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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Global distribution of methane emissions, emission trends, and OH concentrations and trends inferred from an inversion of GOSAT satellite data for 2010–2015" *by* Joannes D. Maasakkers et al.

Anonymous Referee #1

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The manuscript "Global distribution of methane emissions, emission trends, and OH concentrations and trends inferred from an inversion of GOSAT satellite data for 2010-2015" from Maasakkers et al., submitted for publication in Atmos. Chem. Phys., describes the application of a methane emission and OH concentration inverse modelling scheme to six years of GOSAT column-averaged methane data to obtain information on methane emissions, OH concentrations and their trends. They explain the method including error characterization, the conducted sensitivity experiments and discuss the results, which are compared with published results and emission data bases. The pa-

per contains new results and covers aspects relevant for Atmos. Chem. Phys. The paper is very well written and I recommend publication after the mostly minor aspects listed below have been considered by the authors.

General comments

Several publications exist using GOSAT data in combination with inverse modelling to improve our knowledge of methane emissions. According to my knowledge, this is the first publication aiming at simultaneously inferring information on OH (which is the largest methane sink) together with methane emission information from satellite data.

Despite many efforts in recent year to obtain information on regional / country scale methane emissions and related trends from satellite data there are still many open questions (e.g., w.r.t. the reason or the reasons of the renewed methane growths after 2007) and the published results and conclusions are often apparently in contradiction. For example, in the abstract it is written: "The observed 2010-2015 growth in atmospheric methane is attributed mostly to an increase in emissions from India . . ." whereas the recent Nature communications article "Atmospheric observations show accurate reporting and little growth in India's methane emissions" (Ganesan et al., 2017, cited by the authors, and also using GOSAT satellite data) suggest that this seems not to be the case. Maasakkers et al. state (page 2, line 13 and following) that "Here we use global 2010-2015 methane observations from the GOSAT satellite in an analytical inverse analysis with full error characterization to better quantify methane sources and interpret the recent trend, including changes in both methane emissions and OH concentrations." The authors claim to use "full error characterization" and this is a bold statement. I recommend to formulate this less drastically. One aspect not "fully covered" are biases of the satellite data (e.g., unknown spatio-temporal error correlations). To assess the impact of biases is difficult but using an ensemble of satellite data should help to identify robust features and to obtain more robust conclusions. Here the authors used one GOSAT data product, namely the University of Leicester GHG-CCI v7 proxy product, but more products are available (from GHG-CCI and other projects) and it is unclear to what extent the findings are related to the specific product used for this study. I am aware that using an ensemble of products would significantly increase the computer time and related resources as needs for data analysis etc. but I recommend to make use of the available ensemble of satellite products for future studies.

Specific comments

Page 3, line 20: The statement related to GOSAT observations (no drift and no degradation of data quality) citing Kuze et al., 2016, refers to the interferograms/spectra but not the derived methane product. Please modify that sentence to make this clear.

Page 6, line 24: Including a polynomial of latitude has (according to my knowledge) first been used by Bergamaschi et al. when applying methane inverse modelling to SCIAMACHY satellite data. I recommend to cite the relevant paper here.

Page 7, line 15: Sentence "SO is taken to be diagonal for lack of better information but the general effect of error correlation in the observations is accounted for in the inversion by a regularization factor". See above my "General comment". The "general effect" can be very different from the "specific effect" of biases of a given satellite product. The approach used may help or not to deal with this aspect. In the best case, it accounts for some aspects of error correlations but to what extent this is true is unclear.

Page 7, Eq. (2): Is the use of the gamma factor related to this specific inversion method or have similar methods to avoid overfitting also used by others (in their peer-reviewed publications)? If yes are the "scaling factors" similar? Reducing the weight of the observational terms by a factor of 20 seems very large. Would be good if additional information on this aspect could be added.

Page 13, line 23: Statement "but this does not exclude the significant increase that we find here". Why not? Please provide justification for this statement.

Page 29, Fig. 3: Middle right panel: What is the reason that the difference has a trend

only in 2014 and 2015? Please explain.

Page 29, Fig. 3: Bottom panels: The posterior slope is worse (larger deviation from 1.0). Is this a significant finding (or is the slope error too large)?

Page 35, caption Fig. 10: "... in response to the increasing concentration ...". Which concentration? OH? Please add.

Page 35, Fig. 10: The shown growth rates appear to differ significantly from the growth rates published by NOAA (see https://www.esrl.noaa.gov/gmd/ccgg/trends_ch4/#global_growth). Can you comment on this?

Technical comments

Page 4, line 8: add space after "inventory".

Page 6, line 3: add space after "fields".

Page 11, line 3: add space after ".org".

Page 24, reference Sheng et al., 2018. The paper is published in ACP. Please update the reference.

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