

Interactive comment on “Improved Atmospheric Characterization through Fused Mobile Airborne Surface *In Situ* Surveys: Methane Emissions Quantification from a Producing Oil Field” by Ira Leifer et al.

Anonymous Referee #1

Received and published: 26 June 2017

Comments on Leifer et al: Improved atmospheric characterization. . .

General comments: This is an interesting and innovative paper, describing how both mobile surface measurement across strong local topography and parallel aircraft measurements have been used to derive methane emission fluxes. This is an important problem, and has wide applications. By using aircraft and vehicle to validate each other, both methodologies are improved. In the US, instrumented aircraft are available. In many less wealthy nations that is not the case and measurement will have to depend on vehicles supplemented by light drones. The topographic problem is also

C1

widely applicable, in the many locations where the boundary layer is poorly mixed. Thus although aspects of the experiment can be criticised and the introductory section is very inadequate, the paper is an interesting and useful contribution, that should be published after revision.

Specific details: The Introductory section is incoherent, and depends in part on outdated papers. It needs to be revised substantially.

Line 35 should probably cite up to date NOAA results, perhaps from Dlugokencky's papers. For lifetime (also line 43-44), be careful not to mix up the various definitions of lifetime and again best to use a recent Dlugokencky paper or use IPCC. Line 38 – Nisbet et al 2015 is an error. No such paper. It's 2014. Maybe better cite the major Nisbet et al 2016 Glob. Biogeo. Cycles paper, and also Schwietzke et al. (in Nature recently) on fossil fuel emission. Either here or in L50 cite Saunio, M. et al (2016) The global methane budget 2000–2012, Earth System Science Data, 8, 697-751, doi:10.5194/essd-8-697-2016. Line 43 Claim of a 40% drop in lifetime - There is a major discussion on whether or not OH is changing and maybe that's far beyond the scope of this paper. Also may be confusing lifetime definitions (perturbation/replacement) in general comment. Line 49. See for example Rigby, M. and 18 others (2017) Role of atmospheric oxidation in recent methane growth. Proc. Natl. Acad. Sci. USA. 114, 5373-5377. Line 50. Maybe cite Saunio, M. et al (2016) again. Line 51 on – several more recent papers to cite on US fossil fuel emissions. Bruhwiler et al (2017) in JGR very effectively questioned Turner et al. , so I think this could be rewritten as the Turner et al conclusions should be discounted. Line 58-59 – The inventory discussion could be picked up around line 55 and should lead into Kirschke et al, and Saunio et al (2 papers, 2016, 2017) and maybe mention Nisbet and Weiss's earlier top down/bottom up comment in Science, 2010. Line 62 – many more recent papers than White 1976! This paragraph L61-70 needs updating. Also it could mention some of the European work – for example in several papers by Bergamaschi et al. Line 92-3 could be written by a PR firm selling dodgy goods. Most people live on the plains, not in the high Hi-

C2

malayas. Line 120 – reference for air flow to 1km only? Figure 1a looks like a photo of some poster on the floor. . .terrible, key is unreadable. Why the angled projection???

Line 168 ‘stranded CH₄ clouds’ – we see these too, but don’t call them stranded. They are not washed up on the beach (strand) – they are making their own independent progress as yet unmixed. They are important as they come from single event sources, which can be keys to the emissions from a gasfield – eg water processing. Line 196 - precision and accuracy please, calibration etc etc. (though I accept the point in L252)

Lines 228-247 calculation and also 248-253. Maybe cite some of the Aliso canyon work here? L271 – pre/post-surveys. Good. When was this? How comparable in meteorology?

L300 ± 25 This section is very interesting. I’m surprised the PBL is located to a 20m precision in the complex topography. Seems very precise. L312 – tall pine trees? How tall - Not being Californian, are these 5m (tall in N.East Norway hills) or 150m redwoods? L342 – really a town called Bodfish? are they inland codfish? or an obscure mermaid-body genus? Is this paper really about Bodfish bubbleology? The mind boggles. L365 – there is a lot of agriculture in the area – cows, wet fields, drains. This needs to be discussed. Are there any large cattle feed lots in and around Bakersfield? Landfills? Fig 8 – really interesting figure could have a bit more discussion. L396 – how rapid is the rise? Any guesses? L402 – nice to get some d¹³C(CH₄) isotopes to pin down the methane. See comment above about cattle and landfills in the area. Also Bakersfield is a big city – sewage. L435 – intermittent activity – what the paper calls ‘stranded plumes’ – any idea how common this is? L440,442 – collected Lagrangian – English language disjunction. ?in Lagrangian mode?? L441 – poor English again – a platform? Or platforms? L471 – faux precision. 1.6km L473-480 – good. L503 – mountain peak – hitherto hidden in the discussion, now pops out. I hope you told the pilot. . .could be mentioned earlier as a problem. L532 – maybe cite some of the NOAA work in other gasfields – intermittent sources. L533 – CO₂ – maybe, or just a lot of trucks and all sorts of other sources? L540 and 551 – would be nice to see some isotopes on this. L556 – repeat comment – no, we do NOT all live on the tops of Everest

C3

and Aconcagua. We live in the swamps of Calcutta and Shanghai. Schiphol runway, the center of Holland, is 11 ft under the ocean. Agreed, this comment is very useful for Tehran and La Paz (both with major local gasfields) but it’s irritating to hammer it twice.

Overall – interesting paper. Accept with revision.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-133, 2017.