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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.

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First, we thank the two reviewers for providing their comments on our analysis. Their comments have helped us to clarify the motivation and derivation of our analytical model. They have also helped us to put our work in the context of the wider challenges associated with detecting changes in Arctic emissions of methane. We apologize for the unnecessarily large number of typos, which are an egregious oversight. Below, we respond to individual reviewer comments (italics).

Reviewer 1

This paper addresses the inter polar difference in methane. This concept was introduced as a "data-only" method to study changes in the methane emissions. Most notably, the observed step-like drop in 1991 was attributed to sudden changes in the emissions from the former Soviet Union. Likewise, it could be a useful metric to signal increases in Arctic methane emissions resulting from climate-related thawing of permafrost. The second part of the paper addresses the detectability of emission changes in the Arctic, and claims that changes and variability in emissions elsewhere (mid-latitude Northern Hemisphere, tropics) would blur the Arctic signal. Although not very surprising, this is a message that could deserve a paper. In the first part of the paper, the authors introduce an "analytic" model, mainly for illustration purposes. This part is not well thought of, unclear, contains errors, and does not contribute in any positive way to the paper. There is absolutely no link to the analytical model and the 3D model simulations with perturbed methane sources. The two points made around line 80 can be made without the analytical model, so I recommend (and this is expressed mildly) to remove this part entirely from the paper.

We included the analytical model because it illustrated that the IPD as originally defined would unlikely be able to achieve what it claimed. It also helped to motivate the numerical experiments. Admittedly, we only included a summary of this model description, and did not include a link between the first and second equation. Because we strongly believe this model provides pedagogical value to the paper we have expanded our description of this model, and in doing so we show more clearly why accounting for atmospheric transport prevents the IPD from effectively isolating Arctic emissions of methane.

Detailed argumentation: Method is simple, but contains a number of flaws. On equation (1): 1. IPD has units ppb, which clearly differs from L (mass per time) and B (also mass per time, but unit in the integral should be different. add: per degree latitude).

This reviewer is correct. The IPD could equally well be described in terms of the mass or the mixing ratio of methane. For consistency with the original definition we have clarified this point in our derivation.

2. Now it looks there is a hard cut-of at r, where a unit of emissions decides to flow either to the NP or to the SP. In reality there is probably a latitude where emissions do not contribute to an IPD. North and South of this break-even point, an emission progressively contributes to setting an IPD. This issue is not reflected in the formula.

This is a continuous integral in which air masses are free to flow either northward or southward. The point r can be evaluated anywhere between the North (r=0) and South (r=R) pole. If the role of atmospheric transport was negligible (equation 3) then the original definition of the IPD would be able to isolate L(t). Evaluating the importance of these atmospheric transport terms is the main point of the study. Otherwise, we do not understand this reviewer's comment.

The limits of the integral are not clear. Where is "r"? Somewhere between the North pole and the South pole?

Correct, as per the definition in the paper (line 68 of the ACPD manuscript) and in the accompanying Figure 1.

On equation (2): This now adds more confusion: I do not see how equation 2 follows from equation 1 (this relationship reduces to. . .?). Even more surprising is the change of sign, which suggests that an increase in emissions North of r would reduce the IPD. I really wonder if this analytical model has been tested using a realistic latitudinal emission distribution. I guess not!

We have now provided a more thorough version of the derivation. Our interpretation of equation is different to this reviewer. We hope the revised derivation will remedy this.

Further remarks and corrections to the manuscript are in the annotated pdf. Please also note the supplement to this comment: https://www.atmos-chem-phys-discuss.net/acp-2017-1041/acp-2017-1041-RC1- supplement.pdf

Typos have been addressed. The main points about the equations are addressed

above.

We have fixed a bug in the IPD error propagation. We thank the reviewer for pointing this out.

This reviewer raises the point about the sine weighting of the measurements (equation 3). To test the original definition of the IPD we have faithfully followed the definition provided by Dlugokencky.

Reviewer 2

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.

We use the (revised) analytic model to highlight the main flaws of the original IPD approach, which allowed us to pinpoint the necessary computational experiments to run. Additional experiments (not shown) only serve to repeat our message. We hope this paper will dissuade future studies from using this metric for the purpose of isolating Arctic emissions.

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).

We agree this is very valuable content. But this was only achieved by developing the analytical model that highlighted the numerical experiments we needed to run that subsequently allowed us to determine the timescales over which we could detect changes in Arctic emissions.

A useful contribution would require (i) a thorough evaluation of the limitations of the method as defined by Dlugokencky et al., and

We could not find any specific recommendations put forward by Dlugokencky. Nevertheless, our analytical model and accompanying numerical experiments show a fatal flaw in the use of the IPD that negates any further evaluation of this metric.

(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.

We believe there is no such usable metric and that is one of our points. This paper was about the role of atmospheric transport in the real IPD. Bayesian inference of Arctic emissions lead to uncertain estimates that reflect data sparsity of ground-based coverage and seasonal-bias of SWIR satellite observation (coverage is non-existent during winter months when the solar zenith angle precludes useful retrievals of column methane). The advent of active satellite sensors (e.g. MERLIN) capable of observing during day and night in all seasonal may open up the possibility of developing a more robust observational capability for the Arctic. We would then not require any data-led metric.

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...

We agree with some of this comment. We explicitly mention (abstract and main text) that we need to maintain that network of measurements in the Arctic. But there is no quick data-led metric that can isolate Arctic emissions of methane. Bayesian inversions

do provide valuable insights but the data are not particularly dense over and upwind of the Arctic. Warming of the Arctic due to changes in climate is uneven so that could easily envisage a situation where methane emissions are far upwind of any measurement. If anything we would argue for a high density of measurements over the Arctic if and only if someone was determined to develop a data-led metric.

The authors are recommended to drastically improve the quality of the manuscript and to complement its scientific content. Here are suggestions: 1) 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; 2) comment and analyze the observed IPD in term of real emissions as represented by your model; 3) assess the impact of the choice of stations (number, location) on the IPD; 4) suggest an improved IPD (probably including some transport but still being simple enough) that might point to changes with some confidence interval; 5) 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.

We thank this reviewer for their suggestions. Suggestion 1) is the reverse of what we have studied. But we do not believe this approach would necessarily (dis)prove the IPD. The forward approach we have taken is cleaner and more straightforward to understand. The inverse approach, because it is a under-determined and ill-posed inverse problem due to poor data coverage and transport model errors, results in a range of possible outcomes. A similar argument can be made for suggestion 2. Re suggestion 3, choosing the stations to determine the IPD is not a useful line of questioning given our results. Without explicitly accounting for atmospheric transport a data-led metric will not work. Suggestion 4 is a Bayesian inversion but the data are too sparse. Ensemble model runs show a range of possible emissions for the Arctic (AMAP 2015 ref from main text). Suggestion 5 is mentioned in section 4 of the paper, although not in the context of the IPD. We have expanded on our description of satellite data based on our responses to reviewer comments.

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