Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-929-AC1, 2018

Atmospheric Chemistry and Physics Discussions

Open Access

ACPD

Interactive comment

Interactive comment on "Recent increases in the atmospheric growth rate and emissions of HFC-23 (CHF_3) and the link to HCFC-22 ($CHClF_2$) production" *by* Peter G. Simmonds et al.

Peter G. Simmonds et al.

petergsimmonds@aol.com

Received and published: 19 January 2018

We thank the Referee #1 for their time and effort in evaluating this manuscript and for their suggestions for improvements. Our responses to the points made by the reviewer are addressed on the following pages.

Replies to Referee 1

1 Overview:

Review of "Recent increases in the growth rate and emissions of HFC-23 (CHF3) and the link to HCFC-22 (CHClF2) production" by Simmonds et al. Simmonds et al.

Printer-friendly version

Discussion paper

present an estimate of the HFC-23 and HCFC-22 emissions using observations from the AGAGE network. They perform two Bayesian inversions: (1) in a global 12-box model and (2) a regional inversion for Europe using FLEXPART. Overall, the manuscript reaches scientifically interesting conclusions. However, many of the details necessary to follow the conclusions are not included. Specifically, most of the details of the forward and inverse modelling are omitted. This makes it difficult to interpret the results. Additionally, some of the conclusions seem overly speculative. I would suggest major revisions for the manuscript.

2 Major comments:

2.1 Structure of the manuscript and description of the modelling The authors spend most of the methods section explaining the measurement protocols and calibration methods. Are the measurements used here fundamentally different from the previous work using the same AGAGE measurements? It seems that this paper is, at it's core, an inverse modelling paper because the novel analysis is related to the derived emissions. However, the forward and inverse modelling is not well described. The authors spend a single paragraph explaining the 12-box model. Is the box-model including a seasonal cycle or annual concentrations? If there is not a seasonal cycle, how are the authors removing the seasonal cycle from the observations? The authors mention that they use an inter-annually repeating OH but some recent work (Rigby et al., PNAS, 2017; Turner et al., PNAS, 2017) has shown variations in OH, would this be important for the modelling here?

Reply. We have added further detail to Section 2.6 to provide further details on the inverse method. Similarly to the reviewer's concerns regarding the length of the description of the measurements, we opted to keep this section brief, because the methods have been described in detail elsewhere (Rigby et al., 2011; Rigby et al., 2013; Rigby et al; 2014). Regarding the influence of OH variations on the inversion, small differences were found in the derived emissions of HCFC-22 (which we now show in the supplement), whereas, owing to its very long lifetime, negligible differences were found for HFC-23. For HCFC-22, the magnitude of the difference when Rigby et al (2017) OH was used versus a constant OH concentration were much smaller than the derived uncertainty (Supplementary Information).

Questions related to the 12-box model inversion: How is the inversion done? What is the state vector? Is it annual global emissions or the emissions for each of the latitudinal boxes? Presumably the distributions are Gaussian? Are there off-diagonal covariances? How are the model-data uncertainties specified (ie., what is the model error)? Some of these details could go in a supplement, but they should be described somewhere.

Reply. We have added this information to Section 2.6.

Similarly, the authors present a second inverse analysis within the results section (Section 3.2). The modelling is explained in a single paragraph, yet this is a very complicated inverse model they present. In the paragraph that follows, the authors state "Considerable differences between the two inversions with different a priori emission distributions occurred on the country scale" but do not explain the different inversions, priors, etc. Supplemental Section 3 provides a good explanation of the inversion framework from Brunner and Henne and I would recommend incorporating some of that text in the manuscript, it would be very useful for the reader.

Reply. We agree with the referee that more details of the regional inversion could have been included in the main text. However, it was the intention to keep the text as concise as possible and the applied inversion tool has frequently been used in previous studies. Nevertheless, we follow the suggestion of the referee and included the details on the inversion method in a new section (3.7) in the main text, while keeping the more comprehensive discussion of the inversion results in the supplementing material.

2.2 Differences between this work and Miller et al., (2010)? The authors note that "Miller et al., (2010) calculated global emissions of HCFC-23 using the same AGAGE 12-box model as used here, but with a different Bayesian inverse modelling framework."

(Section 3.2). However, the authors do not seem to explain the differences between the inverse modelling frameworks. This seems like a crucial detail because the HFC-23 emissions from Miller et al. are outside the errorbars presented here (Figure 3). This comment seems to go back to my previous comment on the structure of the manuscript. The authors spent a lot of time explaining the measurements but, from my reading, it doesn't seem like the measurements are what give them different emissions.

Reply. It is important to note that the Miller et al (2010) emissions shown in Figure 3 are bottom-up estimates. We had omitted to specify this in the figure caption and have now added this in the revised version. Small differences are observed between the top-down estimates from Miller et al (2010) and those presented here. This is because: a) we include a new firn dataset in our inversion; b) Miller et al. (2010) constrained the inversion to absolute emissions from a time-varying prior, whereas we used a much simpler prior constraint that the assumed emissions growth rate would not vary by more than ±20% of the maximum bottom-up estimate, between any two years.

2.3 Speculative statements

There are a number of statements that seem overly speculative and it's not clear that they are supported by the analysis. Here I list two rather provocative statements:

Statement (Section 3.2): "We also note that this minimum occurred during the global financial crisis of 2007-2009 and in fact HFC-23 emissions mirror global GDP growth rates for the years before and after 2009https://data.worldbank.or/indicator.NY.GDP.MKTP.CD). We can only speculate that this may have reduced the overall demand for PTFE, thereby impacting global HCFC-22 production and the co-produced HFC-23."

Reply. This statement has been removed.

Statement (Conclusions): "With the support of the Chinese Government, 13 new destruction facilities at 15 HCFC-22 production lines not covered by CDM were started in

2014 (UNEP, (2017a). The timing of these new initiatives is consistent with the most recent reduction (2015-2016) of global HFC-23 emissions, although we cannot confirm a direct link."

Reply. Respectfully, we would wish to retain this statement of fact and have slightly edited the text as follows-: In 2014, with the support of the Chinese Government, 13 new destruction facilities at 15 HCFC-22 production lines not covered by CDM were started (UNEP, (2017a). The time frame of these new initiatives is consistent with the most recent reduction (2015-2016) of global HFC-23 emissions.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-929, 2017.