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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Detecting changes in Arctic methane emissions: limitations of the inter-polar difference of atmospheric mole fractions" *by* Oscar B. Dimdore-Miles et al.

Anonymous Referee #3

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Detecting changes in Arctic emissions is critical in the framework of fast regional warming in the poles. CH4 emissions are expected to rise dramatically in the next decades with the timing, amplitude and localization remaining largely unknown. The inter-polar difference was proposed in the past to detect emission changes with a simple metric. The authors evaluate the relevancy and robustness of such method.

The IPD is known to have weaknesses and flaws. Only highlighting them with two simulations computed with a GCM clearly would not make a valuable scientific contribution to the community. The only valuable content of the manuscript is to show that in idealistic (hence unlikely) conditions it would require >15 years to detect changes

in Arctic emissions (hence forever under-realistic global emission scenario). A useful contribution would require (i) a thorough evaluation of the limitations of the method as defined by Dlugokencky et al., and (ii) quantified recommandations towards a less "simple-minded" but usable metrics. The poor scientific and presentation quality of the submitted manuscript hardly contribute to (i), and (ii) is fully missing. Plus, the introduction and conclusion seem to be written independently of the main body of the manuscript: it is obvious that we need a long-term accurate network in the Arctic, and that we must analyze the data with some Bayesian inversions to get valuable insights, event though the manuscript discussion does not really proved it explicitly...

The authors are recommended to drastically improve the quality of the manuscript and to complement its scientific content. Here are suggestions:

- propose combinations of emissions that would explain the observed IPD of the 30 past years, but would be unrealistic, hence proving the weaknesses of the IPD
- comment and analyze the observed IPD in term of real emissions as represented by your model
- assess the impact of the choice of stations (number, location) on the IPD
- suggest an improved IPD (probably including some transport but still being simple enough) that might point to changes with some confidence interval
- what impact TROPOMI and MERLIN could have on the IPD estimates? Having idealistic polar satellite coverage could make a comparison with the idealized IPD as defined in equations 1 and 2

1 Units and equations

Not a single equation in the manuscript uses homogeneous units. This makes the equations either useless, or even misleading.

Regarding equations (1) and (2), the authors try to formulate explicitly what information is included in the IPD. It is impossible to know exactly where the terms come from and more complex ones are probably missing. It is recommended to explicitly mention all possible terms influencing the IPD (transport, chemistry, emissions, from every points on the globe), and then simplifying the equation step by step to deliver a clear message.

With rigorous formulation, the rest of the discussion might be articulated around approximations and limitations in the IPD

2 Other comments

- Is it needed to fill the missing values in observed concentrations? The original IPD was designed to be used at the annual scale as a very general indicator of the integrated difference in hemispheric emissions
- The measurement uncertainty, computed as a standard deviation, seemed to be based on two values only. That hardly makes a statistical sampling...
- l. 160: is the noise temporally distributed? spatially? It seems that emissions have a biweekly noise amplitude of 50% which seems unrealistic. A 50% noise could be realistic but on a longer time scale

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