Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-90-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measuring methane emissions from oil and gas platforms in the North Sea" *by* Stuart N. Riddick et al.

Anonymous Referee #1

Received and published: 21 March 2019

General Comments

This study presents estimates of methane emission rates from oil and gas production platforms in the North Sea during normal operation without flaring. The authors collected and analyzed three types of data; 1) in-situ observations of CH4 mole fractions at Weymouth, UK; 2) samples of air collected in Tedlar bags and analyzed for CH4 mole fractions and carbon isotope ratios using continuous flow gas chromatography/isotope ratio mass spectrometer; and 3) shipboard in-situ observations of CH4 made circular transects around individual platforms. The in-situ observations of CH4 mixing ratios at Weymouth were only used for qualitatively. They analyzed the isotope data with a keeling plot to determine that enhancements had a major thermogenic component. They analyzed the shipboard in-situ observations with a Gaussian plume model. Anal-

Printer-friendly version

Discussion paper

ysis of the shipboard observations suggested methane emissions significantly higher than public inventories would suggest, but insufficient to explain the enhancements measured at Weymouth.

Overall, I think that this is paper has promise but needs major revision and possibly more work. I have reservations about: 1) the applicability of the Gaussian plume model to the conditions, though these reservations could well be ameliorated with additional information and author responses. 2) The relevance of the observations made at the coast 3) the quality of the results of the Keeling plot analysis 4) the uncertainty analysis of the Gaussian plume model

I may be missing it, but I can't find public access to much of the data in this paper. Best practices for reproducibility would have all data publicly accessible with access instructions given in the paper. I would appreciate the opportunity to inspect and analyze the data before making the recommendation to publish. There are a few places in the paper with insufficiently detailed information to fully understand the study - for example, what are the coordinates of the platforms and on what day was what platform observed? How much did winds vary over the course of the ship transects?

Specific Comments

Critique #1

The use of a Gaussian plume model requires careful consideration of the assumptions that go into such a model. Namely, the model assumes a homogenous, steady state flow with a steady point source.

I think that, for the most part, the conditions in this study satisfy those assumptions, but I do have the following reservations.

The Gaussian plume model employed by the authors assumes an infinitely high boundary layer and homogenous mixing throughout the boundary layer– which is to say that they include a reflection term at the surface, no reflection term at the top of the bound-

ary layer, and a uniform vertical mixing. This is a marine environment in a cool climate during the summer, and so a marine layer is likely. This would come with a very low boundary layer height and temperature inversion near the ocean surface. The emission heights are 50-70m and the inlet height is 2.5m. The assumptions of homogenous vertical mixing and no reflection off the top of the boundary layer are at risk.

I had code on hand to extract and plot meteorological fields from the GFS global forecast model archives (raw data obtained from https://ready.arl.noaa.gov/archives.php). I extracted boundary layer heights and winds at 1300UTC on the days of the campaign and plotted them below. Boundary layer height capped at 1500m in the plot for visibility. These data carry the caveat that GFS archive forecast data has error – particularly in the boundary layer height. I am happy to share these data/plots with the authors for their own use if they wish.

The paper includes a plot of the studied platforms, but no quantitative description of the locations (e.g., latitude and longitude, and which day each was measured). However, it does appear that the boundary layer height in the vicinity of the platforms may have been quite low, depending on when each was observed.

Critique #2

While the investigation that forms the bulk of the study is logically solid, the abstract begins with a line of reasoning that is quite circumstantial. It describes what motivated the study. While it is interesting to read about the authors motivation, excluding this information would make the definitive methods of the investigation clearer.

The passage is: “Recent studies suggest oil and natural gas production facilities in North America may be underestimating methane (CH4) emissions during extraction. This, coupled with unusually high CH4 mole fractions observed at coastal sites during onshore winds in the UK, suggests CH4 emissions from oil and gas extractions in the North Sea could be higher than previously reported.”

I don't think the conclusion necessarily follows. Emissions can vary greatly between facilities and across production fields. The geology and technology used in the North American fields where the aforementioned studies were conducted is much different than those of the North Sea. Unusually high mole fractions observed at the coast when winds came from the sea do not necessarily point to emissions from the oil and gas industry. Airmass trajectories can be quite complicated and there are many sources on a continental scale.

If the authors want to include the in-situ observations at Weymouth in the paper, then they should include a trajectory analysis. The paper does work without this passage, though.

Critique #3

What is the uncertainty of the parameters of the linear model from your Keeling plot (Figure 2a)? What is the uncertainty in the source isotopic signature? From the appearance of the plot, there is a very poor correlation between observations and very high uncertainty in the source isotopic signature. It might be instructive to color the points by time. The data are likely insufficient to describe the source. It could just be that all the data were taken near each other in time, and so there is not enough variation in the CH4 concentration to extract a signal.

Critique #4

The description of the uncertainty analysis for the Gaussian plume model is lacking in detail, and there are some red flags. For one, the total uncertainty is given as +/-54% while the uncertainty due to stability class uncertainty alone is estimated at 54%, and the greatest source of uncertainty is said to be the emission height.

The uncertainty analysis does not explicitly define what is meant by "uncertainty". It is said "The overall uncertainty, calculated as the root of the sum of individual uncertainties squared...". This implies that the uncertainties are standard deviations of normally

distributed random errors. But the uncertainties are almost certainly correlated.

Technical Corrections

Page 1 The abstract should include a concise description of the methods

Page 2, Figure 1 Caption: Mention Weymouth observatory.

Page 3, Line 16: “To investigate the loss of CH4 from offshore oil and gas installations we use two approaches; 1) determine whether the source of CH4 enhancements at WAO could be from oil and gas production platforms; and 2) estimate an average CH4 loss from offshore installations by making direct measurements of CH4 emissions from off-shore production platforms in the North Sea”. The listed items don’t seem to be “approaches” to investigating the loss of CH4. Is this a typographical error? It would be nice to see this information replaced with a clear and concise description of the methods used in the paper.

Page 3, Line 24: “between 10:00 and 13:00” please include time zone.

Page 4, Line 6: “Measurements from boats of CH4 emissions from individual production platforms...” Careful, the CH4 mole ratio was measured and the emission rate was estimated using a simple model. I think it’s a reach to say that the emissions were measured. The previous sentence uses my preferred language.

Page 5, Line 7: “The gas is considered to be well-mixed within the volume of the cone” This is an inaccurate description of the Gaussian Plume model. A Gaussian Plume describes the distribution of the mass of the gas at a given time as a multivariate Gaussian in space. To say that the gas is well mixed within a volume would suggest a uniform distribution in a finite region.

Page 6, Figure 2a): The correlation here looks very weak.

Page 8, Figure 3: Can this figure this be a plate showing all the observations rather than just the observations from 1 platform? I’m assuming the arrow shows the average

wind speed and direction? How much variability was there?

Page 9, Line 4: “The main uncertainty using the Gaussian plume approach in this study is in estimating the height of emission...” I would argue that there are many large sources of uncertainty in the Gaussian plume model, some of which are almost certainly greater than error in the emission height. For example, the assumption of homogenous diffusion.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-90, 2019.

Printer-friendly version

Discussion paper

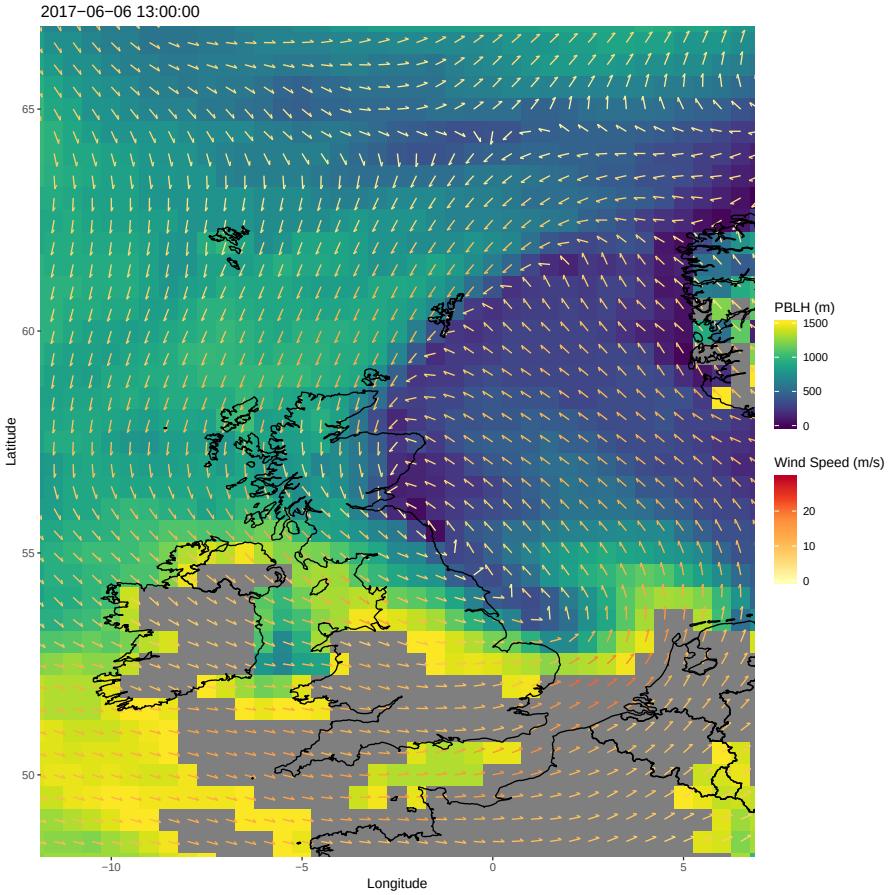

Fig. 1.

Printer-friendly version

Discussion paper

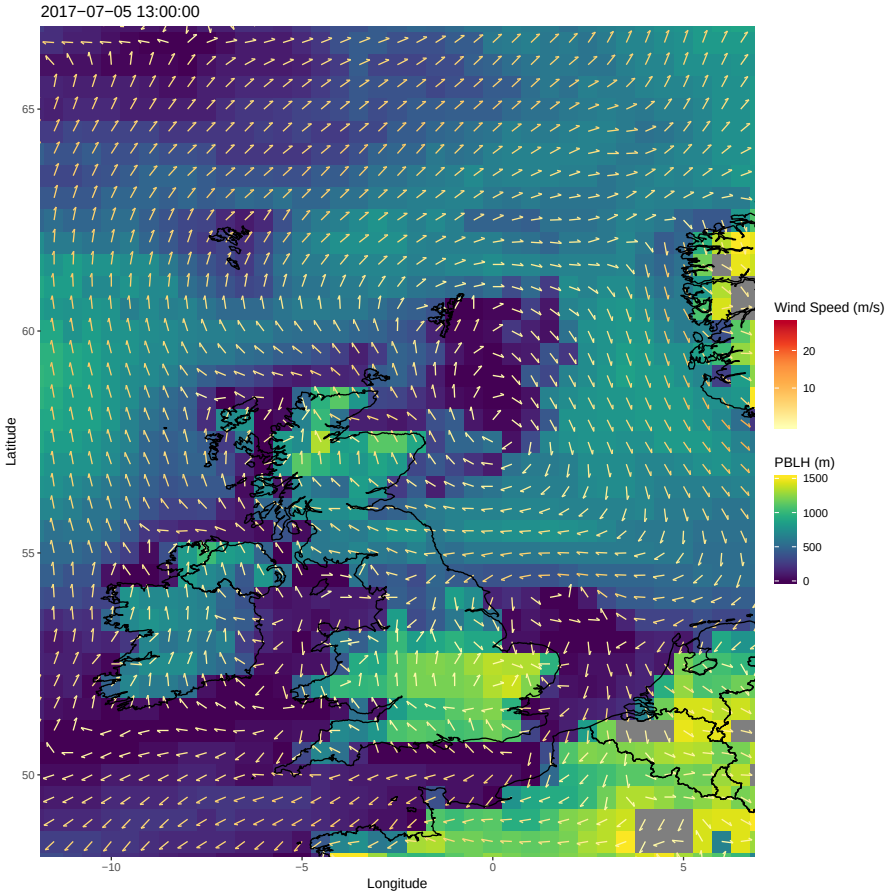

Fig. 2.

Printer-friendly version

Discussion paper

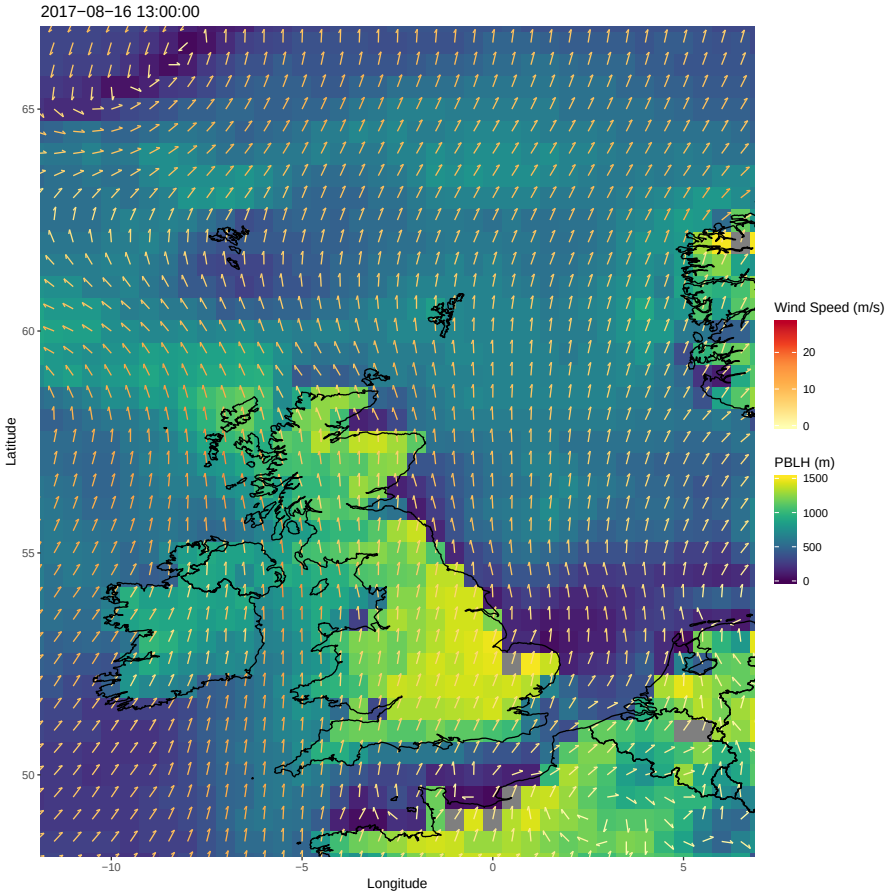

Fig. 3.

Printer-friendly version

Discussion paper

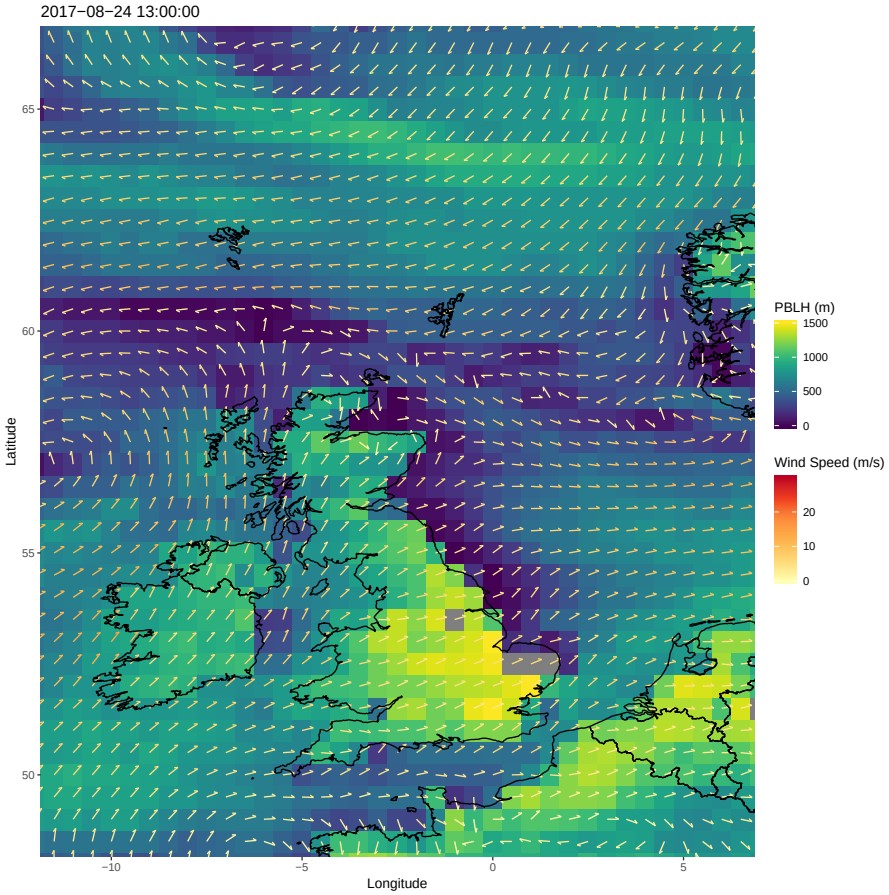

Fig. 4.

Printer-friendly version

Discussion paper

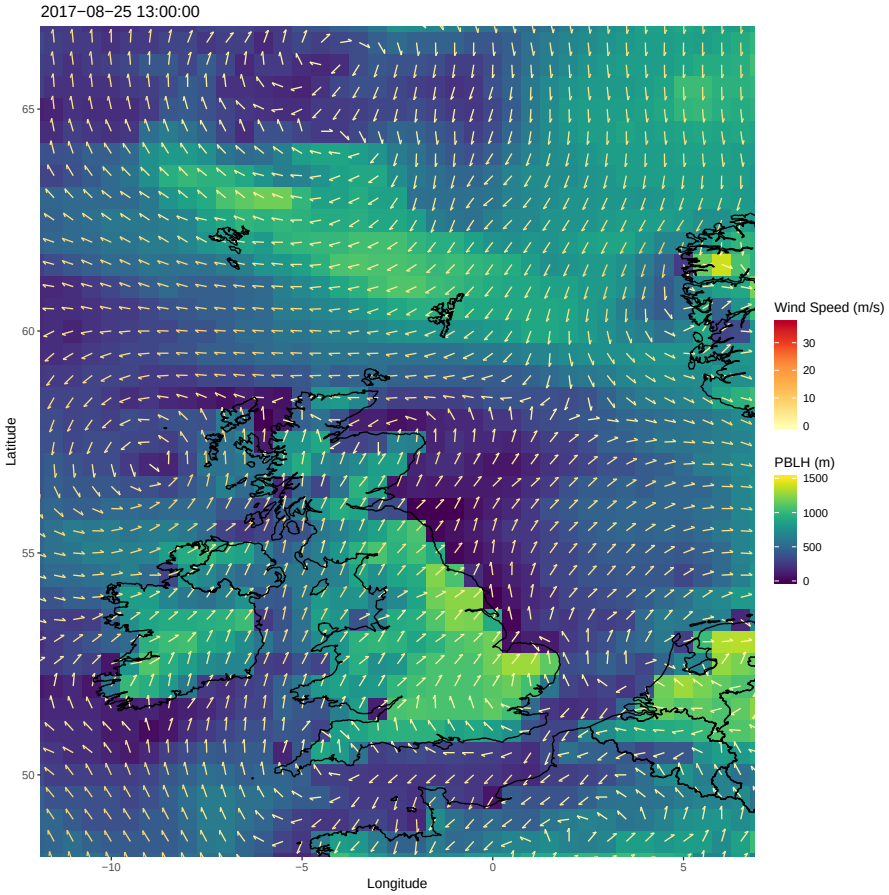
2017−08−25 13:00:00
Latitude
65
60
55
50
−10
−5
0
5
Longitude
Wind Speed (m/s)
30
20
10
0
PBLH (m)
1500
1000
500
0

Fig. 5.