "is used" Page 4, lines 12-16: does this involve nonlinearity ? comment on this. Page 5, line 8: delete "zone" ; "later"→ "latter" Page 5, line 10: "with" → "for distances up to" ? Page 5, lines 16-19: be more specific Page 5, lines 26-end: this is somewhat difficult to follow. Page 6, line 3: "or" → "respectively" ? Page 6, line 16: "for" → "integrated over" Page 6, line 17: "Although is" → "Although it is". Page 7, line 7: "term referred to a bias term" → "term to reflect the bias" ? Page 7, line 9: "between" : another word is needed here. Page 7, line 27: "conclusions are following in Section 4": these are presently in Section 5. Page 8, line 17: "cini is the initial concentration": is this correct ? With f = 0, cmod would still evolve in time. Page 9, line 6: "constrain" → "constraint" Page 11, lines 1-4: the wording is a bit confused. Page 11, equation 6: apparently not referred to and of unknown use. Page 11, lines 8-12: this is an errant block, it should come later. Page 11, line 12: delete "et al." Page 11, lines 16-21: there is a difference in method here: Kountouris et al. (2015) used model-data instead of model-model comparison. And the resulting correlation lengths are also very different, which should be indicated. Page 11, line 23: "ensuring similarity": same remark. Page 12, lines

12-13: Not sure if the acronyms “B1” and “S1” would be the best choice, one might think of more telling names. Page 12, line 28: “and unit variance”: this pertains not to the adjustable term but to the p-coefficients. Page 13, line 4: “which they a-priori have, a” → “which a priori have a” Page 13, line 6: “derived” → “expressed” (nothing is said yet about how values are derived) General about section 2.2.2: It remains unclear in the paper how posterior errors and covariances are derived. Page 13, line 21: “use a different biosphere model”: add eventually references to literature where the same is done, like in the previous sentences. Page 14, line 3: “table 2”: and figure 1. Page 14, line 24: “DoI”: explain that this means domain of interest. Section 2.3: a separate subsection may be superfluous, instead the content could be built in within the results section. Page 15, line 9 and 10: Unclear sentence. “a-priori” in line 9 and “optimized” in line 10 seem to contradict each other. Page 17, line 6: “central Europe”: also south Scandinavia Page 17, line 10: “measures” → “measured”. Page 17, lines 11-12: is this shown anywhere in the paper ? Page 17, line 24: “found” → “was found”. Page 18, line 24: inversion performance: for which of the two inversions ? see also question at figure 9. Page 19, line 10: “Figure”: Figure 10. Page 19, line 28: “65 %”, “64 %”: where is this stated ? Page 20, line 9: “nearly continuous”→ “nearly monotonous” ? Page 23, line 14: “years”: reciprocal years. Figure 4: “R0”, “R1”: wrong acronyms. “ration” → “ratio”. With which time base were the results obtained ? Figure 5: “gCy-1m-2 “: usually this is written as “gCm-2yr-1 “. Figure 9: colors will be often indiscernible in practice (maybe no problem !); why is one arrow seen when there are two ways to calculate a posterior ?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-577, 2016.

Printer-friendly version

Discussion paper