

Review of Dogniaux et al. (submitted to AMT)

This group of authors shows their expertise in the method of estimating emission rates from two imagers, Sentinel-2 and Landsat 8. This is a well-written paper. I have no qualms regarding the use of English. I believe the paper should be rejected because of the two main criticisms (immediately below), combined with the fact that the methane emission uncertainties are already very large. If the uncertainties were not so large, I think the paper is sufficiently interesting and meritorious to be published.

Main criticisms:

- 1) the ensemble approach for estimating the methane emission rate does not cover the correct range of input values. An ensemble should not span  $\pm 1\sigma$  because this only covers 68% of the data. I think it would be more appropriate and simpler to only include  $+1.0\sigma$  and  $-1.0\sigma$  in the ensemble (and not  $0\sigma$ , or other intermediate increments). Consequently, the authors are underestimating most of their uncertainty sources.
- 2) The wind speed calibration coefficients should not have an uncertainty of 5%. I also don't see any justification for such a small value. Did it come from 1.88 versus 2 on L172, which is a 6% difference?

Fig. 1: Please add the dates to the caption for the two reflectance images. Latitude and longitude tick marks would be of interest to the readers.

L107: The linear calibration coefficient varies strongly between Landsat and Sentinel-2. I wonder if these studies are even relevant? Was the reflectance from the bubble monolayer or the multiple-bubble layer used in the Whitlock et al. study? Do they give the same ratio? I could not access the Koepke paper (but the reference is correct). The authors should note that this reflectance ratio should be roughly the same from space and at the ground because the atmosphere is optically thin. On second thought, is (background) methane a strong enough absorber to affect the ratio (satellite versus ground-based)? Whitlock et al. used a ground-based radiometer.

L132: "through" seems incorrect and terse. I suggest "via the use of"

L151: "mask" could be deleted for simplicity

L172: This linear regression equation has a much different slope than the one in Fig. 4 of Varon et al. (2018). Was  $\log(U_{10})$  also tried? There should be more discussion of why the effective wind speed should exceed  $U_{10}$  for this foamy setting. I think a 5% uncertainty (L197) on the effective wind speed relationship is a gross underestimate. Given the effective wind speed equation on L157, the authors should greatly expand the magnitude of this sixth source of error/uncertainty, maybe by an order of magnitude.

L173: What is the relevance of the uniformity of the ventilation?

L189: Why include four speeds and perturb each of them by 50%? The authors might actually be overestimating this source of uncertainty, since L193-L195 show that the wind speed range is not that large, especially on Sept. 30<sup>th</sup>.

L198: I cannot reproduce the numbers.

L218: Re: "1M", is this 1 million? If so, please avoid the shorthand and I don't see why the random draws did not come from ~5 million members, but it might not matter. I simply need to know how the one million members were selected by the authors in order to assess whether this is a biased sample.

L236 (and in the abstract): hypotheses-> assumptions