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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 #1

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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.

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

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! The only thing I really agree with is the word simple-minded.

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

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