

Interactive comment on “Satellite observations of atmospheric methane and their value for quantifying methane emissions” by Daniel J. Jacob et al.

Anonymous Referee #2

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This is a useful review of satellite observations of atmospheric methane although it is somewhat biased towards the US and towards the oil/gas industry. Perhaps this is not surprising given the current political climate in the US but I would nevertheless argue that the review should address some of the larger sources of methane (e.g., wetlands). It is well worth publishing in ACP. I have provided some comments that the authors should consider when they revise their manuscript.

This reviewer was immediately struck by the claims of GHGSat in the abstract (and elsewhere in the manuscript) despite there being little information provided in the scientific peer-review literature about the project or the data. As with all potential performances of future instruments the authors should consider toning down unsubstantiated

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claims.

The authors provide a list of instruments (past, present and future) in Table 1. Is this consistent or an update of the CEOS list? Table 2 does not include MicroCarb, which is a mature mission concept led by CNES (admittedly the CNES website mentions methane as a sidenote to CO₂).

Line 133. What this reviewer would find more desirable for instrument sensitivity is weighting towards the lower troposphere where variations are more sensitivity to surface fluxes. The counter argument would be that a vertical column that is uniformly sensitive to the troposphere is less sensitive to vertical mixing and therefore easier to interpret with atmospheric transport models. In any case, there appears to be two lines of thought on this topic that would be useful to at least mention in this section.

Paragraph starting line 213. This reviewer would shamelessly argue that using the ratio directly is the more natural way to use these proxy data that are less prone to systematic errors. Mentioning the direct use of the ratio data here would help put the later discussion about inverse modelling into context.

Statement starting on line 239. Might reflect? That sounds like supposition to this reviewer. Can the authors provide a stronger statement?

Lien 260. Comment: TIR measurements are useful at quantifying emissions from fires where there is a large thermal contrast between the lower troposphere and free troposphere due to intense surface heating.

What always strikes me as an argument for geostationary concepts is the temporal sampling bias of sun-synchronous instruments. The authors have put together an informative table including local overpass time. Most times are 0930 or 1330. These times matter, although the local overpass times of the satellites are not typically defined by the methane instruments. What implications do these times have on the science objectives? Perhaps not much for the point sources and the oil/gas industry but they will have

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for diurnal-varying sources. Would be useful for a reader to see an acknowledgment of this bias.

This reader thought that section 3.1 was useful but would be more appreciated in an appendix. Section 3.2 was much more relevant to a review on satellite observations of methane, but it does lean on section 3.1.

In section 3.2. What about the soil sink for methane?

Statement on line 820. This section is important for setting the scene for matching science requirements with instrument requirements. The science requirements for policy and point sources are somewhat different, but the science requirements (repeat times, time of day, accuracy/precision) for (natural) diffuse sources are different again. Wetlands, rice paddies and ruminants are significant sources of atmospheric methane but they are not addressed in this review, e.g. what kind of concept would be needed to observe them better than they are today? Thawing Arctic land surfaces are potentially an emerging source of atmospheric methane that will perhaps require an active mission but this is not discussed. This reader is not encouraging the authors to make their review much longer (on the contrary) but there needs to be a more balanced review of the future challenges.

Line 1014. This reviewer agrees that multi-species inversions are the way forward. But the reader should be exposed to the challenges involved, even with observations collected by the same instrument, otherwise the suggestion appears as a trivial extension to existing studies. Differences in vertical sensitivities for different gases (wavelengths) need to be carefully considered.

Figure 2. Some configurations are shown. Atmospheric limb sounding has its advantages.

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