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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lower-tropospheric CO_2 from near-infrared ACOS-GOSAT observations" *by* Susan S. Kulawik et al.

Anonymous Referee #2

Received and published: 4 November 2016

The paper addresses a very important problem, namely increasing the sensitivity of satellite CO2 observations to surface fluxes, by attempting to separate the retrieved CO2 into lower troposphere (LMT) and above (U). The study is well-conceived and executed, being theoretically sound and testing results extensively with a range of validation data. The results seem to show compelling improvements relative to prior knowledge and to XCO2, particularly in comparisons to LMT.

Major issues: I have one important question: to what extent do the results depend on 'bias correction'? The correction quoted in Appendix A is surprisingly large and variable for land, and its variability for ocean, while smaller, is considerable. Given my concern on this point I would like to see some of the comparisons to validation data (e.g. Figs 5, 7, and/or 8) shown for results without the GOSAT bias correction.

Further, if I read Sec 4.3 and Appendix A correctly, it seems that 'bias corrected' U is derived solely from bias corrected LMT and XCO2, i.e. does not depend on the satellite's radiance measurements directly. If true this should be made explicit.

On another matter, I found the arguments very difficult to follow in many places. The terminology is not always clearly defined, and the definitions are hard to find. Generally, error and related terms need to be better defined, and their definitions better flagged; I am very confused by the table entries: e.g. in Table 3, 'predicted error', 'GOSAT standard deviation', 'true variability'; are these directly comparable quantities? In Table 2, I am not sure what 'bias' means here; 'co-location bias', 'prior bias', and 'GOSAT bias' are not defined and their precise meaning is unclear

Also, careful proofreading would make the paper more readable.

Assuming my concern about 'bias correction' can be allayed, I recommend publication after the other issues raised have been addressed.

Specific items:

Line 51: Connor et al has been published in AMT and the reference should be updated

Line 75: unclear language: 'fluxes versus for XCO2'

Line 130: 'obspack' is used before it is defined

Line 250: language: 'chosen constrain'

Line 267: the use of 'y' as a part of the state vector is confusing to those familiar with the symbols in Rodgers, Rodgers & Connor, 2003 etc., where 'y' is the measurement vector; Rodgers & Connor use 'u' instead.

Line 416: 'CCGCRV' is used before it is defined

Line 458: there is no section 5.6

Line 463: define 'WRF-STILT'

Line 489: ‘()’ what is this?

Line 501: ‘the a’: ‘a’ is a mathematical symbol but as used it looks like an English word

Line 507: ‘be used’?

Line 598 et al: ’CAR’ and many subsequent abbreviations for observing stations are never defined; sometimes they are capitalized and sometimes lower case (or do the 2 cases mean something different?); please list the stations with names and abbreviations

Line 659: ‘variability for ESRL ocean’ is not clear; variability of what?

Line 778: ‘factor is’ what is it?

Line 793-5: sounds like there is no doubt that bias correction changes the correlation; why discuss it?

Line 843: why a mismatch in airmass for these but not others?

Sect 5.4: I find the discussion of emission ratios opaque: relative DoFs? vertical sensitivity? why would variations in CO and CO2 ’be either zero or solely from locally influenced fires’?

Line 926: ‘CO2 and’ and what?

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