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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Linking global terrestrial CO_2 fluxes and environmental drivers using OCO-2 and a geostatistical inverse model" *by* Zichong Chen et al.

Anonymous Referee #1

Received and published: 26 May 2020

The authors have developed a geostatistical inverse method to interpret satellite observations of carbon dioxide (CO2) collected by the NASA Orbiting Carbon Observatory collected during 2016. As far as this reviewer can see the study is scientifically sound but describes only an incremental improvement to the method and does not lead to any new scientific insight. Unfortunately, the authors' choice of OCO-2 data raises more questions than it answers.

Major comments

The environmental drivers for ecosystems located at mid/high and tropical ecosystems are unsurprising. Perhaps that's the point. I wasn't sure. PAR is by definition photo-

synthetic active radiation so its ability to describe large-scale CO2 fluxes isn't anything new, particularly over one year that is dominated by the seasonal cycle. Any insights from using the diffuse and direct components of PAR? Similarly, temperature and precipitation roles in the tropics are nothing new. However, I am surprised that precipitation is such a useful driver over the tropics where complex basin-scale hydrologic controls are at play. In other words, where it rains is not necessary where the water ends up.

The authors have gone some way to 'fess up that the geostatistical inverse method uses prior information for which I commend them. It might not be defined in the same way as the classical Bayesian approach but nonetheless it uses prior information. Otherwise, inferring fluxes for 10ˆ6 grid boxes using 10ˆ5 measurements is an ill-posed problem. The method uses environment driver data with uncertainties that are difficult to quantify (see comment below about estimated posterior uncertainties).

It would be useful to reiterate to the reader the benefit of the geostatistical inverse method over more traditional methods. Certainly, it provides an alternative perspective but I have seen no evidence to suggest it is better or worse.

Line 216: This reader is surprised that OCO-2 data are not sensitive to biomass burning emissions, particularly during the El Nino period. The manuscript would benefit from having more explanation on this point.

Why are correlations higher when environmental drivers are passed through the atmospheric model. Figure 3 doesn't cut it - the color scale is almost binary as currently defined. Using the square of the correlation might be a better way to illustrate these calculations.

Line 263: widespread and prolonged drought conditions, together with large-scale land-use change, is a more accurate description of what's going on over these regions.

Paragraph 298: comparison of the reported work and other groups is weak. Not many people have used v9 of OCO-2 data so I think it would be useful for the readership to provide a more detailed assessment of results compared with past estimates using v7 data. The comparison between the model and independent measurements is minimal (in supplementary information). The uncertainties associated with the posterior estimates are unrealistically small. The classical Bayesian inversion as typically employed underestimates posterior uncertainties so certainly the uncertainties estimates reported with the geostatistical method are grossly underestimated. This reviewer is left wondering why this might be so and how a possible explanatory imbalance between prior and observation uncertainties would influence model selection and the analysis that follows.

Sure, the tropical flux estimates are important to discuss. However, are the reviewers are in a position to dismiss the results over tropical North Africa without further explanation. Why did they find themselves in terms of environmental drivers? Surely, their results over tropical Africa aren't exclusively determined by measurements collected over tropical Africa? Do they find that seasonal differences in measurement over tropical Africa lead to a bias in the flux? Answers to these questions would represent a useful contribution to the field.

There is almost nothing in the manuscript about the large differences between other geographical areas where we would expect much better agreement, e.g temperature North America, Europe, Eurasian temperate. Without a more comprehensive evaluation of the fluxes it is difficult to know whether the method is at fault or the data they have used. This manuscript would benefit greatly from a better evaluation of the posterior fluxes.

Minor comments

Line 59: it would be fairer that Chevallier 2018 argues not suggests.

For context, it would be useful for the reader to understand that 2016 was an El Nino year.

Line 91: how did the authors decide that four months was a sufficient spin-up period?

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