Dear Editor,

In this manuscript van Ettinger *et al.* integrate a new Axetris methane sensor into a package for UAV deployment. They conduct extensive sensor testing and deduce that temperature is a key factor influencing measurements. They therefore use a heat coil to regulate cell temperature, which yields encouraging mole fraction results. They also conduct a water correction and calibration, which they presumably use to correct field sampling data, although this is not totally clear. They derive emission fluxes from both the Axetris and a simultaneously deployed AirCore (sampled by a LI-COR LI-7810), when conducting UAV sampling downwind of a cattle farm. Emission fluxes from both datasets showed good agreement, with the Axetris providing a far superior sampling frequency, resulting in a better spatial resolution. A simple mass-balance flux approach is used to derive fluxes, where measurements are weighted for averaging into spatial grid-squares and then integrated. An Ornstein-Uhlenbeck-based uncertainty analysis is conducted (modelling a single specific UAV flight in this work) to evaluate the importance of different sources of uncertainty, where wind speed variability and background assignment are identified as the most important factors, as expected. It is also found that higher emission fluxes result in lower uncertainties, as expected. This is a useful development in UAV methane sensor testing that will be of interest to readers of Atmospheric Measurement Techniques. This paper is very well written and easy to follow. However, there are a number of issues with the manuscript and a number of areas for improvement. I therefore recommend minor revisions before publication.

General comments

Perhaps the title can be a little more specific on the type of sensor being tested. Many *in situ* methane sensors have been tested so it might be nice to add some more details on which one, to emphasise the novelty of this work.

The authors have based their description of different UAV flux techniques in the introduction on a small number of references with limited newer citations. Lots of work has been done on testing different near-field flux quantification methods in recent years. In addition, perhaps some more recent UAV flux results could be included in the discussion section for uncertainty comparisons.

The linearity test is not really a linearity test. This section presents the results from a simple calibration, which is fine as a calibration. However, to evaluate linearity, one must evaluate the goodness of the linear fit. This may be quantified using the root-mean-squared error of the fit, for example. Without any indication of the quality of the linear fit, the linearity remains unknown. An identity line (or one-to-one) fit does not necessarily mean that an instrument is linear, as the data could average out to provide a gain factor of unity over the full sampled range, but different parts of the sampled range may deviate from the linear fit.

A water correction coefficient derived at approximately 81 ppm was applied to all sampling data. This seems like a strange approach. It is indeed a useful test to confirm that the coefficient remains constant over a large methane mole fraction range, but this is not useful to derive a single universal coefficient, for application to all methane mole fraction levels. It would be better to derive such a coefficient at an ambient methane mole fraction level, where most UAV sampling takes place. Unless the authors can confirm that the coefficient is independent of methane mole fraction, it should be re-evaluated at roughly 2 ppm, with the 2 ppm coefficient instead used.

A calibration conducted between 0 ppm and 2 ppm (according to Figure B1) was applied to UAV sampling conducted up to 3 ppm. It would make more sense to sample higher methane mole fraction levels during this calibration. Furthermore, the mole fraction range presented in Figure B1 is inconsistent with the description of the experiment provided in the text, which instead suggests that sampling was conducted up to 81 ppm. This is clearly not the case from Figure B1.

What is missing from this manuscript is a comparison of emission flux estimates derived using raw measurements versus those with a water correction and calibration applied to them. The authors claim that the laboratory testing helped to improve their measurements, but it would be interesting to evaluate the importance of this effect on final emission fluxes. Perhaps, the Axetris data does not require any supplementary correction and raw measurements would still be competitive with the AirCore (provided that the heating coil is used).

This manuscript presents a comparison in methane emission flux estimates using AirCore data and Axetris data. The most logical way in which to test the effectiveness of a sensor would be to compare mole fraction measurements directly, as identical mole fractions would result in identical fluxes. This alterative sensor comparison approach must be clearly justified throughout the manuscript. I think I understand the reason. The AirCore data is smoother and a background must be derived in a different way. But please make this clearer.

This paper then compares flux uncertainties from this work to flux uncertainties from other studies, as a means to evaluate the sensor performance. This is incorrect as there are many factors determining flux uncertainties, as the authors themselves show in this work. This comparison to other studies instead allows the overall UAV flight strategy to be evaluated, including the sensor as a component of the overall sampling framework. This is useful in the context of evaluating the overall the UAV flux quantification strategy, but cannot be used to draw conclusions on the quality of the sensor.

A key advantage of the Axetris is that it offers a higher temporal resolution than the AirCore. It may actually be better than the AirCore in general, if the appropriate laboratory-derived corrections are applied and a heating coil is used. This may be tested with a controlled release in future work. It would be interesting to see. Maybe the authors can include this thought in the discussion section, if they agree.

Specific comments

Line 14: "accurate quantification of its emissions is critical for mitigating climate change"

- Strictly speaking, quantifying methane emissions does not mitigate climate change. It allows for the improvement of climate change mitigation strategies.

Line 16: "in situ CH4 sensor (Axetris)"

- Please provide details on which specific sensor model is used. This is an important detail in the abstract.

Line 19: "provided a water vapour correction term"

- How can the tests "provide" a water correction "term"? Does this mean that the authors devised a water correction by conducting laboratory testing? I don't think it is right to say that the tests provided a "term". Perhaps a water correction was applied to raw measurements using a laboratory-derived water correction coefficient. But that is not clear from what is said here.

Line 21: "This mean flux was comparable to the value of 4.2 ± 1.1 gCH4/s obtained from the established AirCore technique."

- Does this mean that the AirCore sampled simultaneously to the Axetris on a UAV?
- It seems a bit strange to compare fluxes when evaluating the performance of the sensor. It would be better to compare direct mole fraction measurements. The flux technique is irrelevant. If the two sensors provide identical mole fraction measurements then they will inevitably provide identical fluxes. Some justification is required.

Line 22: "Ornstein-Uhlenbeck method"

- Maybe a few short words on this method could be good here, but I understand if the authors prefer to avoid this here.

Line 33: "1942 ppb"

- This is quite precise, considering it represents mole fraction for the entire planet for a full year. Maybe it is better to say 1.9 ppm. But it's fine if the authors insist.

Line 35: "Saunois et al., 2020"

- There is a new methane budget paper: https://doi.org/10.5194/essd-17-1873-2025. It is better to cite and use this instead of or in addition to the 2020 paper.

Line 37: “climate mitigation”

- This doesn’t make much sense (mitigating the climate). I thing the authors mean to say that reducing emissions is essential to mitigate climate change. “Climate change” and “climate” do not mean the same thing.

Line 43: “underreporting”

- There can also be over-reporting (or double-counting) taking place.

Line 44: “scarce”

- This is a strong word. While it is true that data can be scarce in places, some regions on Earth have dense sampling networks.

Line 48: “exhibit complex emission patterns with substantial diurnal and seasonal variability”

- Some citations are required here to support this statement.

Line 65: “max”

- Please replace this with “maximum”.

Line 66: “relatively low implementation cost”

- UAVs may be cheaper than aircraft, but not necessarily cheaper than ground-based vehicular sampling. This statement should be qualified.

Line 72: “these techniques”

- Which techniques is this referring to? The previous sentence refers to three “deployment strategies”. This next sentence is describing how sampling acquired from any of the three deployment strategies can be used to derive a methane emission flux, which seems like a logical order to present this information.

Line 73: “they all rely on the mass balance approach (MBA; Nathan et al., 2015; Vinković et al., 2022) or the inverse Gaussian approach (IGA; Andersen et al., 2021) for flux quantification”

- This seems like a major oversimplification. All methods are effectively a type of mass balancing as they compare downwind enhancements (due to source emissions) to an upwind background. Both approaches subtract a downwind plane from an upwind plane. The difference is how measurements on the downwind plane are integrated. Gaussian plume modelling typically applies Gaussian statistics to downwind sampling to obtain a smooth sampling plane (hence yielding an inverted flux). By contrast most other so-called simple “mass-balancing” approaches do not apply Gaussian statistics and simply spatially interpolate available measurements or average them into grid squares (yielding a flux through spatial integration). I would therefore argue that Gaussian plume inversions are a more advanced approach to (or subset of) mass-balancing.
- I would recommend replacing this statement with the something like “Although these deployment strategies differ operationally, mole fraction measurements from any approach rely on mass balancing to derive a flux, where downwind sampling is either spatially integrated (following spatial interpolation or grid-square averaging), or modelled using a Gaussian plume inversion.”.

Line 75: “it can directly convert atmospheric concentration measurements into fluxes”

- How is the so-called MBA method more “direct” than IGA approach? Both methods require some computation to convert mole fraction into flux. So I disagree with the idea that one approach is more “direct” than the other.

Line 79: “20% to 75%”

- Multiple citations are required here to support this.

Line 80: “non-uniform sampling”

- What does this mean?

Line 86: “Tuszon et al., 2020”

- I can’t find this in the reference list.

Line 86: “Allen et al., 2019”

- Allen *et al.* did not conduct any sensor development. They tested pre-existing commercially available sensors.

Line 87: “high cost and maintenance demands”

- High precision sensors do not necessarily have higher maintenance demands. Quite often, they are easier to maintain.
- It may be worth noting here that better sensors are often heavier, potentially resulting in reduced flight endurance.

Line 88: “low-cost”

- According to my personal understanding a low-cost sensor is less than 100 €. So I would not call an Axetris low-cost, in my opinion.
- A rough cost range should be given here.

Line 88: “medium-precision”

- What does medium-precision mean? I think the authors should provide a rough parts-per-million range here.

Line 92: “low-cost”

- See the comment above. It is fine to call this sensor “low-cost” if the authors insist (although I advise against this), but then a rough cost range definition of “low cost” should also be provided in the manuscript.

Line 94: “minimise drift”

- Laboratory testing cannot be conducted to minimise drift. It is only possible to measure drift or to conduct testing to “account” for drift. But the sensor will naturally drift regardless of whether it is tested in the laboratory or not.
- The authors add a heating coil to the sensor, which improves stability by reducing temperature sensitivity. However, this is not a part of laboratory characterisation testing. This is instead hardware development and sensor optimisation.

Line 102: “in situ CH4 sensor”

- It might be nice to introduce the Axetris here and discuss the sensor technology here (*i.e.* the second sentence of subsection 2.1). This would make this subsection seem a little less abstract. But it is fine if the authors want to leave it as it is.

Line 113: “We developed a UAV-mounted sensor package”

- Consider including an annotated photograph of the sensor package (highlighting the inlet and outlet). This may be included as a subfigure of Figure 1.

Line 120: “the interface PCB”

- I don’t follow this. How can the Axetris report the interface PCB on which it is mounted?

Line 131: “ground time”

- What is the ground time?

Line 137: “PID”

- This acronym has not been defined.

Line 144: “Figure 1”

- What does “REF” stand for in this figure? Is it defined in the main text?

Line 162: “short-term drift”

- Was pressure and temperature controlled when making this assessment, with sufficient warm-up time?

Line 166: “Linearity of the Axetris”

- This subsection (in its current format) does not provide an evaluation of the linearity of the measurement, but rather provides a calibration of the instrument. A linearity test shows whether the increase in one measurement is consistent with increase in a second measurement, following a linear regression. The magnitude of any derived linear coefficients is independent of the standard of linearity.

Line 167: “high CH4 concentration (~81ppm)”

- If this is the case, why is Figure B1 plotted between 0 ppm and 2 ppm? Why not plot it up to 81 ppm?
- If a high concentration was diluted with ambient air, how was 0 ppm (lower than the ambient background) achieved in Figure B1?
- Where did this high concentration gas come from? Was a specially prepared gas blend purchased? If so, did the authors consider the presence of interfering trace gases in the overall gas matrix?

Line 168: “ambient air”

- Where did the ambient air come from? Was a compressed gas cylinder used? If it was natural laboratory air, can the authors be sure that is was not contaminated?

Line 169: “LI-7810 system linearity”

- Please make it clear that this linearity was tested for a different LI-7810 to the one being used in this work. Linearity results from a previous study for a different unit are being assumed to be valid here.

Line 171: “wide range of concentrations”

- Please provide the concentration range here (assuming this is referring to the ICOS testing).

Line 178: “The slope confirms linearity between the two systems”

- Calculating a gain factor value for the slope of a linear regression does not provide an indication of the linearity of the fit. A gain factor of exactly 1 or a gain factor of 100 can both be derived from perfectly linear data, assuming all of the data points follow a straight line. Linearity can instead be evaluated by the root-mean-squared error of the fit, indicating that the gain factor (regardless of its magnitude) remains valid over a large mole fraction range.

Line 184: “significantly”

- This word is unnecessary. Sometimes the effect is large, sometimes it is small.
- A dilution effect is, by definition, only as large as the mole fraction of water. As water mole fraction is typically around 1% (and no more than around 3%), it is hardly “significant”.

Line 191: “high concentration of CH4 (~81 ppm)”

- Why use a high concentration source? It would make more sense to characterise the water effect at an ambient methane mole fraction. The UAV will rarely sample at 81 ppm, so it seems strange to obtain a universal water correction coefficient for extreme sampling conditions.

Line 195: “1.5% – 61 % at 25 ºC”

- At which pressure were these values derived? I don’t think relative humidity is important here, but if the authors insist in including it, they should include the pressure that this corresponds to.

Line 199: “Chen et al. (2010)”

- Is this citation necessary? In this work, the wet and dry data is simply compared and a linear fit is applied to it. It’s not really a new “method” devised by Chen *et al.*. Furthermore, Chen *et al.* used a polynomial fit, while a linear fit is used in this work.

Line 207: “Chen et al., 2010”

- Rella *et al.* (https://doi.org/10.5194/amt-6-837-2013) should also be cited here.

Line 212: “With this, we assume that the non-dilution part of the water vapour correction remains the same at ambient levels as at high concentrations.”

- But the spectral interference and overlap between the water and methane peaks can change depending on the methane mole fraction level. Methane affects the water peak and *vice-versa*. So, the most standard condition in which to evaluate the influence of water mole fraction variability is at ambient methane mole fraction levels of 2 ppm (see Figure 2b in Chen *et al.* (2010), for example).

Line 218: “System precision”

- Was the data used for this test calibrated and water-corrected? This is especially important for the UAV precision test, where varying water mole fraction may have influenced instrumental measurements.

Line 222: “(e.g., the Axetris)”

- Is this necessary?

Line 229: “observed”

- Where were these observations made? Were these measurements from the Axetris cell or from somewhere in the ambient laboratory?

Line 230: “UAV's precision”

- Please rephrase this. The UAV precision is not being measured but rather the sensor precision during UAV deployment.

Line 231: “three consecutive flights”

- Please clarify that an Allan variance test was applied to each flight individually. This is an important detail, as it would otherwise be incorrect to merge different datasets into a single Allan variance analysis.

Line 241: “To maintain measurement precision below 10 ppb, calibration every hour is required.”

- I do not understand the basis of this statement.
- The minimum Allan deviation is 60 ppb, at 1 Hz. So how can a precision of less than 60 ppb (*i.e.* 10 ppb) possibly be achieved?
- How is this time of 3600 s between calibrations derived from this Allan variance test?

Line 246: “without additional temperature control”

- Please provide some temperature measurements (and standard deviation) values here please for the three UAV flights.

Line 249: “Figure 2”

- The title for the horizontal axis title should either be the correct Greek symbol (τ) or be replaced with the text “averaging time”.
- It might make more sense to change the colour of subplot b and then to use consistent colours in subplot d. Otherwise it is confusing because different colours mean different things in the four subplots.

Line 287: “~100 minutes; Figure 2c”

- I don’t see this.

Line 293: “Both instruments were deployed”

- Please clarify here whether they were both deployed simultaneously or if they were deployed individually for separate flights.

Line 298: “dairy cow farm”

- It would be good if site coordinates can be included here (although I understand if this is not possible).

Line 300: “The farm was”

- Please replace this with “Emissions from the farm were”.

Line 306: “senor package was mounted”

- But where was the air inlet? Was the air inlet also below the plane of the propellers? If so, has the influence of downwash been considered?

Line 314: “drone”

- Replace this word with “UAV”.

Line 316: “altitude”

- Please replace this with “height above ground level”. Altitude generally refers to a measurement with reference to mean sea level.

Line 324: “as the average the”

- Please replace this with “as the average of”

Line 324: “during the duration”

- Consider replacing this with “for the duration”.

Line 325: “height”

- Please replace this with “height above ground level”.

Line 354: “shifting the assigned timestamps of the project AirCore data”

- Was the lag time through the air inlet of the Axetris accounted for?
- Were Axetris measurements shifted to the AirCore time-stamp or *vice-versa*?
- The best way to adjust concentration measurements is to ensure that they correspond to the time stamp of geospatial position (with lag time and time lag accounted for). Shifting mole fractions so they agree with each other, rather than adjusting them independently to geospatial position measurements, seems suspicious.

Line 364: “altitude”

- Please replace this with “height above ground level”. Altitude generally refers to a measurement with reference to mean sea level.

Line 369: “complete”

- Remove this word.

Line 376: “Spatial interpolation”

- This work presents a very sensible approach to spatial integration. Perhaps it can be alluded to in the abstract or introduction. This approach is much better than geospatial kriging or simple grid-square averaging, with no weighting.

Line 378: “observed CH4 enhancements were interpolated onto a grid”

- A fundamental requirement of this approach is the assumption that the emission plume does not move during sampling. This method assumes that turbulence does not change the location of the plume and that the wind direction does not change. Please discuss this here as a caveat to this approach.

Line 397: “Grid cells with no contributing observations (i.e., beyond the cutoff radius of the nearest measurement) are assigned missing values (NaN) and excluded from subsequent analysis.”

- An inevitable consequence of this is that if a grid-square is under-sampled, but it potentially contains parts of the emission plume, this will inevitably result in flux underestimation. Please comment on this in the main text.
- Unless the authors can be sure that each grid square containing flux emissions has been sampled, the entire plume will not be captured, resulting in flux underestimation following this approach.

Line 401: "spatially interpolated CH_4 concentration"

- Please describe exactly which corrections were applied to the data. Was a calibration and water correction applied, as alluded to in section 2? Perhaps this detail can go somewhere else in this section. But it needs to be stated clearly somewhere.
- I am not totally sure if the sensor data is calibrated and water corrected or not. It should be totally clear if the corrections from Section 2 are used and how.

Line 408: "Δx and Δz"

- Please provide the exact size of these values chosen for this work.

Line 411: "Mvol gives the molar volume"

- So this equation actually provides molar volume in dry conditions (excluding all water molecules), as the partial pressure of air in dry conditions is used here. This is very nice to see and often overlooked in most research. I am glad that the authors have included this detail. However, this only works if a water correction is applied to raw mole fraction measurements to obtain dry mole fractions. This really must be clarified throughout the manuscript.

Line 437: "The averaging background method is not ideal for the Active AirCore due to its intrinsic signal smoothing (Morales et al., 2022)."

- Please can a bit more explanation be provided here? I don't really understand why the Aircore data cannot be used in the same way as the Axetris data. This is an important point.

Line 450: "the integral of raw and smoothed Axetris data is identical"

- What exactly does this mean? Does this mean that adding up all of the numbers in the smoothed Axetris and Aircore time series is identical?
- Please provide the result here, showing how identical this result is. I think "identical" is a strong word, unless it can be shown that it is exactly the same.

Line 460: "in subsequent analysis"

- Please replace this with either "in subsequent analyses" or "in the subsequent analysis".

Line 481: "Figure 3"

- What size are the grid cells? I think this detail needs to be included here and in the main text. It is difficult to see any grids in this image.
- What do the black contour lines represent? If this data is averaged into grid squares, there shouldn't be any continuous contour lines.
- The green and purple colours in panel a are impossible to distinguish. They both look grey to me. I suggest using two alternative bolder colours.
- How was sampling conducted so close to ground level with a UAV? What was the minimum sampling height (excluding take-off)? Perhaps provide this detail in Table 1.

Line 535: "θ is the mean-reverting term"

- I would recommend using a different symbol here, as θ is already used in section 3, to describe the flux quantification method.

Line 541: "drone"

- Replace this word with "UAV".

Line 457: "4D"

- Please define this acronym.

Line 555: "using a typical flight trajectory from the previous field campaign"

- This means that this analysis is only valid for the type of sampling conducted in this specific UAV sampling campaign. The sampling extent and sampling spatial density are specific to flight 1 in this work. Different uncertainty analysis results may occur if using a different UAV piloted in a different way. I think this point needs to be explicitly made in this section.

Line 573: "Flight duration"

- This analysis requires the flux, wind conditions, atmospheric turbulence and position of the plume to remain constant throughout the full duration of each of the six flights. In reality the plume will be influenced by all of these things. Therefore, I am not sure how useful the conclusions of this subsection are in the real world.

Line 596: "Figure 4"

- Why not combine Figure 4 and Figure 5 into a single figure with four subplots?

Line 642: "improved spatial resolution the nominal plume"

- Consider rewriting this, as I don't follow the text.

Line 663: "standard deviation of ± 40%"

- How does this compare to the standard deviation between the four UAV fluxes from this work? It is interesting to compare here, in this subsection?

Line 690: "a persistent uncertainty of approximately 20%"

- But this 20% minimum threshold is specific to the UAV sampling strategy in flight 1. If sampling over a greater area or with a higher spatial density, this uncertainty threshold may be lower. It is worth making this point.

Line 693: "cost-effective CH4 sensor"

- How much does the Axetris cost? What is the total cost of the finalised UAV enclosure, in terms of raw materials? The authors cannot make such a statement unless they provide at least a rough idea of how much their system costs.
- How much does the AirCore cost for comparison? This also requires a LI-7810 so the net cost is inevitably higher, for this work.

Line 697: "exceeds 39%"

- Why say that it exceeds 39%? It is better to just provide the actual value.

Line 700: "The results"

- Please specify which results are being referred to here.

Line 702: "mid-cost sensor"

- In the introduction it is called a low-cost sensor. The cost of the sensor is not consistent, even in this manuscript.
- It is better to provide the cost (even roughly) and stick to either mid-cost or low-cost, with a defined price range for the chosen term.

Line 702: "the Axetris sensors uncertainties are similar to those found during prior UAV-based studies (Andersen et al., 2021; Karion et al., 2013; Morales et al., 2022; Nathan et al., 2015) using higher precision and accuracy sensors."

- I disagree with the way the sensor uncertainty is evaluated here. This statement does not represent the sensor uncertainty. It represents the uncertainty in fluxes derived using a specific sensor with a specific UAV sampling strategy. As the authors show in Figure 6, the flux uncertainty in a function of the emission flux rate. Unless all of the previous papers cited in the sentence sampled the same flux rate with exactly the same UAV sampling strategy (and identical atmospheric conditions), this comparison cannot be made. This is not a way to evaluate sensor performance as it conflates other factors specific to each sampling campaign.

Line 732: “creating favourable conditions for both IGA and MBA”

- As mentioned in the introduction, I don’t think mass balancing and Gaussian plume modelling can be split into two totally different approaches, as in this paragraph. Both methods are a form of mass balancing as they compare upwind measurements (assumed background) to downwind plume enhancements. The key difference is that Gaussian plume modelling attempts to model a continuous plane from available sampling using Gaussian statistics whereas simple “mass-balancing” integration methods do not. These simple mass-balancing methods either spatially interpolate available data or average the data into a grid, as in the method presented here.
- All flux methods (including Gaussian plume modelling) are affected by wind speed. This is not exclusively a problem for the grid-square averaging integration method presented in this work. This paragraph may therefore seem to be misleading.

Line 739: “making appropriate the use if a method that does not rely on Gaussian plume assumptions.”

- I disagree. If enough sampling is conducted, Gaussian plume methods can also deduce the average emission flux, as a turbulently moving plume and the UAV randomly intersect. These random intersections should average out over time. It certainly isn’t better for a non-Gaussian approach, such as for the grid-square averaging approach in this work. The assumption in this work is that each grid square will include a statistical sample of the moving plume as well as the background.

Line 753: “in situ CH_4 sensor”

- I think the sensor should be named here. Otherwise it is a bit of an empty statement.

Line 761: “lab”

- Replace this with “laboratory”.

Line 768: “the close agreement”

- Please add a sentence here stating that different background methods were used for the difference sensors. This is an important detail, to emphasise that the data was not processed in the exact same way.

Line 768: “robust”

- Consider the implications of this strong word. A 20% uncertainty may not be that robust for some.

Line 778: “Multiple flights”

- It is important to include the caveat here that this requires the emission flux to stay the same and atmospheric conditions to also stay exactly the same.

Line 848: “https://icos-atc.lsce.ipsl.fr/docs”

- This link does not seem to work.

Line 933: “Figure B1”

- It is really difficult to distinguish the line from the points. Perhaps make the line bright red with small black dots for data.

Line 954: “drone”

- Replace this word with “UAV”.

Line 961: “drone”

- Replace this word with “UAV”.

Line 968: “drone”

- Replace this word with “UAV”.

Line 974: “drone”

- Replace this word with “UAV”.

Line 980: “drone”

- Replace this word with “UAV”.