

Interactive comment on “Quantifying the Loss of Processed Natural Gas Within California’s South Coast Air Basin Using Long-term Measurements of Ethane and Methane” by D. Wunch et al.

Anonymous Referee #2

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This manuscript presents two independent time series of column measurements of methane (C1) and ethane (C2) in Southern California and uses them to infer trends in annual emissions in the region. The manuscript is generally well-written and I would recommend publication with targeted additions and clarifications as further described below. In particular, the manuscript would be improved by providing a more systematic discussion of the causes of recent changes in C1 and C2 emissions.

General comments

The authors should consider and discuss the statistical significance of the reported trends in observed C1 and C2. The confidence intervals around the annual averages in Table 1, for example, suggest the annual averages across the 3-years shown are

C1

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not statistically different. On the other hand, assuming the error bars shown in Figure 3 are correct, the 2015 values for C2 emissions seem to be statistically higher than those during 2006-2010. The authors should consider whether the monthly C1 and C2 emission time series in Figure 6 provide an alternative basis to determine the existence of a significant trend (e.g., are the slopes statistically different than zero?).

Wunch et al (2009) used CO₂ instead of CO as the basis to estimate CH₄. Also, Wunch et al (2009) pointed out the possible underestimation of CH₄ if it was computed from CO emissions, given their differing diurnal profiles (CO emissions primarily influenced by traffic, which was believed to be a stronger daytime source than methane). This new discussion manuscript does not address these issues. The authors should clarify how potential differences in the diurnal profiles of CO, CH₄, and C₂H₆ could affect the emissions estimates calculated with Equation 4.

The authors should emphasize the importance to their analysis of the changing C₂:C₁ ratio in pipeline gas. This trend appears to serve as tracer of opportunity, a unique fingerprint that allows attribution of the total observed C₁ signal to infrastructure associated with handling, storage, delivery and use of pipeline quality natural gas. This is done indirectly in line 220, but the scientific novelty and utility of the trend deserves greater attention.

The manuscript's impact would be improved if the authors could provide a more complete picture about the contribution of specific source types to the observed C₁ and C₂ trends. Having partitioned the fraction of total methane signal due to pipeline gas (possible due to its increasing ethane content), can the authors further delve into the individual methane and ethane trends and provide a conceptual model that explains the recent trends or patterns in monthly/annual C₁ and C₂ emissions. [It would seem the C₂ emissions might be reducible to a 2-source model (pipeline gas and associated gas/geologic seepage) with appropriate adjustment for vehicle emissions. Similarly, C₁ emissions might be reducible to a 3-source model, by adding a generic third term for biogenic C₁ sources.] At a minimum, the authors should clearly indicate whether the

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increasing C2:C1 ratio in pipeline gas is, by itself, sufficient to explain the potentially increasing C2 trend in Fig 3 and 6? Or can the balance of the C2 budget not explained by pipeline gas losses be explained: for example, given likely associated gas compositions, could the local oil/gas production to which Peischl attributed 32 Gg C1 also account for the excess C2 that is not explained by losses of pipeline quality gas? Alternatively, are other causes required?

Once the C2 budget is determined, and knowing the C2:C1 ratio of pipeline gas, what can the authors say about the trend in C1 emissions due to losses of pipeline quality gas? It would be valuable if the authors could provide an assessment of whether the data indicates that downstream natural gas emissions in the region are changing.

It is not clear from the text at line 225 and the reference cited how the authors derive the mass of C1 delivered by SoCalGas to customers within the SoCAB. It is also unclear why sales data going back to 2003 are relevant at this point of the discussion focused on the regional methane budget in 2015 (it would be more relevant – indeed desirable – to show historical gas deliveries in Figure 4). Southern California Gas' annual report (Sempra Energy 2015 Financial Report) reported annual volumes of gas sold in 2013, 2014 and 2015 of 999, 944 and 925 bcf (average 960 bcf, or 17.5 Tg assuming a methane content in gas of 95%). The authors should explain how they partition the SoCalGas' systemwide sales to isolate the customers solely within the SoCAB. Because not all of the gas sold by SoCalGas is consumed within the SoCAB and may or may not be transported through the SoCAB, the authors should report multiple metrics for the loss of pipeline quality gas that is sold or transported across the basin. One metric would be % of methane delivered that is emitted, and the other is the emissions as a percent of methane throughput in the SoCalGas system. The latter yields a loss rate for pipeline gas of 1.4% of potential throughput (242Gg/17.5Tg). The comparison to Wennberg et al's 2% loss rate should be done with caution, ensuring that the quantities in the numerator and denominator are apples-to-apples between this work and the previous work (it seems the 2% in Wennberg would most appropriately be compared

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to 1.4%, as calculated above).

Figure 4. The manuscript would be improved if the hydrocarbon production data provided was specific to the SoCAB rather than statewide (these are publicly available from state agencies). Additionally, since hydrocarbon production is only a small contributor to C1 and C2 emissions in the SoCAB, this figure would be much more useful if it presented publicly available activity trends for other chief sources – in particular, I would suggest SoCalGas’ natural gas sales and livestock populations. Recent CH₄ emissions data or landfills and waste water treatment plants may also be available through the US EPA Greenhouse Gas Reporting program or California state equivalents.

The richest findings seem to derive from the more recent and denser Caltech FTS measurements, with the JPL MkIV FTS data providing corroboration and further insight about historical trends. The manuscript’s flow and clarity might be improved with some reorganization of the results and discussion or more explicit delineation of how the two data sets are used to support the conclusions reached.

The results relating to Aliso Canyon are interesting and important, but are not central to the paper’s main findings. I would recommend moving the Aliso Canyon discussion into a separate subsection.

Detailed comments

Abstract Line 9. The introduction of “Our methane emissions record” here is confusing since line 4 refers to a record dating back to the 1980s.

Abstract Lines 10-15. This wording might be misconstrued to imply that the source of the excess methane is the gas storage facility. In fact the gas storage facility is only mentioned since it is a reliable source of C₂:C₁ ratios. But the authors have a secondary data source (delivered gas) that yields a statistically indistinguishable trend line in Fig. 5. The authors should revise the language to indicate the comparison is between atmospheric measurements and measured C₂:C₁ of gas delivered and stored in

the region. Additionally, the authors should more explicitly indicate the scope of natural gas infrastructure implicated in the final sentence – to indicate it includes gas delivery infrastructure including pipeline leaks (transmission and distribution), compression and storage facilities, and post-meter losses among others.

Line 179. It was unclear how the statement about ethane to acetylene ratios followed from statements about C₂:CO and acetylene:CO; please elaborate on the significance.

Line 226. The statement attributing 242 Gg/yr C₁ to natural gas infrastructure should be linked back to the prior paragraph's finding that 54% of total excess was due to natural gas (e.g. "242 Gg/yr, equal to 54% of the SoCab total. . .").

Lines 248-255. The specific value used for GWP₁₀₀ should be stated (e.g., 25, 28, or 34). The choice of 100-yr GWP in this paragraph does not account for the greater short-term climate impacts of CH₄. The authors should consider reporting a 20-yr CO₂e value in addition to the 100-yr value. The reference to climate impact in the last sentence needs to explicitly distinguish short- and long-term impacts; if only 100-yr GWP comparisons are made, then the sentence should be clarified to refer to "long-term climate impact. . ."

Figure 1. The very rapid rise in C₂ mole fraction in the most recent JPL MkIV FTS measurements should be explained (panel 3). Is this trend due to the increased C₂:C₁ ratio, the Aliso Canyon blowout or both? Should the C₂ rise be accompanied by changes in C₁?

Editorial Comments

Line 141. The word "are" appears twice.

Figure 1. The black Mauna Loa data points are significantly obscured by the CO and C₂ data points.

Fig 3. The error bars are hard to make out and the symbol for the Peischl et al is not evident.

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