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Geoscientific Model Development Discussions
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Interactive comment on "The CarbonTracker Data Assimilation Shell (CTDAS) v1.0: implementation and global carbon balance 2001–2015" *by* Ingrid T. van der Laan-Luijkx et al.

Anonymous Referee #2

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General comments

The authors present a new design of the data assimilation code named "CarbonTracker". The paper is interesting at first glance and well written, but the scientific content is rather shallow. The editor has a much better view than me about what can be published in GMD, but as a reader I feel rather frustrated. The core of the paper is structured into four parts. The first one is like "CarbonTracker meets Python and adopts classes and modularity". This change certainly represented a large amount of thinking and work, but the use of Python which is described is quite basic and common. My codes are mostly in Python and are structured the same way, even for the documen-

tation, and my colleagues do roughly the same. Again, this is not a judgement about the technical value of the work, or about the involvement of the developers, but rather a judgement about its meaning for external readers. The second part is about recent results obtained for CO_2 and describes recent updates of the configuration. There are interesting parts (in particular the comparison between the successive product releases) but that may not go far enough. I could not find any information about the way the error statistics are cycled from one window to the next, or about the way temporal correlations are handled within an assimilation cycle (actually the last lines of the paper suggest that they are not handled at all, which is surprising), or about the global prior error budget, or about the ensemble size in the gridded state vector configuration (given the curse of dimensionality), etc. Some of the results also seem to have already appeared in Le Quéré et al. (2016). Posterior errors are shown but are immediately discredited (p. 12, l. 21-22), which suggests a major gap in the new shell. The third part is an overview of applications: it shows that the authors have nicely structured a community, but is there anything scientific that the reader should take from it? The fourth part is a short list of planned developments. I would recommend that the authors put more scientific material in their paper before it is published in GMD.

Detailed comments

- p. 1, l. 11,"We show…": what is the difference with the CTE material and associated conclusions displayed in Le Quéré et al. (2016)? How robust is this result (can the atmospheric data properly separate between land and ocean fluxes?)?
- p. 1, l. 12-13: is this really news (that forests are the dominant sink in Europe and that drought reduces it)?
- p. 1, l. 13: do the authors suggest that the historical version was not versatile and could not allow such applications? I know several large Fortran codes that

still had many applications despite horrible coding.

- p. 2, l. 11: computational time has nothing to do with the number of code lines.
- p. 3, l. 15 and 17: “error” is missing before “covariance” at both places.
- p. 3, l. 19: if the system is robust for Europe and Northern America only, why are results for other parts of the globe shown (e.g. Fig. 8)?
- p. 4, l. 3: from a quick check of the ECMWF web site, the situation of OOPS at ECMWF seems to be less advanced than what is suggested here (http://www.ecmwf.int/sites/default/files/elibrary/2017/17179-strategy-data-assimilation.pdf).
- p. 10, l. 10: "Olson ecoregion" is not a standard expression.
- p. 10, l. 34: I could not find the 2.12 factor in Prather et al. (2012). The reference may be wrong.
- p. 11, l. 1: “well” should be quantified. What is the scientific meaning of the error bars in the figure if they are not properly computed (also for Fig. 7)?
- p. 11, l. 1-2: the authors forget the role of transport model errors and the assumptions behind the NOAA estimates.
- p. 12, l.22-23: how can this feature be an advantage? I would think otherwise.
- p. 12, l. 23: comparing a range with a standard deviation is not trivial. How is this done? Is the range assumed to represent 4 sigmas, 6 sigmas, . . .?
- p. 12, l. 24: this statement is valid only under the requirement that the realizations are of the same quality level.

- p. 13, l. 29: this result does not seem consistent with the plan to further shorten the assimilation window (p. 15, l.9).
- p. 13, l. 31: the statement is intriguing and I could not find its origin in the Babenhauserheide et al paper. In its section 5.1.1, the latter paper discusses rejection and error assignment issues rather than optimization methods per se.
- p. 13, l. 33: the statement seems to be too trivial for a "demonstration". "Illustrate" would be better, or am I missing something?
- p. 14, l. 30: why did the use of the new Python shell need to be demonstrated in the first place?

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