Reviews to acp-2017-1041

Anonymous

The authors accounted some of the remarks made by the reviewers and marginally improved the presentation of there results, but the use and description of the so-called analytical model (formulation would be more correct) remain highly problematic due to the lack of pedagogy in presenting it, to missing information on the definition of variables and still problematic unit homogeneity (see details below).

Removing all mention to this analytical formulation (in its current state) would make the manuscript much clearer and suitable for publication after relatively minor revisions. But the insistence by the authors to relate all comments on the results to equation 2 is misleading (especially as the equation by itself is not clear).

The authors are proposed to re-think their manuscript without the analytical formulation but keep the same numerical protocol. As currently described, the numerical protocol quite convincingly proves that the IPD is not a sufficient tool to infer fully data-driven conclusions on Arctic emission trends, even though there is some hope that a thorough model-enhanced use of the IPD might give some first diagnostic on emission trends. It would fully address the objectives that the authors stated in their replies to the referees. Such an amended manuscript might be suitable in ACP if correct presentation is achieved as a "Peer-reviewed comments" paper to comment Dlugockencky papers. The limited scientific content would not justify a research article paper in that case.

A second case using the analytical formulation would make a suitable contribution as a research paper if presented and used properly. However, such an approach (that seems the preferred one by the authors) needs major improvements as detailed below. A correct development of the analytical formulation would make the numerical protocol and discussion on it much more relevant than the artificial comments in the current manuscript.

1 The analytical formulation of the IPD

As mentioned above and in the first round of reviews, the analytical formulation seems flawed, or even incorrect (or I did not understand some points, which would at least prove some clear lack of pedagogy).

Here are the main elements posing issue in the current manuscript:

1. as the formulation plays a pivotal role in the manuscript, the authors are recommended to fully develop the equations in a dedicated method section, including the details on extra steps in the annex.

2. the so-called "full IPD" is never explicitly defined; is it the difference of concentrations between the two poles as points or the difference of average concentrations over polar regions (as defined with the 53^o limit?)

3. why making a difference between IPD and IPD_{Δ}? the same formulation could be develop for IPD_{Δ}; with the same implications and conclusions

4. the authors lightly tried to address the unit homogeneity issue with little success; B is described as a source (in kg/s) and then integrated over r (a latitude? a distance to something?); thus the term $\int_r^0 B(t-\tau_N(r'),r')dr'$ is in kg.s^{-1}.latitude and then added to L that is in kg/s, which is incorrect; the mass to mole conversion is barely sufficient and just hides other issues

5. it is not clear at all what the variable B represents; it is first called an "inter-polar source", homogeneous with an emission term, but it is pretty clear from the rest that it includes some transport and "transport history" as stated later in the manuscript; moreover, B is defined as a source at latitude r but is then evaluated over all latitudes; it would be much more pedagogic to split this term between source terms (surface emissions) and atmospheric transport terms from a source at point r to Arctic and Antarctica respectively

6. L is lightly called a source term, but like the term B it implicitly included all transport from the Arctic to the Arctic integrated from the past to present day, which would be more pedagogic to explicitly say

7. the transport from the Arctic source to Antarctica is neglected but this assumption does not help the argument, nor is correct as eventually Arctic emissions are globally mixed (the contrary would mean that IPD would diverge pretty quickly...)

8. what does "B arrives at both poles simultaneously"? Is be an emission? a transport term? both?

9. even though the development from equation (1) to (2) is mathematically correct (if agreeing on the assumptions), the conclusions on it seem incorrect; the authors mistake determining L and changes in L; thus there analysis on the formulation of the IPD at time t is clueless; they should analyse $IPD(t) - IPD(t_{ref})$ instead

For all these reasons, the authors are required to start their analytical formulation from scratch and thoroughly analysing each term and intermediate step of it starting from the crude definition of the IPD:

$$IPD(t) = \frac{1}{\Delta r} \int_{r=53}^{90} c(t,r)dr - \frac{1}{\Delta r} \int_{r=-90}^{-53} c(t,r)dr$$

with $c(t,r) = \int_{t=-\infty}^{t} \int_{r=-90}^{90} S(r',t') \times H_{r' \to r}^{t' \to t} dr' dt'$; S being a surface flux term and $H_{r' \to r}^{t' \to t}$ being the transport term. Such a formulation represent the temporal past influence of sources all over the world. The authors could equivalently develop the formulation for IPD_{Δ} as the weighted average of point concentrations.

Developing such an expression and then calculating $IPD(t) - IPD(t_{ref})$ should make obvious the flaws in using the IPD directly to deduce Arctic emission variability and then could be used to comment and discuss efficiently the numerical experiments. Explicitly separating emissions to transport would also allow to comment the inter-annual variability from the varying transport patterns.

2 Technical comments

1. p.2 l.54: $-53^o >$ latitude $> 53^o$: even though understandable, this writing is clumsy
2. section 2.1: observations are not used in the manuscript, apart from there location; this section could be shorten (especially details on instruments, calibration, etc.) and moved to appendix, as well as figure 1
3. p.5 l.131: a sinus is a mathematical operator with no unit; thus it is not correct to directly add σ_i (in ppb) to $\sin(\phi_i)$
4. p.5 l.140: "the atmospheric transport fluxes of emissions": this is not clear
5. p.6 l.175: "during 1990" -> "during the year 1990"?
6. p.6 l.180: when is the noise? all over the period? is it normal then that the IPDs merge together in 2010 in fig. 6B?
7. p.9 l. 279: please check the date of launch when reaching the proof-reading step as it is regularly postponed...