1 Introduction

Dear Editor, Reviewers,

This document contains the author responses to the reviewer comments to 'The ICOS Ecosystem Station Loobos: a pine forest site exposed to atmospheric pollution' submitted to Earth System Science Data (https://essd.copernicus.org/preprints/essd-2026-99/). Before all, we thank the two reviewers for the time and effort they put into reading the submission, considering its quality and giving constructive feedback. We are especially thankful for the many comments to improve the paper beyond what was already written. Because we are confident that we could satisfactorily address all comments, we prepared a revised manuscript, in which we use the reviewers' comments to improve the originally submitted paper. Below we give our responses to the reviewers' comments and explain how we take them into account in the revised manuscript.

For readability, we put the reviewer comments in italics and *blue* and our responses in normal black text. When referring to line numbers, we added the new line numbers in the revised paper including the track-changes.

Michiel van der Molen,

17 August 2026

also on behalf of the co-authors.

2 Response to 1st reviewer comments

(https://doi.org/10.5194/essd-2026-99-RC1)

This paper is organized and surely planned as the future reference paper for the ICOS class 2 station NL-Loo. It contains an extensive description of the core Eddy Covariance as well as the many ancillary measurements required for an ICOS station of that class. This part is written in a fashion mainly documenting what instrumentation is present and where exactly (e.g. measurement heights along the tower), and is rather standard in the ICOS community. It is possible that this rather conventional way of presenting and documenting an ICOS site in ch. 1.2 to 2.5 is impacted by the use of the Copilot.

Thank you for your appreciation of the structure and contents of the manuscript. The manuscript is indeed loosely based on the ICOS labelling report (I do not think these are publicly available). But we restructured it considerably and added much extra information in this manuscript.

Line 785 contains our AI statement, where we claim that we used MS Co-Pilot to better structure parts of the text. This refers to section 4 – Outlook, we will make this more explicit in the revised text. We did not use AI in any other part of the text. As Dutch scientists, we operate according to the Netherlands Code of Conduct for Research Integrity (https://www.nwo.nl/en/netherlands-code-of-conduct-for-research-integrity) and we certify the AI statement and we take full responsibility of all text in the manuscript.

The Introduction provides the history of the site both from a paleoclimate as well as a management perspective. While not needed properly for the description of the measurement *program, it is an interesting story demonstrating that the dunes, the podzolization of the soils and the pine forests growing there now is a result of specific landuse conditions and decisions previously made. The site is rather homogenous over quite some spatial distances and represents a forest not very typical for the Netherlands, as can be seen e.g. through the phenocam. NB at the time of writing this review, the phenocam had no updated imagery for almost a month. Beyond the daily delivery to the CP, the sites runs its own visualization website with updates every 30 mins for many of the variables.*

You write '*The site is rather homogenous over quite some spatial distances and represents a forest **not** very typical for the Netherlands, as can be seen e.g. through the phenocam. NB at the time of writing this review, the phenocam had no updated imagery for almost a month.'*

We think the 'not' in 'a forest **not** very typical for the Netherlands' is a typo? Pine is the most frequent tree species in Dutch forests (28.1%, Schelhaas et al., 2022, table 6.1) and planted forest with a uniform age represent 49.5%, table 4.1 'gelijkjarig bos'). The phenocam as well as the tree inventory (section 1.2) and airborne photography indeed shows that Loobos consists of a vast majority of pine trees.

Our submission of the phenocam data repository to the Northern Arizona Phenocam Network has indeed been discontinued, because ICOS has taken over the data repository for European sites. The data can be viewed via https://data.icos-cp.eu/portal/#%7B%22filterCategories%22%3A%7B%22station%22%3A%5B%22iES_NL-Loo%22%5D%2C%22type%22%3A%5B%22etcPhenocamDaily%22%5D%7D%7D. As far as I understand ICOS is still in the process of taking over the storage of incoming data and collecting the historical data. In the meantime, the historical data can still be viewed on https://phenocam.nau.edu/webcam/sites/nlloobs/. We will update the text in section 2.2.

Up to and including ch. 2.6, the manuscript is a standard documentation of an ICOS class 2 site in a non-spectacular manner. However, from 2.7 onwards, it offers new perspectives on the feasibility of flux (rather than only concentration) measurements for VOC and ozone, clearly the highlight of the paper: the site has non-ICOS equipment installed and thus observations available which are rare, if not unique within the set of ICOS ecosystem sites. The authors spend a significant amount of effort into the quality assessment of these non-standard fluxes, not the least since the VOC concentrations are obtained at a much lower sampling frequency compared to H2O, CO2 and T. Spectral analysis is used to convince the reader that for the VOCs, 1 Hz measurements are sufficient. The spectra reveal, however, a high-frequency noise part which is surprisingly increasing with frequency (or rather, the product of frequency times spectral power is increasing), in stark contrast to H2O, T, and CO2 which are closer obeying Kolmogorov's law or at least show a power-law attenuation at high frequencies. This should be the topic of further investigation.

Thank you for noting the division in ICOS and non-ICOS measurements. We intentionally made this separation in order to avoid confusion. Here, we want to make the point that the VOC and O_3 concentration can in principle be used in eddy covariance to calculate fluxes. However, the entire data processing deserves its own paper and we are indeed working on two dedicated papers for O_3 and VOC's, where we will describe data processing, calibration, the effect of rotation, spike detection and removal, trend removal, etc. in detail.

Chapter 3 has a focus on additional campaigns or experiments indicating the potential of NL-Loo to become an ecosystem research hub interesting for a broader scientific community than only ICOS. This ambition is confirmed by the Outlook chapter.

The paper is well organized as a site reference. Still, the annotated pdf attached to this review contains 63 detailed comments pointing to some smaller errors or improvements which can be made, which, however, accumulate to only minor revisions. After implementing them, the paper should be acceptable for publication in ESSD.

We list the annotations below and describe how we revised the manuscript accordingly.

2.1 Changes to the manuscript:

Line 785 (now 949): Replaced 'We used Microsoft Copilot to help better structure parts of the manuscript.' by a AI statement section (now line 950):

AI Statement

We used Microsoft Copilot to help better structure section 4 of the manuscript. We did not use AI to write or rewrite other sections. We take full responsibility for the text in the full manuscript and supplements.

Annotations

Comments about figure references

Line 45 (now line 53): Fig. Figure 1 → *Fig. 1*

Line 55 (now line 58): Fig. Figure 2 → *Fig. 2*

Line 74 (now line 76): Fig. Figure 2 → *Fig. 2*

Line 74 (now line 77): Fig. Figure 2 (2nd time) → *Fig. 2*

Line113 (now line 116): Fig. Figure 3 → *Fig. 3*

Line 158 (now line 183): Fig. Figure 5 → *Fig. 5*

Line 161 (now line 185): Fig. Figure 6 → *Fig. 6*

Line 173 (now line 197): Figure Figure 7 → *Fig. 7*

Line 247 (now line 274): Fig. Figure 8 → *Fig. 8*

Line 338 (now line 373): Figure Figure 9 → *Fig. 9*

Line 372 (now line 413): Figure Figure 10 → *Fig. 10*

Line 495 (now line 536): Fig. Figure 12 → *Fig. 12*

Line 511 (now line 551): Fig. Figure 13 → *Fig. 13*

Line 516 (now line 557): Fig. Figure 13 → *Fig. 13*

Line 518 (now line 559): Fig. Figure 12 → *Fig. 12*

Line 618 (now line 660): Fig. Figure 14 → *Fig. 14*

Line 620 (now line 662): Fig. Figure 14 → *Fig. 14*

Line 675 (now line 723): Fig. Figure 15 → *Fig. 15*

Comments 1-18 should be corrected by MS Word automatically. We have no clue how these incorrect references appeared, but we are sorry for missing them in the final proof.

Line 24 (now line 24): is "nature area" a term well defined in the Netherlands? Otherwise, since any spot on Earth is part of "nature" (NB NOT: natural), it doesn't make much sense to me. It could be a nature reserve, protected area, national park etc., but this is apparently not the case?

The Veluwe area a loosely defined region in the central part of the Netherlands. It is largely covered by forest and heather fields. It contains the largest Natura 2000 area on land in the Netherlands (https://www.natura2000.nl/gebieden/gelderland), while there are villages, roads, patches of agriculture and small scale industry too. That makes it somewhat difficult to catch in one term. We thought the term 'nature area' would describe it well enough for the purpose of this paragraph.

Line 50 (now line 53): in the main text, it says "Saalian" - which one is correct?

You are correct, the area was shaped predominantly by the Saalian ice age. The Weichselien ice age affected the area too, but the ice wedges did not reach the Netherlands at that time. We corrected this. (Line 50: Weichselien → Saalian)

Line 83 (now line 86): 'For centuries, citizens and governments fought the wind erosion and sandstorms, unsuccessfully': in which sense? They failed to turn it back into an agricultural area? To plant a sustainable forest is a success

No, they fought the wind erosion and sandstorms. They did this by building wooden structures, hedges, planting trees. But until the early 1900's this approach was unsuccessful. (One of the main reasons was that the inhabitants used the wood for heating in the cold winters of the Little Ice Age).

Line 88 (now line 91): WOII → WWII:

We corrected this.

Line 104 (now line 107), remove comma after 2010

We corrected this.

Line 104 (now line 106): '(86 on average)': which is a bit in conflict with a planting year of 1911

Yes, this is correct and also a bit puzzling. Based on story telling from local inhabitants, the planted trees did not grow much in the first decades. The forest service says that no thinning has taken place (officially), although it could be that off-the-record harvest took place during WWII. However, if the trees were still small, this is not very likely. Perhaps some trees have regenerated, which could explain this observation. Since we do not know how to explain it, we choose to mention the observation, but not attempt to explain it in this paper.

Line 133 (now line): this is not a square but a general quadrilateral

Yes, correct. Well, it accidentally represents the projection of a square with a non-zero viewing zenith angle in Google Earth. We replace 'square' with 'box'.

Line 141 (now line 138), Table 1 caption: The whole table caption should be reworked. According to it, there is no difference between minor and major diseases (or damages??), and "damage" is explained two times with identical wording.

The ICOS instructions for Ancillary measurements in forests (https://www.icos-etc.eu/icos/documents/instructions/ancfor) require the 'health status' of individual trees to be recorded during tree inventories. The health status has 6 categories: healthy, minor diseases, major diseases, dead, fallen, removed. The last one did not occur in Loobos. The categories healthy, minor and major diseases depend on 'damage', i.e. branches that have lost leaves or where leaves have changed colour and how much the trees are affected (no damage, less than 50%, more than 50%).

To make this clearer, we simplified the caption of Table 1 from:

'Table 1. List of species observed in the 2023 inventory in the CP's and SP's (Fig. Figure 4) and their health status. Here 'Healthy' is defined as no visual indication of damage, where 'damage' is defined by branches that have lost leaves or where leaves have changed colour; The crown is affected by less than 50%. 'Minor diseases' as visual indication of the crown being affected by diseases, herbivory, storm damage. 'Major diseases' as visual indication of the crown being affected by diseases, herbivory, storm damage. Damage is defined branches that have lost leaves or where leaves have changed colour. The crown is affected by more than 50%, but healthy leaves are still present. 'Dead' trees are the ones still standing and 'Fallen' are the ones fallen over. These definitions are according to the ICOS Instruction for ancillary vegetation measurements in forest (2021).'

To:

'Table 1. List of species observed in the 2023 inventory in the CP's and SP's (Fig. Error! Reference source not found.**) and their health status.'**

And we added the following text to section 1.2.2:

The 'health status' has 6 categories:

- 'Healthy': no visual indication of damage, but healthy leaves are still present;
- 'Minor diseases': visual indication of the crown being affected by diseases, herbivory, storm damage. The crown is affected by less than 50%;
- 'Major diseases': visual indication of the crown being affected by diseases, herbivory, storm damage. The crown is affected by more than 50%;
- 'Dead' trees: are the ones still standing;
- 'Fallen' are the ones fallen over.
- 'Removed': this category did not occur in Loobos.

Here 'damage' is defined as branches that have lost leaves or where leaves have changed colour. These definitions are according to the ICOS Instruction for ancillary vegetation measurements in forest (2021).

Line 142 (now line 173): there is no "damage" category in this table

This has been addressed in the response to the previous comment.

Line 151 (now line 176): 'managed and was found to be encroaching the area from about 2000 onwards.': Repetition from above

We could not find this repetition. There is one earlier mention of Prunus, but in a different context.

Line 151 (now line 177): 'Recently, ...': i.e. after 2023? The inventory shows only 3 dead trees

Yes, correct. The area with dead Prunus shrubs is patchy and Prunus dieback did not occur in the CP's. It occurred in some of the SP's, but this is indeed not reflected in the number of dead trees, because the diameters of the dead shrubs were less than 5 cm, which is the lower limit for being recorded in the inventory.

Line 171 (now line 195), Figure 6: please explain why SP-I_03 has the highest tree density by far (almost a factor of two) but its basal area is completely average. Are there large amounts of very young / small trees? Why only there?

Yes, you are right. SP-I_03 was in an area where there were lots of thin trees several meters tall. We don't know what causes the thicker trees to be absent, but we hypothesize that windthrow has occurred here.

Line 183 (now line 208): 'that the canopy height is more homogeneous than the surface topography': we don't see this from Table 2 - in which sense do you mean "more homogeneous"

The tree height is almost inversely proportional to the small-scale topography height (See figure below). So trees on top of dunes are less tall than the ones in valleys. We concluded this from airborne lidar measurements.

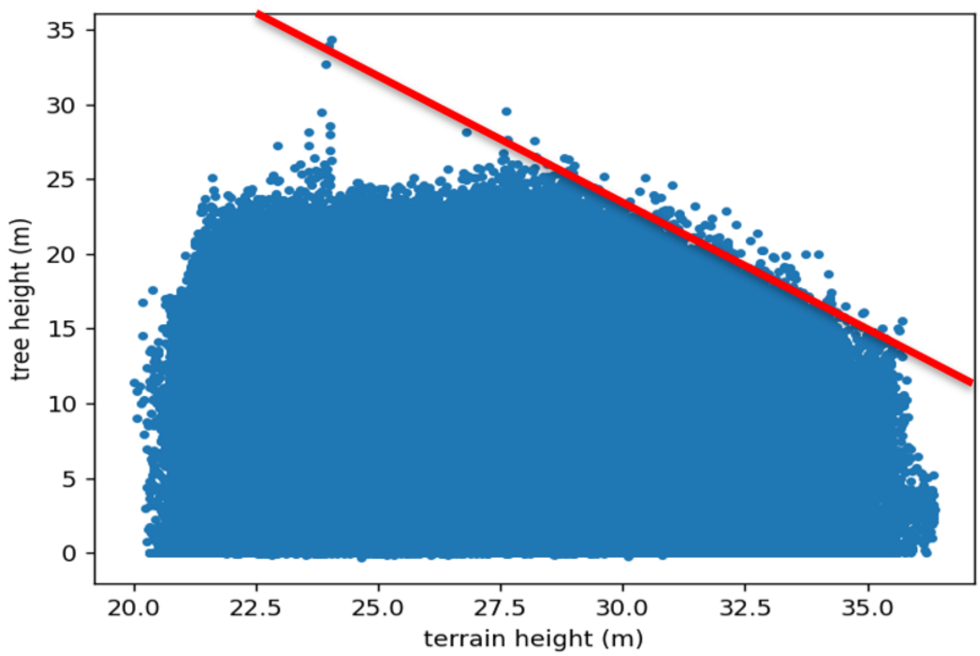

Figure: Tree height as a function of terrain height measured by airborne lidar (AHN4). Three height refers to the height where laser beams were reflected, and since the top of the canopy is not 100% closed, some laser beams are reflected by branches, needles and stem below the canopy top. The upper envelope thus represents the height of the tree tops, which on higher dunes (> 27.5 m) is inversely proportional to the terrain height.

In order to clarify this, we rephrased the sentence from:

'It must be noted that the canopy height is more homogeneous than the surface topography, suggesting that the tree height is inversely related to the dune height, i.e. trees on dunes are shorter than trees in valleys between the dunes.'

into:

‘It must be noted that the top of the canopy is smoother than the surface topography, because the tree height is inversely related to the dune height, i.e. trees on dunes are shorter than trees in valleys between the dunes based on airborne lidar measurements conducted in 2022.’

Line 189 (now line 215): ‘matches well’: seems to be a contradiction: when it matches well and the growth rate can be suggested, and then the resulting height for 2023 is way higher than measured, how can the match be well or the growth rate be reliable?

Table 2 refers to the results of the ICOS tree inventory carried out in the CP’s and SP’s in 2023. Table 4 in Zhao et al (2025) refers to non-ICOS inventories carried out in 1996, 2000, 2005, 2008, 2012 and 2025 in a quarter hectare SW from the first tower, which however, partly overlaps with CP-02 (see figure below). So they are predominantly independent data and they match in the sense that the tree heights are in the same range (18.4 m in the 2023 ICOS inventory vs 20.5 m in the 2025 non-ICOS inventory.

To clarify that the numbers refer to different sampling plots, we added ‘another plot’ to line 193: ‘This tree height matches well with tree height measured in **another plot** in the target area in earlier years’

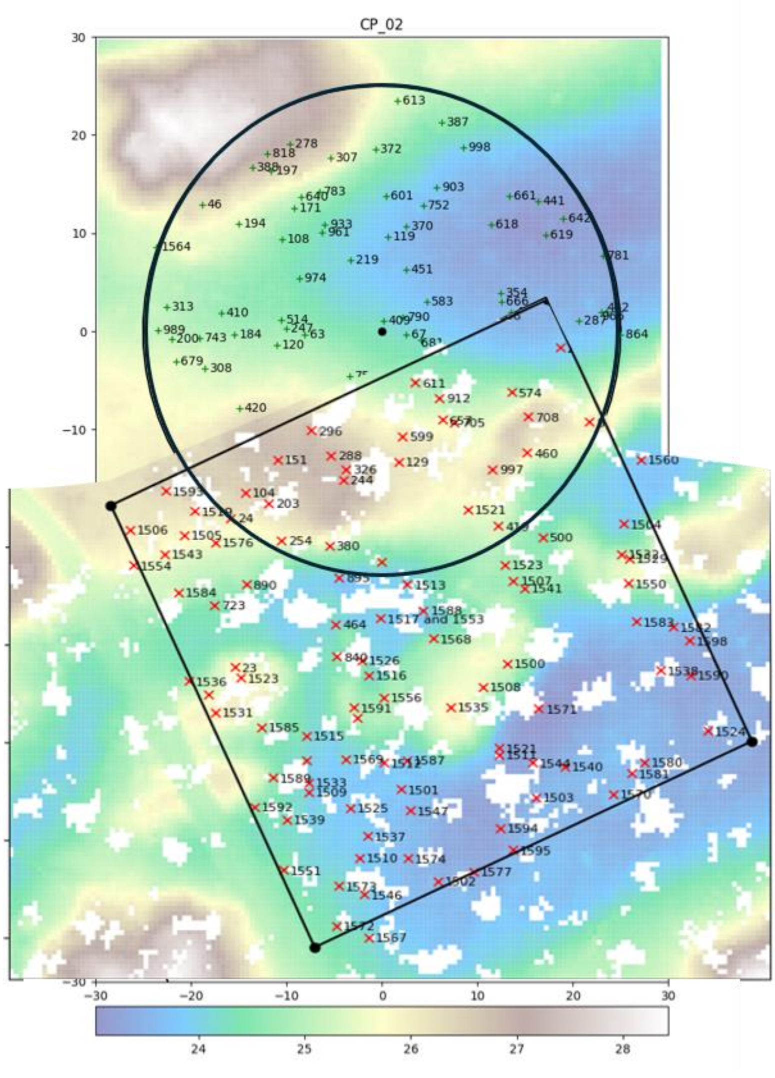

Figure showing the extents of the ICOS CP-02 and the non-ICOS rectangular quarter hectare plot reported about in Zhao et al. (2025). The x- and y-axes indicate m from the centre point of CP-02 and

the colour scale indicates the surface height above mean sea level in meters.

Line 192 (now line 218) 'is' → 'in'

We corrected this.

Line 204 (now line 228): 'and category dependent': if they are category dependent, they should have the index X also, e.g. \kappa_X

We added subscripts X to κ, λ and μ.

Line 215 (now line 241): 'of' → 'is'

We corrected this.

Line 236 (now line 262): subscripts H, V and R: you could mention that this convention is taken from the ICOS protocols

We added '**According to the ICOS instructions,** the variable names have subscripts H, V and R, …'

Line 242 (now line 269): '(Fig.)' → '(Fig. 8)'

We corrected this.

Line 253 (now line 280): Kipp & Zn → Kipp and Zonen

We corrected this. Sorry, this is a common abbreviation in Dutch.

Line 256 (now line 283): Kipp & Zn → Kipp and Zonen

We corrected this.

Line 266 (now line 296): Correct reference to section …

We corrected this.

Table 5: 'Eastward distance': from where? The EC system?

This was explained in line 247: '…eastward and northward distance with respect to the centre of the tower's base…', but we will add it in the caption once more to clarify.

We added 'The heights, east and westward distances are defined relative to the reference point, which is the centre of the tower's base.' to the caption.

Line 291 (now line 324): a + sign is missing here

We added a +-sign.

Line 324 (now line 359): caried → carried

We corrected this.

Line 339 (now line 374): 'show' → 'shown'

We corrected this.

Line 392 (now line 432): rephrase (and → at)

We think this sentence is correct. We added a ‘m’ to 1.5:

‘The two lowermost are ramified with two inlets at 1.5 **m** and 0.4 m and 4 inlets at 0.1 m.’

Line 413 (now line 453): what does "articifial" mean in this context?

The leaf wetness sensor looks like the figure below. It is described in detail at https://metergroup.com/products/phytos-31/ and used in Loobos by Melman et al, 2026 (https://doi.org/10.1016/j.agrformet.2026.111358). The sensor is made of a combination of plastics and metals and simulates a natural leaf. The sensors inside measure the conductivity of the artificial leaf surface, which is a proxy for leaf wetness.

Figure: Photo of a leaf wetness sensor.

Line 426 (now line 466): a 56 m long ~50 m PFA tubing

Our excuses, the correct length is 56 m and the associated theoretical delay time should be 9.5 s. We corrected this in table 9.

Line 426 (now line 466): (ID=6 mm): Inner diameter?

Yes, correct, we replaced ID with inner diameter.

Line 427 (now line 468): called "shipping" there, use just one of these two sea container or Shipping container

Yes, that is clearer. We replaced all instances of ‘sea container’ with ‘shipping container’.

Line 433 (now line 474): ‘Mixing ratios of VOC’s of > 500 individual masses < 1000 Th’: ? please explain

Th refers to the unit Thomson, which is used in mass spectrometry to quantify the mass-to-charge ratio. We changed this sentence into:

‘Mixing ratios of VOC’s of more than 500 individual masses with a mass-to-charge ratio < 1000 Th …’

Line 446 (now line 487): dependant → dependent

We corrected this.

Line 468 (table 9) (now line 508): this is a detail only: with 6 mm inner diameter, 10 L min^-1 flow rate and tube length 56 m, the theoretical delay time would be 9.5 s, not 8.6 s. The 8.6 s fit to roughly 50 m tube

length - and line 426 (now line 466) is strange but fits to this difference. What are the correct values?

Our excuses, the correct length is 56 m and the associated theoretical delay time should be 9.5 s. We corrected this in table 9. On top of that, we discovered that the time of the VOC data recorded drifts with respect to UTC. The vertical wind component is recorded with respect to UTC/CET. To clarify we added:

'It must be noted that the time in VOC data recorder's time drifts by about 7 seconds in 17 days with respect to UTC and right after a time reset, the delay time is close to 10 seconds.'

Line 473 (now line 513): which close → which is close

We added 'is'.

Line 477 (now line 519): 'definitive': precise? better? what do you mean with "definitive" here?

Here we show preliminary results from a covariance between w and VOC concentration to make the point that VOC flux measurements are feasible with the current setup. For definitive flux calculations, we need to consider other operations too, such as rotation of the wind components, trend removal, spike detection, etc. We are working on a dedicated paper about VOC flux calculations and the results.

Line 479 (now line 521): 'a the' → 'the'

We corrected this.

Line 504 (Figure 12) (now line 546): check formatting of the y-axis label here, you probably mean the complex conjugate of F_X(f) here, not \`

The '\' refers to the normalisation with respect to the total (co)variance.

Line 532 (now line 572): 'in a 9 point raster': according to the ICOS standard protocol, it should be in a 13 point raster at the CP's, and not regularly every second month but six times a year, with a focus on the growing season

We refer to 'Initial plot setup and installation', page 16 of https://www.icos-etc.eu/icos/documents/instructions/ancfor, where it says 9 points.

We collect the GAI samples every second month, as soon as the weather conditions are favourable for data collection. Particularly outside the winter period, it is sometimes difficult to find a time without direct sun, rain and with homogeneous cloud cover.

Page 9 indeed instructs to measure 6 times per year: 'Green Area Index is measured in the CPs at least six times a year for both deciduous and coniferous forests: two times during the period of leaf development, once at the peak GAI, two times during the leaf fall period and a final measurement during the dormant season.' The results can be seen via this website: https://icos.uantwerpen.be/Content/AncillaryReports/NL-Loo_Ancillary_Report.html#grp_gai. The seasonal cycle is captured with our sampling protocol, although it is difficult to predict when the transition periods are and to find a suitable moment. Perhaps we can use the phenocam data (https://icos.uantwerpen.be/Content/AncillaryReports/NL-Loo_Ancillary_Report.html#phenocam) to better time our GAI measurements.

Line 615 (now line 657): Section 2.5 → section 2.6

We corrected this.

Line 619 (now line 661): 'and, it' → 'and

We removed ', and'

Line 638 (now line 680): 'the stability expressed' → 'the stability is expressed'

We added 'is'.

Line 641 (now line 683): too → to

Yes, we replaced 'too' with 'to'

Line 663 (now line 706): unbalance → imbalance

We replaced 'unbalance' with 'imbalance'.

Line 699 (now line 803): 'Our data will feed into the ICOS Carbon Portal': they already do

Right, we removed 'will' in line 699 (now line 803).

3 Response to 2nd reviewer comments

(https://doi.org/10.5194/essd-2026-99-RC2)

First of all no need to be anonymous: I am Simone Sabbatini working at the ICOS Ecosystem Thematic Centre. Knowing my role can be useful to understand my perspective on this station and its data.

The authors provide an interesting overview of the measurements taken at the Loobos site, an evergreen needle forest on the Netherlands, in the last approx 5 years, that is since when what they call "the second tower" was operational. The second tower was indeed built to comply to the ICOS standards, and it represents the legacy of the previous tower ("the first tower"), in place since the Nineties and described on a previous paper published in this same journal earlier this year. The manuscript represents an important illustration about the relevance and the potential of the ecosystem monitoring in terms of GHG and beyond. However, in some parts it seems to overlook some important information, while avoiding to go deeper in key aspects, like for example the legacy. Also, it presents additional measurements not (yet) part of the ICOS portfolio, but the link with ICOS is not always clear (what is ICOS, what is not, how it works). All in all this mixture of info and some lack of completeness can confuse the reader and make it difficult to grasp the importance of the measurements. Also, the readers target is should be clearly identified: for example, is it OK to assume they know what is a footprint climatology and to have confidence with atmospheric characteristics like (z-d)/L?

For more details, and a line-by-line list of changes requested, please see the attached pdf file: (contents have been copied below by author).

We respond to your 'Final recommendations' (section 3.9) at the end, as many of them are addressed via the detailed comments already.

3.1 Abstract:

20 (now line 20): this mention of the quality in the abstract is important, but there is not much about quality in the main text: please integrate (e.g. how many data discarded for bad quality in the processing?)

We made a major revision of section 3.2 (Continuity of NEE timeseries from the first tower to the second), where we focus on data continuity, effects of measurement height, frequency corrections, flux footprints, etc. We would like to refer you to our responses to your comments to 'Demonstration' (section 3.4 in this document) and your 'Final recommendations' (section 3.9 in this document).

3.2 Introduction:

24 (now line 24): in the seek of clarity, I suggest finding a direct qualifier for the "first" and "second" towers, e.g. "with first tower we refer to the sensors deployed and the measurements taken in the pre-ICOS phase, that is from the first installation till the ICOS labelling".

It is indeed the purpose of this paragraph to distinguish the first and second towers and their equipment. Zhao et al. (2026) describes the instrumentation of the first tower, this paper describes the equipment of the second tower. To make this clearer, we change lines 27-30 from 'In 2018 a grant from the Dutch Science Fund (Ruisdael Observatory, 2026, NWO, 2026) allowed the station to be rebuilt with modern instrumentation in a taller tower according to ICOS instructions. In March 2021 a new robust scaffolding tower was built, which was equipped in subsequent months.'

into:

'In 2018 a grant from the Dutch Science Fund (Ruisdael Observatory, 2026, NWO, 2026) allowed the station to be rebuilt with modern instrumentation in a taller tower according to ICOS instructions. This second tower was built in March 2021 in the form of a robust scaffolding tower, accessible with stairs, and it was equipped in subsequent months.'

25 (now line 25): please note Zhao et al. was published in 2026. Please correct here and elsewhere.

Yes, Zhao et al (2026) was published during the submission process of the current paper. We change Zhao et al. (2025) to Zhao et al. (2026) and we update the reference list.

31 (now line 31): please shortly mention the main changes required to be labelled

We include 'The main changes required for ICOS labelling include a taller tower, a change from open path to enclosed path CO_2 and H_2O sensor in the eddy covariance setup, a CO_2 profile with many more levels and more extensive ecosystem characterisation.' in lines 30-32 (now lines 31-33).

32 (now line 35): what is geographic history?

I suspect your question is if we should refer to geology or geography. Geologic history describes how geological processes (tectonics, vulcanism, sedimentation) have shaped the soil, landscape and ecosystem over time, whereas geographic history includes the formation of landscapes, interaction with climate and ecosystems. So, indeed we should consistently refer to geographic history.

34-36 (now line 37-39): I think for CO2 eddy covariance you focus only on the fluxes calculated before the postprocessing (no ustar filtering and no gap filling): this is clearly a possibility, but should be stated somewhere, and a reason provided (as this is the ICOS tower, and the data are also centrally processed and post-processed, you may be able to use the final datasets)

This is a good point, let's make it explicit in section 3.1. To the caption of Fig. 14, we add:

'Here we show the eddy covariance fluxes without post-processing (u_* filtering and gapfilling) as output from EddyPro.'

37 (now line 40): see comment 32: should be the same or not?

Yes, indeed, we changed the section heading to 'Geographic history'.

69 (now line 72): the soil was poor for use or in general? because first you mentioned agriculture, and then not suitable for agriculture

The inhabitants needed agriculture for sustenance, but the soil was rather poor. We added 'however' to highlight the controversy: 'The poor soil ('Veluwe', in old Dutch 'Vale Ouwe' is Germanic for 'fallow lands') was, however, not very useful for agriculture'.

77 (now line 80): section 2.4 is air meteo sensors

We removed the reference to section 2.4.

49,79 (now lines 49, 79): consider merging the two figures in the same one

Figure 1 shows the regional geographic processes as a result of ice wedges, whereas figure 2 shows the local situation at the site as a result of wind erosion superimposed on the larger scale landscape formation. We think they both have their unique function and prefer to keep both. Or do you mean to merge them in one Figure with two panels?. We think the two figures suit their purpose in the current setting.

88 (now line 91): WWII. However this is counterintuitive, as in the first period the water table depth was supposed to be closer to the root system, and the acidification was lower... In addition, if the microorganisms were depressed by acidification, what triggered the growth after WWII if also decomposition was slow?

Yes, this is an interesting situation, before WWII the trees probably had easier access to ground water, but fewer nutrients. After WWII, the water table declined, but nutrients became more available. We think it still took the trees some time to grow roots towards the water table after plantation. Once the water table was reached, the roots probably grew deeper with declining water tables (5 m supposedly is not very deep for Pine roots). But growing roots towards the water table was probably slow because of the low nutrient availability. Unfortunately, we do not have water table measurements going back so long ago. The figure below shows how N-deposition and Basal Area Increment evolve over time in since the 1900's, which suggest a tight connection, although we cannot tell the relevance of water table.

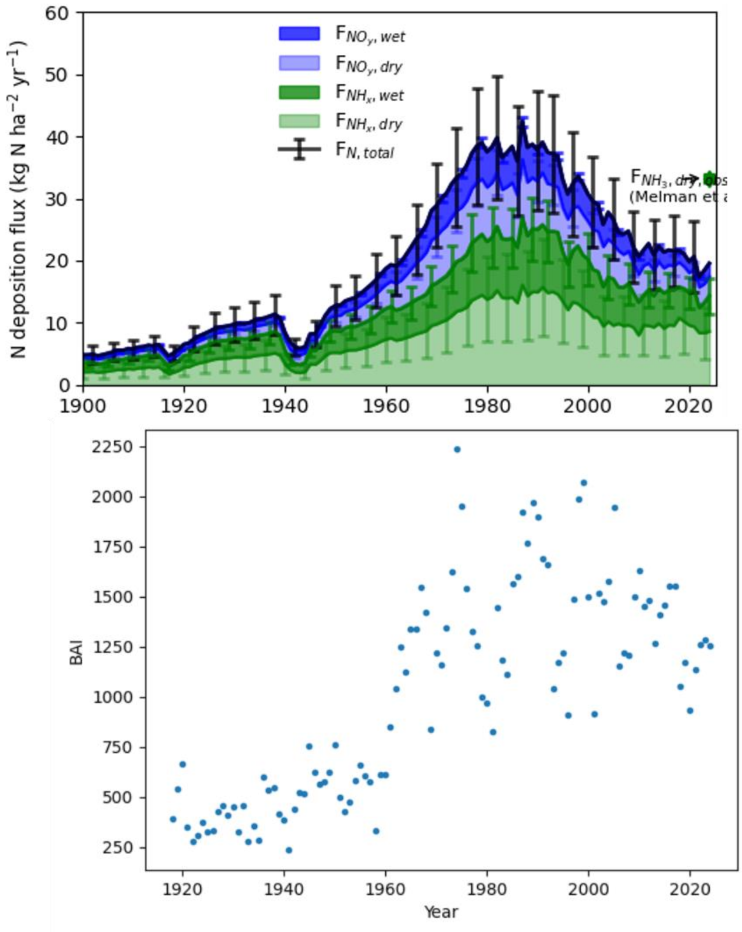

Figure showing timeseries of N-deposition and Basal Area Increment.

A decline in ecosystem respiration was detected since the start of the CO2 flux measurements in 1997 and related to acidification (vdMolen et al, 2026, GCB, https://onlinelibrary.wiley.com/doi/10.1111/gcb.70849). However, in order to decline, it must have first increases, since a young forest planted on bare sandy soil cannot have respired a lot of CO2. We anticipate that the declining respiration started gradually with increasing carbon stocks and increasing cumulative N-deposition.

106 (now line 109): was there an impact of Prunus invasion in the tree rings (e.g. competition)?

We do not think there is a lot of competition between the Pine and Prunus. The Prunus represent only 2% or the total biomass (https://icos.uantwerpen.be/Content/AncillaryReports/NL-Loo_Ancillary_Report.html#grp_tree-data-at-stand-level). The Prunus grow in the shade of the Pine trees, there is plenty of nitrogen. The Prunus probably does not root to the ground water table and are more sensitive to drought (they shed leaves early in dry summers). So, interesting hypothesis, but we do not think so.

110 (now line 112): can you show the decrease in of ecosystem respiration?

Yes, of course: we'd like to refer to our recent publication in Global Change Biology (van der Molen et al., 2026k, https://doi.org/10.1111/gcb.70849) about the declining respiration rates. Figure 1 shows a timeseries of the total ecosystem respiration rates and the paper explains in detail how we assessed the data and how we relate it to acidification and N-deposition:

van der Molen, M. K., M. van de Sande, M. i. '. Zandt, et al. 2026. " Declining Ecosystem Respiration Linked to Nitrogen Deposition: Insights From a 26-Year FLUXNET Record." *Global Change Biology* 32, no. 4: e70849. https://doi.org/10.1111/gcb.70849.

116 (now line 119): would be nice to see the borders between horizons in the figure. Please also note that the ICOS sampling scheme has not been introduced yet (the reader doesn't know yet what is an SP)

Thank you for the suggestion, we annotated the figure to indicate the horizons and their borders. We also added a reference to section 2.8.3 Soil Inventory in the caption: Picture taken at SP-I_12 (see section **Error! Reference source not found.**) on 24 January 2024.

121 (now line 125): This is a good spot to introduce what was required for the labelling, what are the CP, SPs (so you can refer here in fig.3) etc. with mention of the instruction doc (please note this is not the proper way to cite the ICOS Instructions, see References). And also the fact that AGB is retrieved via allometric equations. In what consisted the tree sampling campaigns?

We are not exactly sure what you mean with 'what was required for the labelling', because we basically built an entire new station next to the first one. The CP's and SP's, and the tree inventory methods are introduced in detail in section 1.2.1. We searched the document for 'ICOS Instructions, and we added references to specific ICOS instructions where relevant (to Papale and Nicolini (2016) and Gielen et al. (2017), Sabbatini and Papale et al., 2017 and Loustau et al., 2017). However, we still prefer to keep the link to ICOS ETC, 2026, because it provides the landing page for all instruction documents.

133 (now line 138): technically not a square...

Yes, correct. Well, it accidentally represents the projection of a square with a non-zero viewing zenith angle in Google Earth. We replace 'square' with 'box'.

136 (now line 141): a.o.?

Right, the abbreviation a.o. = amongst others apparently is a form of Dunglish. We rephrased the sentence from:

'The 'target area' is the area of which we aim to understand a.o. the carbon and water fluxes.'

into:

'The 'target area' is the area of which we aim to understand the carbon and water fluxes and other processes.'

137 (now line 142): based on kml files provided by ICOS ETC?

Yes, correct.

151 (now line 175): did you count only the shrubs of Prunus only with DBH>5 cm? For this species, did you apply the ICOS protocol for measuring AGB of the understory?

Yes, correct. In fact, there are quite some seedlings with diameters less than 5 cm or even less than 1 cm.

161 (now line 185): if SP-I_05 and 16 are in a less densely populated part of the forest, we should see it in Figure 5, not 6 (true for 16, less for 5). So the trees in SP-I_05 should be smaller than average: is it the case?

Yes, correct. In SP_I-05 there are thinner trees. Maybe 'more open' is a better formulation than 'less densely populated'.

We changed 'less densely populated' with 'more open' and referred to figure 5 and instead of only to figure 6.

180 (now line 204): I am not sure fig. 7 is the best option to visualise diameters distributions. I think a classical histogram with the number of trees in each class could better describe the type of forest

We agree the figure appears somewhat chaotic. However, that is also due to the underlying data. We tried several other types of figures, but they do not really look better, for example the figure below shows absolute tree densities as a function of diameter range and species. In this representation, the less abundant species become nearly invisible.

We propose to keep the current representation, as it is still easy to read and helps quantifying relative and absolute numbers.

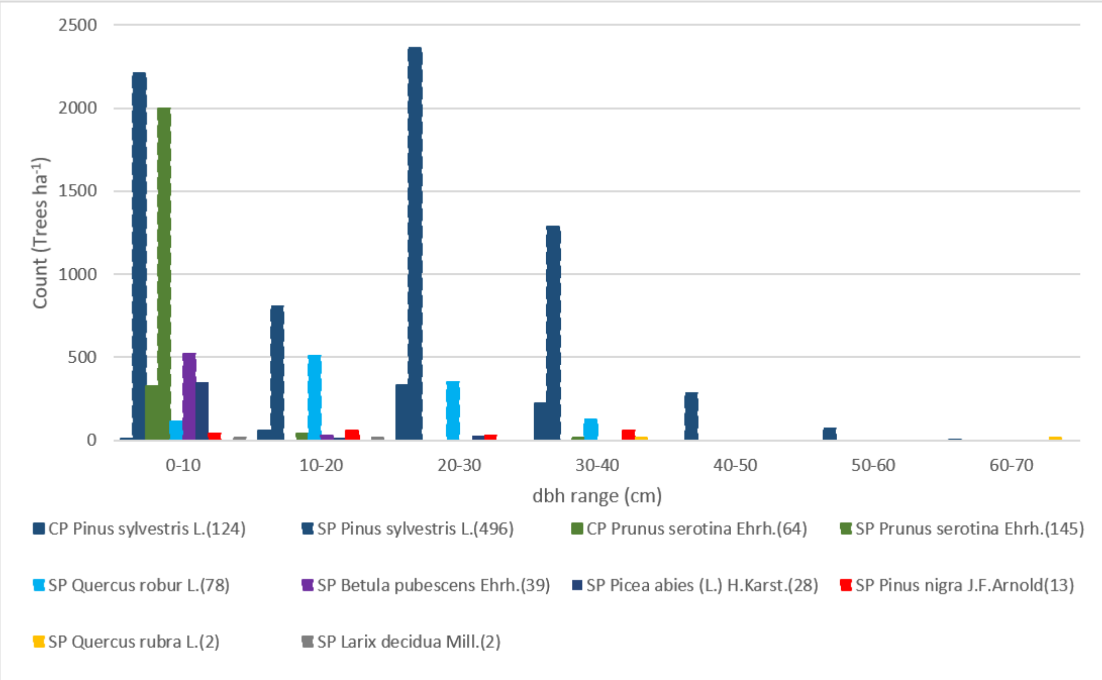

Figure: Alternative for Fig. 7, which we think is not really an improvement.

185 (now line 211): then the canopy height is not taken from the soil (i.e. if over a dune, from the top of it)? If so, the fact that it is homogeneous would mean that the trees grew the same in the valleys and in the dunes.

Tree height is by definition measured from the foot of the tree, regardless the local topography height. Because the trees are taller in valleys than on dunes, the top of the canopy is rather flat and appears at a height of 20-22 m relative to the base of the tower.

186 (now line 212): the uncertainty is based on SD or SE? If SE, it is much higher than first tower

We added '± 1 standard deviation' to the caption.

196 (now line 221): is this also the equation used in ICOS?

Yes, correct.

200 (now line 226): specie-specific parameters

We added 'specie-specific'. See also the comment of reviewer #1 to line 202.

205 (now line 231): again if you make clear what is pre-ICOS versus what is post-ICOS, you may also add the root biomass

The exact same allometric relations are applied in the pre-ICOS and ICOS inventories. We think that is sufficiently clear from the equations and tables. The point is more that ICOS does not quantify the root biomass.

218 (now line 244): without an uncertainty analysis, the comparisons between data taken with different sampling schemes are really difficult to be interpreted. Would it be possible to check for an overlap of sampling points, and use only data that overlap?

See the response to Reviewer #1's comment to line 289. The figure shows that 22 trees are both in CP_02 and the quarter hectare plot sampled since 1996. In our situation, looking at uncertainty between non-ICOS and ICOS inventories comes down to looking at the consistency of the observed dbh's and heights of those 22 trees, because the allometric relations are identical. The figure below shows how dbh and height of those 22 trees have developed over time. Here we included the 1996, 2000, 2005, 2008, 2012, 2023 and 2025 inventories.

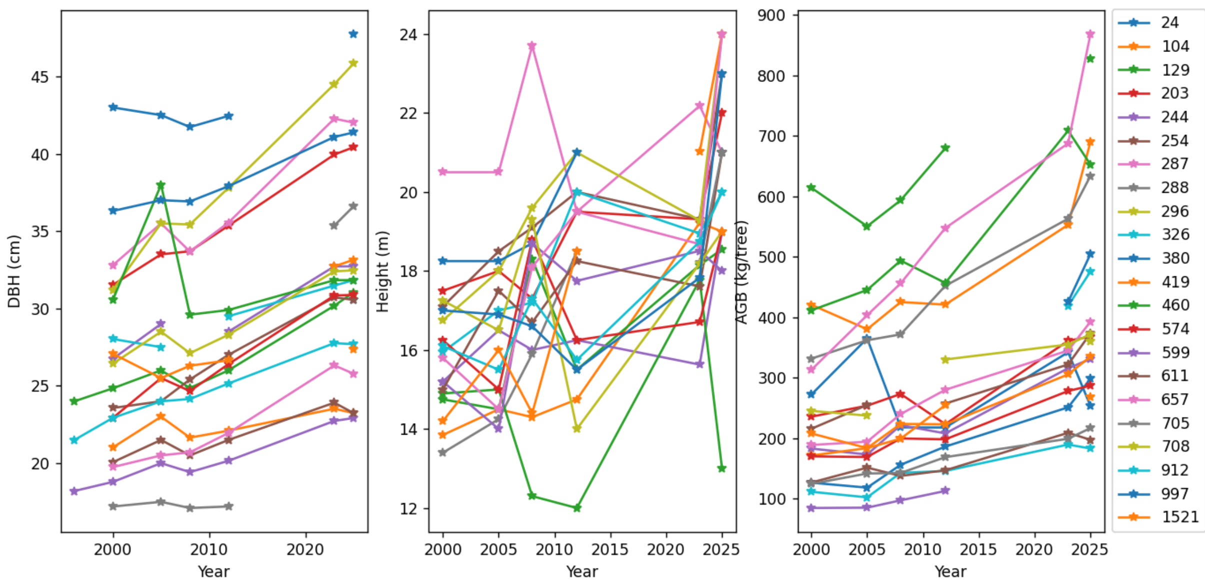

Figure: DBH, height and AGB of 22 trees which are in the non-ICOS inventories (1996, 2000, 2005, 2008, 2012, 2025, Zhao et al., 2026) and in the ICOS inventory (2023).

The figure shows quite consistent growth of dbh over time. Some trees have little growth or even a declining dbh and have died after 2012. The observed heights seem to increase less consistently. However, the pine trees sometimes lose (part of) their crown during incidental heavy snow load, a decrease in height is unrealistic. The above ground biomass seems to increase quite consistently.

We think these data suggest that the inventories are meaningful and largely consistent. But looking at uncertainty goes much further than looking at consistency, it also includes the absolute and random errors of the dbh and height measurements, the quality of the allometric relations and the sample size. We think this is clearly outside the scope of this paper. There are likely papers dedicated to this subject.

221 (now line 247): was dead biomass included in the previous sampling?

Tree health status was not reported. Tree ID's that do not continue in the database could be dead trees.

223-224 (now lines 249-250): the allometric equations were not the same in the two campaigns? Didn't you make your own equations for ICOS?

Yes, the allometric relations were identical in all campaigns. They are based on the 7th Dutch Forest Inventory (Schelhaas et al., 2022) and hence these results are comparable with the Dutch forest inventories.

3.3 Instrumentation:

237 (now line 263): R stands for replicate in ICOS

We corrected this.

238 (now line 264): this is not the ICOS standard: if an instrument is replaced, the variable name does not change. If it is added in close proximity, then the index of the replicate is raised.

OK, thank you. We replaced

'If an instrument is replaced by another instrument, the R index is increased by one.'

with

'If a sensor is added in proximity to another one measuring the same variable, the R index is raised.'

243 (now line 270): please note current location of ICOS tower from ICOS BADM is: 52.16644722N, 5.74355E, while older tower is reported at 52.166553N, 5.743589E. In the manuscript: Latitude 52.166581 °N and Longitude 5.743556 °E. These are small differences but would be better to be consistent

The correct coordinates are 52.16644722N, 5.74355E, which is in agreement with the BADM (i.e. the internal ICOS instrument registration system). The coordinated of the first tower you mention are also correct. So we replace

'The station coordinates in the WGS 84 system are: Latitude 52.166581 °N and Longitude 5.743556 °E.'

with

'The station coordinates in the WGS 84 system are: Latitude 52.16644722 °N and Longitude 5.743556 °E.'

244 (now line 271): elevation is 33 m in ICOS BADM

The correct height of the tower's base is 24 m AMSL. We should correct this in BADM.

251 (now line 278): why radiometers and phenocams together?

We wanted to limit the number of sections and hence grouped sensors by type of measurements as much as possible. Phenocam measurements are also based on radiation, so we included it with radiation.

253 (now line 280): Kipp & Zonen (here and elsewhere)

We corrected this.

263 (now line 290): here and elsewhere: if this is ICOS data the Carbon Portal should be also mentioned

We add 'These data are available via **the ICOS Carbon Portal** (van der Molen et al., 2026b, 2026c, 2026f, 2026g).

268 (now line 295): (here and elsewhere) what is the purpose of having var names? To retrieve respective location according to the rules explained above? However you have height and distances. And what about the serial numbers?

We included variable names to facilitate the use and explanation of our datafiles. It is important to document variable names, locations and serial numbers, because we experienced this information is easily lost after some time with changing instrumentation, setup, technicians and PI's.

270 (now line 339): Phenocams are also going to be included soon in the ICOS portfolio

Correct, this is a new development. We updated lines 189-297 from:

'A StarDot Netcam SC5 IR was installed at 35 m height in the tower facing the North. Images are collected hourly from 10:30 to 17:30h CET. Images are automatically uploaded to 'PhenoCam, an Ecosystem Phenology Camera Network' operated by the Northern Arizona University (Richardson et al, 2018) and automatically analysed for green chromatic coordinate (GCC) and normalised difference vegetation index (NDVI). All images and results can be viewed via PhenoCam (2026) (Richardson et al., 2018, Seyednasrollah et al., 2019). The GCC and NDVI time series show similarity with the LAI measurements (Zhao et al, 2025), i.e. a peak in August and low values between November and April.'

to:

'A StarDot Netcam SC5 IR was installed at 35 m height in the tower facing the North. Images are collected hourly from 10:30 to 17:30h CET. Until 28 May 2026, images we automatically uploaded to 'PhenoCam, an Ecosystem Phenology Camera Network' operated by the Northern Arizona University (Richardson et al, 2018) and automatically analysed for green chromatic coordinate (GCC) and normalised difference vegetation index (NDVI). All images and results can be viewed via PhenoCam (2026) (Richardson et al., 2018, Seyednasrollah et al., 2019). The GCC and NDVI time series show similarity with the LAI measurements (Zhao et al, 2026), i.e. a peak in August and low values between November and April. After 28 May 2026, ICOS took over the function of the phenocam network in Europe. Data are now uploaded to the ICOS Carbon Portal (2026) and the resulting analyses via Iserbyt et al. (2026). Content-wise, nothing has changed in the data processing and analysis.'

277 (now line 309): it would be great to superimpose fig. S1 with a DEM

Yes. It is technically challenging to superimpose the DEM, because the airborne photo's are not available in the same software package. And we think the figure would become confusing. But we can add a second figure with the locations of the CP's, tree inventory plots and the locations of the soil measurements on top of a DEM (see below).

We included this figure in Figure S1 and modified the caption into:

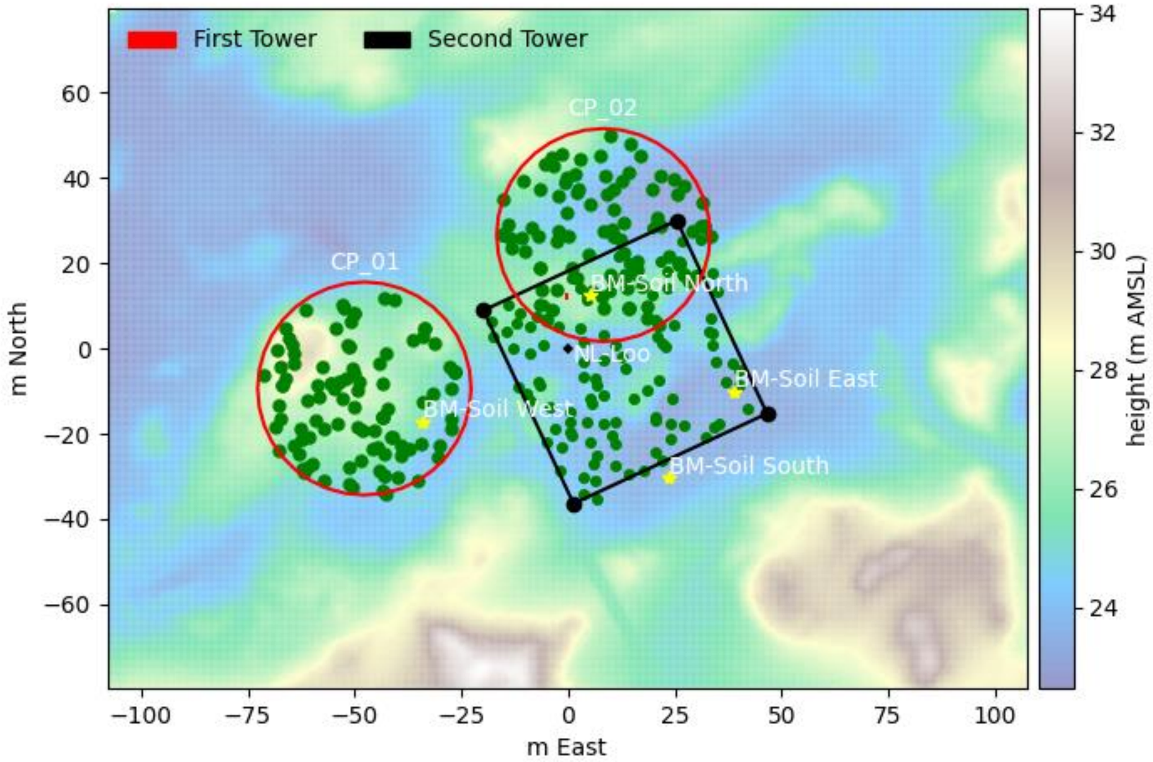

Figure S.1. Location of the continuous soil measurement plots. 'NL-Loo' identifies the location of the tower, superimposed on arial photography (top, Picture by © Google Earth annotated by authors) and on a Digital Elevation Model (bottom, © Nationaal Georegister, 2024). The red circles to the West and North indicate the location of CP_01 and CP_02. The yellow pins show the locations of the 4 soil measurement locations. The West and North plots are on the flank and top of a dune, the East and South plots are in a valley between the dunes. In the bottom figure, the x- and y-scales are in m East and North from the center of the tower.'

278 (now line 310): G is only measured at 5 cm. TS at 1.5 cm and SWC permit the calculation of the storage of G (made by the ETC during the processing)

Correct, this is not entirely clear indeed. We add 'at 5 cm depth' in the sentence:

'Continuous soil measurements are taken at 4 plots around the tower (Fig. S.1). At each plot, the soil heat flux (G) is measured at 5.0 cm depth along with soil temperature (TS) at 1.5 and 5.0 cm deep ...'

290-295 (now line 322-329): I would not only mention the Carbon portal, but also the fact that the ETC is processing this data following the equations reported, and that the meta information needed, as the installation depths, are reported via a standard protocol (BADM)

We add this information: (line 326):

'ETC processes these data following these equations, and the meta information needed, such as the installation depths, are reported via a standard protocol (BADM).'

311 (now line 345): not clear why the below canopy measurements are referred to in here. Indeed, "main" TA and RH do not seem to have been included in any section.

We add '(Section 2.6 **describes below canopy measurements**)'

324 (now line 358): “carried” (typo)

We corrected this.

325 (now line 359): the forest canopy was previously said to be on average between 15 and 18 m, so the EC system should be around 20 m above it. However, for the purposes of the eddy covariance processing, for which the distances from the “surface” is needed, it is not the average that matters, but something representing the “canopy top”. What we use in ICOS is the 90th percentile of the trees heights sampled, which for NL-Loo is 22 m: in this sense the tower is 16 m above the canopy top.

This is not the complete story. The average tree height is indeed between 15 and 18 m, but many trees are on dunes, so their apparent height with respect to the tower base is 20-22 m (line 185). In that sense, the EC system is about 16 m above the canopy.

331 (now line 365): please add something like: "this setup was done by following the ICOS requirements and by discussing with the ICOS ETC, during the labelling phase, the best strategy given the local station characteristics"

We added ‘This setup is compliant with the ICOS instructions.’ Although we acknowledge the support of ETC during installation and checking, we do not think we deviate from the instructions, so adding a note about discussing the best strategy would be confusing.’

In the acknowledgements (line 942) we add:

‘We acknowledge the help and support from ICOS ETC staff during installation, verification of file formats and data submission. Part of this paper follows the structure of the ICOS ETC Labelling report written for Loobos.’

333 (now line 367): see above for the serial numbers (but also logger and file ID). On the contrary, more details may be needed on the distances (e.g. from what?) and wind exclusion ranges

See our response above. The distances and wind direction exclusion angle and range are already included and are always with respect to the center of the tower base as indicated in the introduction to section 2 and Figure S.1.

337 (now line 371): OK, see above

OK

338 (now line 372): in general: what is the target for the readers community? Are they supposed to know what is a footprint climatology? (z-d)/L? But not for example covariance maximisation method (see lines 459-460)? And: how long of a period has been used to extract this climatology? I think providing a short overview of the theory behind the measurements, and the processing steps could really help the reader

That is an important and difficult question. We think it is important to document our datasets well for general and more introduced users of the data. Therefore, the information as it is written down is mostly understandable for most readers, but every once in a while some more specific information is supplied. We trust that readers who do not need this information will skip those parts. In fact, we think many readers will use the paper as a reference, looking up the ‘only’ the information of relevance for their purpose.

339 (now line 373): “shown” (typo)

We corrected this.

348 (now line 383): suggestion: "... as the tower distorts the wind flow and the turbulence field characteristics"

Thank you for this suggestion. We changed the caption into:

'The isopleths are interrupted towards the east, because this area is excluded from flux calculations, as the **tower may distort the wind flow and the turbulence field characteristiscs** from those areas.'

350 (now line 385): please change the section title to something like "Air meteo and profiles", so that you can include also main TA/RH, and justify the presence of measurements above the canopy

Indeed. We changed 'Below-canopy profiles of ...' into 'Vertical profiles of ...' Also in the caption of figure 8.

351-358 (now lines 386-394): this is a theoretical background, which I think is good to provide, maybe even before (See comment 338). However, I would also mention that such a distance between the sensing system and the canopy is supported by the theory as well, because being too close to the canopy top would mean measuring in the roughness sublayer leading to de-coupling and bad measurements in unstable conditions. Also, stable conditions will be mostly discarded by the ustar filtering during the post-processing

Although you are mentioning a true point, we think this paragraph is intended to explain the need for measuring vertical profiles in stable conditions. It is not about EC measurement height, roughness sublayer and bad EC measurements in unstable conditions. Therefore we prefer to keep this paragraph as it it.

360 (now line 396): not clear in this context what wind speed profile is needed for.

We change this sentence from:

'To quantify the below-canopy storage of CO_2, water vapour and heat, these variables are measured along a vertical profile along the tower. CO_2 and H_2O mixing ratios are measured at 11 levels, temperature and wind speed at 5 levels (**Error! Reference source not found.**).'

to:

'To quantify the below-canopy storage of CO_2, water vapour and heat, these variables are measured along a vertical profile along the tower. CO_2 and H_2O mixing ratios are measured at 11 levels and temperature at 5 levels (**Error! Reference source not found.**). In addition, we also measure wind speed and direction at 5 levels in order to characterise below-canopy wind flow. The wind speed and direction profile measurements are not mandatory for ICOS, but still submitted to the carbon portal.'

365 (now line 403): this is a theoretical formula: in the practical implementation, the time derivative is approximated by a difference between consecutive measurements, and the integral by the summation of concentrations measured at the different levels, weighted for the heights of the levels. Again, calculations are made at the ETC and the setup was done in accordance to the ICOS requirements

OK, thank you. We add: ‘The storage fluxes are calculated by ICOS ETC after submission of daily data.’

385 (now line 424): the tree top levels are actually above the canopy

We changed ‘Below-canopy profiles into ‘Vertical profiles’ here too.

394 (now line 433): is this automatised?

What do you mean? The tubes are heated constantly to make a temperature difference of 1 ^oC above the soil temperature by running a constant current through the wire, assuming heat dissipation is constant.

408 (now line 447): "...and via the ICOS CP..."

These references actually point towards the ICOS Carbon Portal. We change the sentence into: ‘The data are available via **the ICOS Carbon Portal** (van der Molen et al., 2026a, 2026b, 2026c, 2026d, 2026f, 2026g, 2026h).’

415 (now line 454): good to mention also non-ICOS data, however as already suggested I think it should be made in a more organic way

We think there is no single best way to structure the paper along types of measurements and whether they are required by ICOS or not. We think the current structure is sufficiently clear and prefer to keep it as it is.

417 (now line 456): all this section 2.7 could be complemented with some recommendations for potential future deployment of these measurements in ICOS: the expertise of sites already measuring is key for the implementation of new measures.

We’d be super happy if we were already at the stage that we could advise other station PI’s. However, at this stage, we provide a description of our instrumentation and an analysis of the potential for applying eddy covariance (which is positive for VOC’s and O_3). However, we clearly need to invest in data processing protocols to determine the need and impact of wind rotations, spike detection, trend removal, WPL corrections, etc., as well as calibration routines. We are working on dedicated papers about the processing and analysis of VOC and O_3 data.

426 (now line 465): 56 or 50 m?

It should be 56 m, excuses for the confusion. We corrected this.

434 (now line 473): please define what Th stands for. Does this mean masses between 500 and 1000 Th? What does it mean to stay in this range?

(See also the corresponding comment by Reviewer #1). Th refers to the unit Thomson, which quantifies the mass-to-charge ratio. We modified this sentence from:

‘Mixing ratios of VOC’s of > 500 individual masses < 1000 Th ...’

into:

‘Mixing ratios of VOC’s of more than 500 individual masses with a mass-to-charge ratio < 1000 Th ...’

449 (now line 488): How is it calibrated?

The instrument is designed for long-term absolute O_3 concentrations. It may be calibrated on a regular basis with an ozone generator. We do not have a protocol for this yet.

452 (now line 491): in this section I would mention what differs as compared to CO2 eddy covariance method. From this text it only seems that you apply timelag correction and some kind of spectral corrections, while mentioning that you also detrended w and C. What about for example coordinate rotation and the other corrections commonly applied in EC? See also comment 338

See also our response to your comment to line 417. The flux measurements as written down here indeed contain time lag correction and covariance with the vertical wind velocity. As explained, we are working on more dedicated papers about the exact data processing chain, including calibrations, rotations, frequency corrections, etc., and their impacts. We think these deserve dedicated papers, while from the point of view of this paper as a site reference paper, it is good to mention the potential for applying eddy covariance to non-CO2 gases.

463 (now line 502): please define "optimal"

With optimal is the most positive or most negative flux. We explained this more explicitly:

'… optimal **(most positive or most negative)**'

465-466 (now lines 504-505): not clear how you used the timelag for monoterpenes, and a constant value of 1 s for O3. In general the strategy for time lag is not 100% clear to me, and taking just one halfhour to demonstrate that it is OK it's not that robust

We are not sure what is unclear here. We think the equation in line 460 is clear: the VOC data are delayed because of the travel time in the tube. The operation intends to synchronise w and the VOC signal. We do this by calculating the covariance at a range of time lags to allow for the time it takes for the air to move from inlet to instrument. This is standard practice (see also Aubinet et al., 2012).

Figure 11 (left) shows that the covariance develops quite as expected with the time lag applied. The shape of the curve is usually best for monoterpenes as it has a high signal to noise ratio and a non-zero flux. A small inconvenience is that the VOC's data recorder's time drifts a bit with respect to UTC (see our response to the comment of Reviewer #1 to line 468). So using a fixed time delay is not possible for VOC's. Therefore we use the time delay for monoterpenes as a fallback option for other species. For O_3, the time delay is quite constant at about 1s. Again, we would like to emphasise that here we intend to point out our potential for applying eddy covariance to VOC's and O_3 and we are working on dedicated papers about data processing and analysis.

472 (now line 511): being based on a single halfhour only, it doesn't look so general

Yes, this could be phrased better. We change the sentence

'Figure 11 shows the resulting VOC and ozone fluxes as a function of the applied delay time for two selected times. The examples show that in general the detected delay time is close to the theoretical delay time.'

into:

'**Error! Reference source not found.** shows a typical example of the resulting VOC and ozone fluxes as a function of the applied delay time. The examples show that the detected delay time is close to the theoretical delay time.'

478 (now line 518): this is not automatic? PWB method from Vitale could be explored (as a future suggestion)

Thank you. Yes, it is automatic. The point is that we need to set limits to acceptable time delays and flags for suspicious ones.

484 (now line 524): what is the meaning of the sign for the lag?

A negative time delay here means that the gas concentration trails behind the wind measurement, as expected. Positive time delays should not be possible. But for the sake of defining the optimum flux well, it helps to apply a somewhat larger range.

488-489 (now lines 528-529): so there is not a real spectral correction, but just a check that the setup is done in a proper way such as to minimise spectral losses? Why?

We did not apply spectral corrections yet, see our responses to your earlier comments. In the dedicated papers we will go into the details of this. Nevertheless, we expect spectral corrections (other than line damping) to be minimal, because the EC system is so high above a rough forest. Spectral corrections for CO_2 are typically less than 2%. But we will address this in the dedicated papers.

491-493 (now lines 531-533): I am wondering if this corresponds to a lower sampling frequency: I understand this should be "worst", however the aim here is to check CO2 behaviour in the case of 2 Hz frequency, so should be done as if it was sampled at 2 Hz.

There is a difference between sampling frequency and sample integration time. A relatively good situation may occur if the sample frequency is lower (e.g. 2Hz instead of 20Hz), but the sample integration time is still short (less than 0.05 s). In that situation, the fluxes are merely calculated with a lower statistical sample size. However, if the sample integration time also become longer with the reduced sample frequency (e.g. 0.5 s instead of 0.05 s), there is a larger potential for high frequency attenuation. This is what we refer to as the worst case scenario. And still in this worst case scenario, by applying a block averaging of up to seconds, the fluxes remain merely the same.

494 (now line 534): however this result is a bit surprising: this basically means that for tall forests and measurement heights we could use a slow response IRGA... Is CO2 behaviour comparable to VOC?

Again, only one day and a few halfhours may not be enough. Also, the difference is based on visual inspection, while it would be preferred to have some statistics. What about H2O, also reactive?

Yes, it is surprising. It also makes us wonder why we should invest in expensive fast response sensors, when it is not always strictly needed. We are not sure if we should mention this in the paper, but the results are very much in line with the presentation Bernard Heinesch gave at the last ICOS ETC MSA meeting in November 2025 in Helsinki (Bernard gave a presentation to the station PI's in the Measuring Station Assembly about the high frequency corrections, showing that they are large for short canopy stations, but nearly zero for tall, rough canopy stations. But this is not publicly available information).

506 (now line 546): why you didn't select the standard log-log scale for cospectra?

We did this because the covariances can be negative in part of the spectra.

508 (now line 548): why 7.5 s?

Yes, this may be confusing. This is because the time delay drifts with the VOC data recorder's time. So the time delay should not be interpreted in an absolute sense. We remove the sentence 'The co-spectra are computed with a sample delay of 7.5 s and 1.2 s for VOC and O_3 fluxes, respectively.' because it is quite obvious that we applied time lag correction and it may be confusing.

514 (now line 545): noise for VOCs starts at 0.04 Hz, except monoterpene that is 0.4 Hz. O3 0.1Hz: can we still talk of "high-frequency" noise?

What we call 'high-frequency noise' occurs in the inertial subrange and outside the energy containing range. But we see your point.

523 (now line 562): definitely something to study in more detail. All this spectral part is a bit generic, and sounds like something you need to invest on, not yet fully implemented. Which is totally fine, however it should be presented as it is

Yes, we will go into detail about this in future publications. However, based on figure it can already be predicted how sensitive the fluxes are to an error in the detected time delay.

525 (now line 564): Sabbatini (double B) :) OK for future publications on that, however it is not clear if you already started producing this data or not. Please note that some ICOS processing steps will likely be not applicable as such (e.g. data cleaning)

We corrected this. Please also see our earlier responses about the purpose of this section.

539 (now line 578): are earlier estimates are not based on DHP, but on LAI2000. Again, comparison should be taken with extreme cautions, and the differences should be highlighted. This may also explain why leaf area is in line (lines 550-551), but not LAI.

True. We added 'which is considerably larger than the earlier estimates (Figs. 5 and 6 in Zhao et al., 2026) **using a Li_Cor LAI-2000 sensor**'.

550 (now line 589): the results are relative to 2024 and 2025

We corrected this.

556 (now line 596): 2024 and 2025. Please note that the sampling strategy described in this caption (10 trees and 15 "foliar samples" per CP) is different from what reported in the text (6 trees per CP, 2-3 twigs per tree for a total of 30 samples and 10 leaves per each sample). Which one is correct? What is prescribed by ICOS Instructions?

We corrected the 'and 2025'.

Yes, this is confusing.

In each CP, 6 trees are selected (12 in total). From these trees, 30 twigs should be collected, so 3 twigs from 6 trees and 2 twigs from the other 6 trees.

We change the caption of table 10 from:

‘Table 10. Mass, area and nutrient concentrations of foliar samples in 2024. In each Continuous Plots 15 foliar samples were collected from 10 trees (in total 30 samples). Each sample consists of 10 needles. The samples were dry weighed at 65 °C, the area measured, then submitted to the ICOS lab in France, where they were dried at 103 °C and analysed for nutrient concentrations. The samples were collected in March 2024/2025. The area and dry mass refer to the total over the 10 needles in a sample. The dry ratio is the ratio of mass at 103 °C and at 65 °C. The mean and standard deviation are calculated over the 15 samples per CP.’

To:

‘Mass, area and nutrient concentrations of foliar samples in 2024 and 2025. In each Continuous Plots 15 foliar samples were collected from 6 trees (in total 30 samples in two CP’s). Each sample consists of 10 needles. The samples were dry weighed at 65 °C, the area measured, then submitted to the ICOS lab in France, where they were dried at 103 °C and analysed for nutrient concentrations. The samples were collected in March 2024/2025. The area and dry mass refer to the total over the 10 needles in a sample. The dry ratio is the ratio of mass at 103 °C and at 65 °C. The mean and standard deviation are calculated over the 15 samples per CP.’

556bis (now line 596): in some cases, adding the findings from Zhao 2026 together with the ones included in the manuscript may help the reader. This is one.

We see your point. But then again, merging the data into a single table also suggests the data are directly comparable to one each other. And the number of elements observed is different in the different foliar analyses. If readers are interested in the foliar data, it is easy enough to combine the two tables. We prefer to keep it as it is.

574 (now line 615): section 2.9 is mostly on the future, as there is no data yet

Correct.

585-590 (now lines 626-631): what is the purpose of mentioning what has been done without summarising the results, but only referring the reader to the publication?

Here we aim to show that the data are applicable for a wide range of scientific studies, as exemplified by the recent publications. We think the interested reader will be triggered to look up the publications. We think it would be too much to summarise all papers here too.

3.4 Demonstration:

620-646 (now lines 661-687): nice example. I would add NEE (FC+ST) to the plots of fig. 14 for a more straightforward comparison, however by mentioning that no ustar filtering and gapfilling are applied yet at this stage (only for demonstrative purposes). Also, please mention that no data cleaning was applied, and a consideration on the fact that the ustar filtering would remove most of the night data of this example, in particular on the third night. It is also interesting to see this smaller below-canopy accumulation at sunset, when the ustar goes down

We added ‘Here we show the eddy covariance fluxes without post-processing (u_* filtering and gapfilling) as output from EddyPro.’ to the caption of Figure 14. In the center panel of the figure, we add the total flux (NEE):

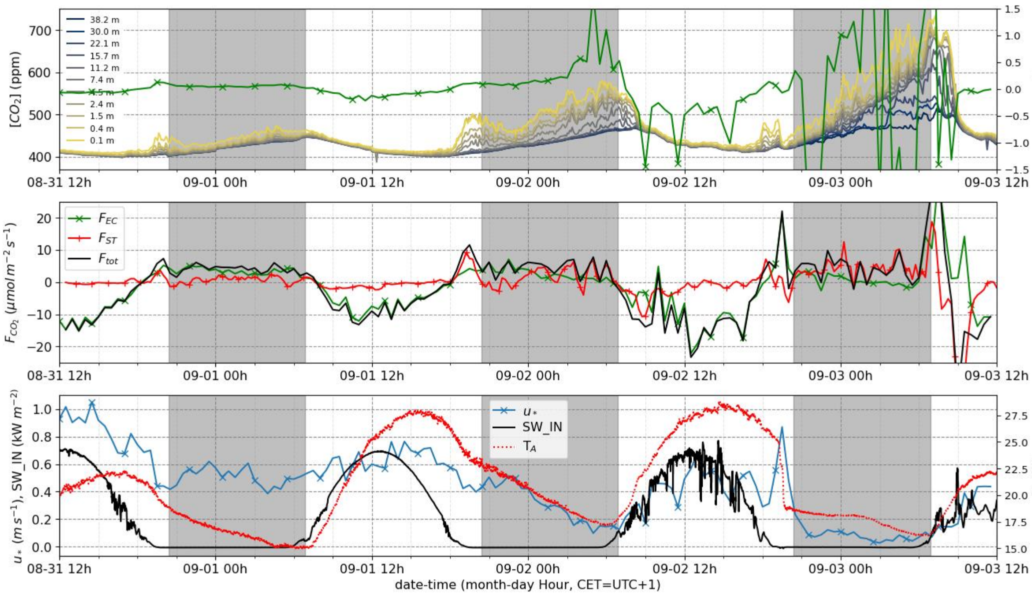

Figure 1. Top: CO_2 concentrations measured in the profile along the tower. Middle: Eddy covariance flux, storage flux and total flux (NEE). Here we show the eddy covariance fluxes without post-processing (u_* filtering and gapfilling) as output from EddyPro. Bottom, left axis: u* and incoming shortwave radiation, right axis: air temperature at 2.4 m. These data were measured between 31 August and 3 September 2024. The shaded areas represent the time between sunset (20:24h CET) and sunrise (06:54h CET).

In line 689, we add: 'Note that u* filtering would flag most of the nocturnal data, particulary in the 3rd night.'

625 (now line 666): 0.1-0.2 instead of 0.4?

Yes, correct, this should be 0.1-0.2. We corrected this.

649-658 (now lines 691-700): as you mention it, please add WS profile plot.

We cannot find a reference to the WS profile. We can add it (see below). It shows that wind speed is consistently higher above the canopy than below, as expected. Wind speed at canopy height (WS_2_2_1) is somewhat larger than below canopy. However, we are not entirely sure how adding this figure to the paper would help the analysis.

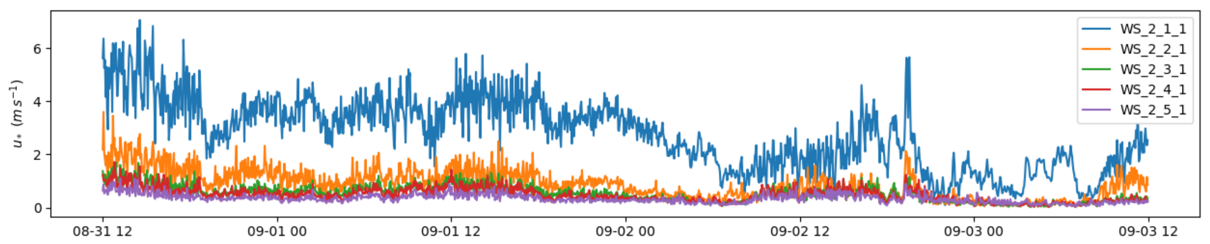

Figure: Wind speed profile, which could be added to Fig. 14.

667 (now line 710): fig 14: consider adding (in a separate plot if too crowded) also the storage of T: could it be a proxy for the impact of different T?

Temperature and moisture storage fluxes are typically much smaller than the CO_2 storage, relative to the total fluxes (see figure below) and the shape of the CO_2 storage flux has quite a different shape than the ones of temperature and water. We do not really understand what you are aiming at? We do not think the heat storage term could help explaining the CO_2 storage term.

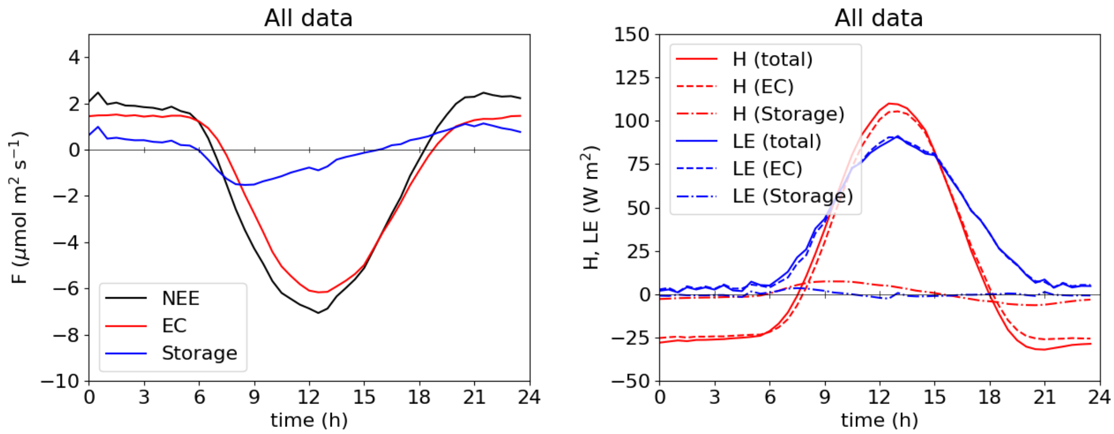

Figure: Eddy covariance and storage fluxes of CO_2 (left) and heat and water (right), represented as mean diurnal cycles.

674-675 (now lines 719-720): please mention that, despite a slight displacement between the two towers, you expect the footprint to basically overlap and look at the same vegetation patches (this should also be mentioned at the beginning)

Yes, good idea!

We add: '**Despite a small distance (< 20 m) between the two towers, the flux footprints of the two towers overlap and cover the same vegetation type.** The diurnal cycles show much agreement **in the four days shown**, even at nighttime, when below sensor storage could be different for the two systems.'

Please note that this paper was first submitted in June 2025. In the time between submission and publication, we have worked up all our data for the new Fluxnet release and published a paper in Global Change Biology about long-term trends in total ecosystem respiration (https://doi.org/10.1111/gcb.70849). Figure 1 in that paper shows a discontinuity in the TER around the time of the transition of the first to the second tower. In the supplement we go at length to try to explain this discontinuity, but without success. Ultimately, we conclude that the period of record of the second tower is still too short to tell. Since this paper was submitted before the GCB paper, this is not mentioned yet in this section. In our response to your 'Final recommendations' #4, we go into depth about this, so we'd like to refer you to that section.

676 (now line 723): the diurnal cycles show much agreement "in the 4 days selected". However, only visual comparison may not be sufficient to understand how the diurnal cycles match.

This was addressed in the previous comment.

677 (now line 724): why? I think the fact that the storage is different for the two systems would not automatically imply that Fc is different

We think the observations could be slightly different because the second tower has 11 levels and the first tower 'only' 5. And if there are differences they need to be at night, because storage is near 0 at daytime.

679 (now line 725): please note qc was not introduced.

That is right. We were thinking this would be general knowledge to interested readers. But indeed good to add a reference. We add:

'Quality flags quantify the quality of the flux data (Vitale et al., 2000).'

And we add Vitale et al., (2000) to the list of references.

684 (now line 730): please also mention that differences may be due to several reasons, like: 1. Different position/footprint (hopefully not much); 2. different sensors; 3. different processing (this can be checked)

True. We add:

'Differences could occur due to differences between sensors, different heights of the sensors, affecting flux footprint, frequency corrections, and different flux processing.'

3.5 Outlook:

690 (now line 792): in this section please clearly separate what is already in place and what is target/goal/ambition

This indeed makes the section clearer and more to-the-point. We added information about ongoing and recently awarded projects. We also decided to merge and remove a few bullet-points.

The section originally was:

'1. **Advancing Carbon Cycle Science**

The installation of a second flux tower at Loobos marks a major step toward its labelling as an ICOS Class 2 Ecosystem Station. This enables us to extend the long-term carbon sequestration record initiated in 1996 (Zhao et al., 2025), capturing the forest's response to gradual, abrupt, and intermittent climate variability—including heatwaves, droughts, and pollution episodes. Our data will feed into the ICOS Carbon Portal, supporting a wide range of continental and global carbon budget assessments.

2. **Linking Air Quality and Ecosystem Functioning**

By integrating measurements of VOCs, NOx, O_3, and NH_3, we investigate how fertilization, acidification and air pollution influence carbon uptake and forest health. Loobos is uniquely positioned downwind of one of the world's largest nitrogen-emitting regions, making it an ideal site to monitor the effects of evolving emission policies. Together with ICOS stations in Cabauw and Lutjewad, Loobos forms a strategic transect for studying sectoral emission impacts.

3. **Quantifying Forest Water Dynamics**

We are measuring the forest's water consumption and yield to address growing concerns about hydrological imbalances in the Veluwe region—a critical source of drinking water. Our observations will provide a scientific foundation for advising policymakers on the role of forest type and management in sustainable water harvesting.

4. **Enabling Fundamental Ecosystem-Atmosphere Research**

Loobos serves as a platform for advanced research into canopy-atmosphere exchange processes. We aim to deepen our understanding of radiation, heat, water, momentum, and trace gas fluxes—particularly the role of in-canopy gradients in shaping photosynthesis, stomatal behaviour, and deposition dynamics.

5. **Model Validation and Development**

Our goal is to generate comprehensive datasets for validating next-generation multi-layer canopy models, including large-eddy simulations of turbulent exchange (Pedruzo-Bagozfgoitia et al., 2023) and the MLC-CHEM model (Ganzeveld et al., 2002; Visser et al., 2022).

6. **Coupling Canopy and Boundary Layer Meteorology**

Understanding the interactions between canopy processes and boundary layer dynamics is essential for improving weather and climate models, especially those incorporating dynamic vegetation (Boussetta et al., 2013).

7. **Supporting Satellite and Aircraft Observations**

Loobos plays a key role in satellite retrieval validation, in synergy with the Ruisdael/WUR research aircraft (PHWUR), enhancing the accuracy of remote sensing products.

8. **Experimental DTS setup**

The infrastructure is attractive to test experimental setups, such as the Distributed Temperature Sensing setup, installed by Delft University (Miriam Coenders and Gijs Vis)

9. **Hosting Targeted Field Campaigns**

With its extensive infrastructure, Loobos is a prime location for field campaigns focused on boundary layer exchange and remote sensing calibration. Recent and ongoing campaigns include the canopy - boundary layer exchange experiment (Faassen et al., 2025), a 2024 HyPlant mission and GNSS studies by Delft, Twente, and Potsdam Universities.

10. **Fostering Interdisciplinary Collaboration**

Loobos brings together expertise from carbon cycle science, hydrology, ecology, soil science, forestry, meteorology, and air quality. This interdisciplinary approach is vital for understanding ecosystem services—such as carbon storage, water regulation, and biodiversity—under increasing anthropogenic pressure and climate change.'

And we changed it into:

1. '**Advancing Carbon Cycle Science**
   The installation of a second flux tower at Loobos marks a major step toward its labelling as an ICOS Class 2 Ecosystem Station. This enables us to extend the long-term carbon sequestration record initiated in 1996 (Zhao et al., 2026), capturing the forest's response to gradual, abrupt, and intermittent climate variability—including heatwaves, droughts, and pollution episodes. The combination of flux measurements and vegetation characterisation makes a significant contribution to the advance of carbon cycle science. Our data feed into the ICOS Carbon Portal, supporting a wide range of continental and global carbon budget assessments. Being part of an international infrastructure will increase the station's visibility and facilitate our ambitions by providing robustness, standardisation, a long-term horizon and international collaboration. We are

in the process of collecting a long-term dataset. An example of its analysis can be found in van der Molen et al. (2026k) who analyse the long-term trend in total ecosystem respiration.

2. **Linking Air Quality and Ecosystem Functioning**
By integrating measurements of VOCs, NOx, O_3, and NH_3, we investigate how fertilization, acidification and air pollution influence carbon uptake and forest health. Loobos is uniquely positioned downwind of one of the world's largest nitrogen-emitting regions, making it an ideal site to monitor the effects of evolving emission policies. Together with ICOS stations in Cabauw and Lutjewad, Loobos forms a strategic transect for studying sectoral emission impacts. We have installed instrumentation for O_3 and VOC fluxes and are in the process of developing operational data processing tools. Ammonia fluxes were experimentally measured from 24 August 2023 to 5 September 2024, which resulted in a much higher estimate of the deposition rate (Melman et al., 2026). We are applying for funding to measure NH_3 and NO_x fluxes on an operational basis to establish a timeseries quantifying the effect of expected nitrogen emission policy changes and to provide data needed for improvements of deposition models. A PhD position starts in August 2026 to explore microplastics concentration and flux measurements. A fast response Portable Optical Particle Spectrometer (POPS) particulate matter sensor is on loan from Delphine Farmer (Colorado State University).

3. **Quantifying Forest Water Dynamics**
We are measuring the forest's water consumption and yield to address growing concerns about hydrological imbalances in the Veluwe region, a critical source of drinking water. Our observations will provide a scientific foundation for advising policymakers on the role of forest type and management in sustainable water harvesting. We are currently partner in the public-private 'Recharge' project (https://tki-recharge.nl/) with drinking water companies and water authorities to study groundwater recharge in the Veluwe area and the relation with climate and ecosystem types.

4. **Enabling Fundamental Ecosystem-Atmosphere Research**
Loobos serves as a platform for advanced research into canopy-atmosphere exchange processes. Faassen et al. (2025) show how boundary layer concentrations are affected by the interaction of surface and entrainment fluxes. Understanding the interactions between canopy processes and boundary layer dynamics is essential for improving weather and climate models, especially those incorporating dynamic vegetation (Boussetta et al., 2013).We aim to deepen our understanding of radiation, heat, water, momentum, and trace gas fluxes—particularly the role of in-canopy gradients in shaping photosynthesis, stomatal behaviour, and deposition dynamics.

   Our goal is to generate comprehensive datasets for validating next-generation multi-layer canopy models, including large-eddy simulations of turbulent exchange (Pedruzo-Bagozfgoitia et al., 2023) and the MLC-CHEM model (Ganzeveld et al., 2002; Visser et al., 2022). The observations and model development will be (partly) supported by the Ruisdael Observatory Virtuel Research Environment (ROVRE) awarded by NWO (2026).

5. **Supporting Satellite and Aircraft Observations**
   Loobos plays a key role in satellite retrieval validation, in synergy with the Ruisdael/WUR research aircraft (PH-WUR), enhancing the accuracy of remote sensing products. We are a partner in the recently awarded ASCENT project (Advancing hyperSpectral imaging for Cross-scale Ecosystem fuNctioning and vegeTation research, NWO, 2026b), where a consortium of Dutch universities will measure and upscale hyperspectral data, such as Sun-Induced-Fluoresence radiation on leaf, ecosystem and regional scales. Loobos, as an ICOS station, also plays its role as ground truth in regional remote sensing studies, such as the NUBICOS associated to ICOS (ICOS Carbon Portal, 2026).

6. **Experimental DTS setup**
   The infrastructure is attractive to test experimental setups, such as the Distributed Temperature Sensing setup, installed by Delft University (Miriam Coenders and Gijs Vis). We are applying for funding to enhance our activities in this field.

7. **Hosting Targeted Field Campaigns**
   With its extensive infrastructure, Loobos is a prime location for field campaigns focused on boundary layer exchange and remote sensing calibration. Recent and ongoing campaigns include the canopy - boundary layer exchange experiment (Faassen et al., 2025), a 2024 HyPlant mission and GNSS studies by Delft, Twente, and Potsdam Universities.

8. **Fostering Interdisciplinary Collaboration**
   Loobos brings together expertise from carbon cycle science, hydrology, ecology, soil science, forestry, meteorology, and air quality. This *interdisciplinary* approach is vital for understanding ecosystem services—such as carbon storage, water regulation, and biodiversity—under increasing anthropogenic pressure and climate change.'

700 (now line 803): being part of a research infrastructure will also amplify the impact of the station and likely facilitate the ambitions mentioned above, by providing robustness, standardisation and long-term horizon

Yes, this is indeed true and already happening in a pleasant and natural way. Please see our previous comment, where we added 'Being part of an international infrastructure will increase the station's visibility and facilitate our ambitions by providing robustness, standardisation, a long-term horizon and international collaboration.' to this bullet point.

702 (now line 809): in the manuscript there is no mention of NOx measurements made in Loobos, only regional estimates. For NH3, two campaigns are mentioned, but it is not clear how long they lasted, and the results of just one are reported (the annual NH3 deposition), as the other one was a test on the setup. Eddy covariance for such gases is challenging, and the effort for making these campaigns is highly valuable: but if you think this represents a milestone for Loobos, probably they deserve more attention in the manuscript?

Ammonia flux measurements were measured on an experimental basis between 24 August 2023 and 5 September 2024. The experiment turned out to be successful and is extensively described and analysed in Melman et al., 2026 (https://doi.org/10.1016/j.agrformet.2026.111358). See also your comment to line 690: we added 'Ammonia fluxes were experimentally measured from 24 August 2023 to 5 September 2024, which resulted in a much higher estimate of the deposition rate (Melman et al., 2026). We are applying for funding to measure NH_3 and NO_x fluxes on an operational basis to establish a timeseries quantifying the effect of expected nitrogen emission policy changes and to provide data needed for improvements of deposition models.' to this bullet point.

713 (now line 834): the in-canopy part is more a future target that you have: could you describe your plan in more details?

In the Meteorology and Air Quality group, Wageningen University, we are developing high-resolution turbulence models (DALES and MicroHH). In these models, the representation of below-canopy turbulence as well as exchange processes and below-canopy chemistry becomes increasingly important. We have recently purchased a spare eddy covariance system and we are developing fast response, spectra-resolving radiometers. In combination with the vertical profile measurements, the ones in place for CO2 and H2O, and the ones we hope to establish with the funding we applied for, we will be able to provide the required data. A new Post-Doc and an exchange researcher will work on the model development. So, our plans are there, execution is pending granting of applied proposals. As we are in the process of making this concrete, it is a bit early to already publish about it in concrete terms.

715 (now line 835): also a goal. This could be also extended to other models to be developed in the future

Yes, indeed. We merged this bullet point with the preceding and following ones and made it more concrete, see our response to your comment to line 690.

721 (now line 832): please add a sentence on the fact that the combination of flux data and vegetation characteristics can give a significant contribution in this direction

Yes, that is very true. We decided to remove this bullet point, but we add 'The combination of flux measurements and vegetation characterisation makes a significant contribution to the advance of carbon cycle science.' to the first bullet point.

722 (now line 848): here you could provide more details if this is something peculiar to Loobos, otherwise (or in addition) mention that Loobos, like all the ICOS stations, provide valuable datasets for the calibration and validation of satellite products (also via the NUBICOS project).

Good point. We added: 'Loobos, as an ICOS station, also plays its role as ground truth in regional remote sensing studies, such as the NUBICOS associated to ICOS (ICOS Carbon Portal, 2026).'

726 (now line 851): there is not much info in the manuscript about DTS, only 4 lines mentioning the target of this project: I would merge points 8 and 9 as collaborations and field campaigns where you recap them all

3.6 Data availability:

738 (now line 865): please be more specific on which of the data you mentioned above are present in the CP as NRT product (updated daily) with the possibility of plotting the variables; and which ones are available on your webpage (also and only)

It is right that we should be clear about which data are available via which platform. In order to better specify, we rewrote the data availability section from:
'All data described in sections 2.2-2.4 and 2.6 are uploaded to https://MAQ-Observations.nl

(last access 4 February 2026) every 30 minutes and to the ICOS Carbon Portal (2026) on a daily basis. Our data is available in near real-time, including customizable charts, at MAQ-Observations. The data are available for use according to a CC-BY 4.0 data licence. Any used data must be cited accordingly, please refer to https://maq-observations.nl/maq-data-license/ (last access 4 February 2026) and https://www.icos-cp.eu/how-to-cite (last access 4 February 2026). On the ICOS Carbon Portal we recommend to look for the L2 product for previous years and to the Near-Real-Time (NRT) data product for the ongoing year. The PhenoCam data are accessible via https://phenocam.nau.edu/webcam/sites/nlloobs/ (last access 4 February 2026). All data described in other sections are available for use according to the same licenses too, please contact the authors of this paper to obtain access.'

Into:

'The data collected with the infrastructure we describe in this paper are available for interested people via a CC-BY-4.0 license. We distinguish data collected according to ICOS instructions and non-ICOS data.

The ICOS data are

- the radiation data described in section 2.2. Note that the Phenocam data are available via the NAU Phenocam Network site until May 2026 and via the ICOS Carbon Portal afterwards;
- the continuous soil measurements in section 2.3;
- the precipitation, air pressure, temperature, humidity and backup measurements (section 2.4);
- The ICOS compliant eddy covariance measurements of CO_2 and H_2O (section 2.5);
- The vertical profiles of CO_2, H_2O and temperature and windspeed (section 2.6), except the leaf wetness data;
- The intermittent ecosystem measurements (section 2.8);

All ICOS data are available via the ICOS Carbon Portal (https://www.icos-cp.eu), more specifically the Loobos ecosystem site's page https://meta.icos-cp.eu/resources/stations/ES_NL-Loo. In general, ICOS publishes

- level 0 data: the raw data we submitted,
- level 1 data: The Near-Real Time (NRT) data, which represents intermediate observational data, quality controlled and updated on a daily basis
- level 2 data: the final quality observational controlled data. Level 2 data are updated on an annual basis.

ICOS offers downloads for all data and previews of key variables in the level 1 and 2 data.

Our own https://MAQ-Observations.nl (last access 4 February 2026) offers download and visualisation of the ICOS level 0 data, including eddy covariance fluxes processed with EddyPro. Additionally, it provides the vertical profile leaf wetness data (section 2.6).

Other non-ICOS data include the ozone and VOC concentration and flux data and NO_2 concentration concentration data. The processed ozone concentration and flux data are available via https://doi.org/10.17887/WUR01-6UGU9F (van der Molen et al., 2026j) and will be updated on an annual basis. The VOC data will be made available via the same yoda.wur.nl platform as the ozone data. Raw (20Hz) ozone and VOC data and all other data not (yet) available via the ICOS Carbon Portal or MAQ-Observations.nl is available upon request via the authors.

In general, we advice to download data preferably from the ICOS Carbon Portal as level 2 data if available, otherwise as level 1 data or level 0 data, because ICOS offers high-quality quality control and gap filling and provides persistent data identifiers.'

748 (now line 905): please mention that the data were downloaded from the CP as indicated in the reference link at line 742 (now line 899)

We do not understand. In Line 748 (now line 905) we provide a download link. Anyone who tries to download the data via that link will be automatically asked to agree with the ICOS data license.

749 (now line 906): please note that this link points to a deprecated version of the L2 dataset: if this is the one you used it is totally fine, but please note that in this case the year of the citation is 2025, not 2026. I think this is the case for all the links of this section, please check them all

Yes, we agree, but we propose to do this briefly before proofreading of a revised version.

3.7 References:

849 (here and in the text) (now line 1014): this is not the proper way of citing the ICOS Instructions. Each document has its own citation and doi, e.g. "Gielen, B., Iserbyt, A., Op de Beeck, M., Michilsens, F., & Papale, D. (2017). ICOS Ecosystem Instructions for Ancillary Vegetation Measurements in Forest (Version 20260201). ICOS Ecosystem Thematic Centre. https://doi.org/10.18160/4ajs-z4r9".

Because ESSD offers the option to refer to websites, we thought it would be more transparent to refer to the general landing page for ICOS instructions, instead of to each document separately. The individual documents will eventually be updated by newer instructions, new documents will be added and some may become obsolete.

906-915 (now line 1087-1104): please check the links and the year of publication of the datasets

Yes, we will do this just before submission of a final revised version, otherwise it will be outdated again (which it will anyway, but the links will tell that the link is deprecated and point towards the latest version.)

3.8 Supplementary:

26 (now line 28): having the 1:1 in figure S.3 line may help the reader to have a better sense of the differences between L2 and NRT agreement. Also, how can R2 in the left plot be equal to 1?

We added the 1:1 lines in the figures as red dashed lines:

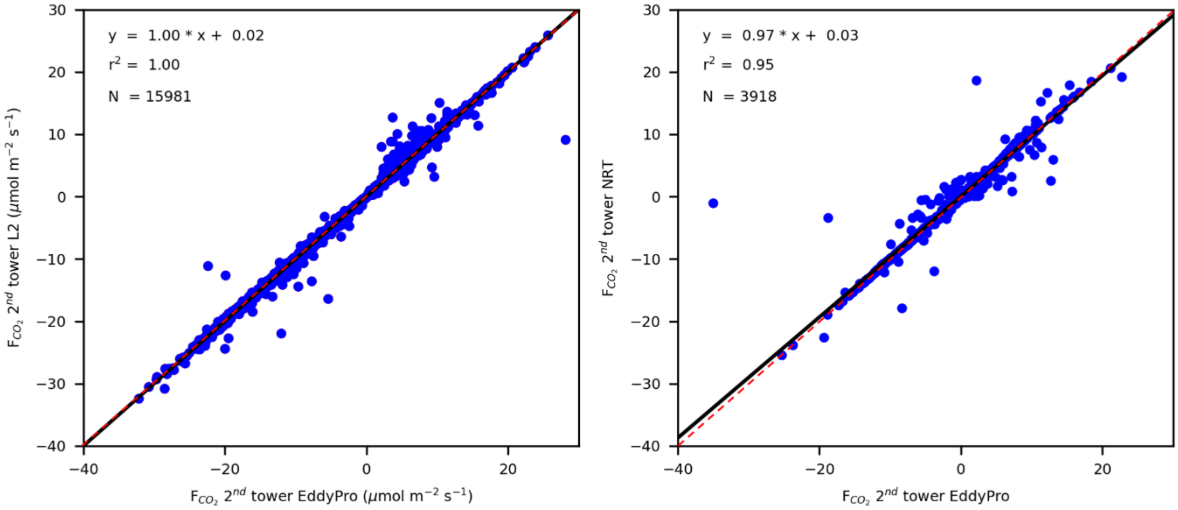

Figure S.2. Comparison of the CO2 fluxes processed by our own EddyPro system and the ICOS ETC L2 product (right, 10 May 2023 to 31 December 2024) and the ICOS ETC Near Real Time (NRT) product (left, 1 January 2025 – 22 May 2025). The red dashed lines represent the 1:1 lines.

The deviations from the 1:1 line in the left figure are only a few points, relative to the 15981 data points, resulting in a r^2 rounded to 1.00.

3.9 Final recommendations

My recommendation is then to have a major revision, where the authors are requested to:

1. make clear what is part of ICOS measurements, and what is not, and restructure the text accordingly so that the reader can have a clear overview of what is done in ICOS and how (reference to the Instructions), and what is out of ICOS and how it is done. What was missing in the first tower to be ICOS-compliant? What is the importance of being part of a standardised network, and at what cost and drawbacks?

In general, the vast majority of the measurements represent ICOS measurements. The only exceptions are:

- the leaf wetness measurements (section 2.6)
- The additional eddy covariance measurements of ozone and VOC (section 2.7).

In an early version of the manuscript, we tried to strictly order by ICOS and non-ICOS measurements, but that resulted in a strange paper structure. We don't think there is a single-best way to approach this, but we consider the way we do it now sufficiently clear. Note that the description of the leaf wetness sensors already mentions that these data are non-ICOS. We will ad a statement to section 2.7 too: 'Note that these observations are not standard ICOS measurements.'

In response to your comment to line 738 (Data availability) (now line 865): we rewrote that section to better clarify which data are available via the ICOS Carbon Portal and which are not.

With these changes, we think the difference between ICOS and non-ICOS is sufficiently clear.

2. Give more room to quality of the data, and be more transparent when comparing with pre-ICOS data (stressing when methods and/or sampling strategy/location and/or processing were different, highlighting the uncertainties on both cases, etc.)

We assume here you refer to section 3.2. We have responded to your comments to lines 674-675 (now lines 719-720) and 677 (now line 724) in detail. We also refer to our publication in Global Change Biology (van der Molen et al., 2026k) about long-term trends in total ecosystem respiration, including the description of the continuity of the timeseries between the two towers. There we show that the timeseries of the second tower are still short to make conclusions about discontinuities. We believe that by doing so, we are transparent about the quality of the data, while at the same time a full analysis of the reason behind apparent changes deserves dedicated analysis in a separate paper.

Please see also our detailed response to your comment 'Final Recommendations #4'.

3. extend the "Instrumentation" section to (other) theoretical insights and processing summary, as done for example for WTD, in part the storage and Ozone. This should include a short overview of the eddy covariance CO2 processing, footprint calculation, and key parameters (e.g. [z-d]/L). This would also be beneficial in explaining the differences between ICOS and pre-ICOS CO2 fluxes, that is considering the different processing steps applied.

We are not entirely sure what exactly you mean here and/or if we are on the same page. The paper under consideration here is intended as a Loobos site reference paper. We describe the ecosystem, the instrumentation and data availability. Only in a few instances where we think it is important for understanding the data (WTD, non-ICOS eddy covariance) and the results (storage fluxes), we describe the data processing. We do not intend to fully describe all data processing done by ICOS. As for the non-ICOS eddy covariance data, we focus on showing that eddy covariance flux measurements are feasible with the current setup, while we also indicate that the description of the final processing and the results needs its own publication.

4. Enlarge the section on the continuity/legacy between first and second towers: not only a single example and a visual inspection, but something more extended (there are 2 months of overlap) and with some statistics. I think it could be great to the reader to see fig 9 of Zhao paper on the first tower, completed with the new period.

Thank you for this comment. Assessing the continuity of a timeseries over a long period of time (almost 30 years) is important. Many small and some larger changes occur in the instrumentation, data processing and the ecosystem, which could cause the resulting fluxes to change, e.g. changing from a closed-path gas analyser to an open-path to an enclosed-path one. Or the change in measurement height. Since the infrastructure measures so many variables, we focus on the most important variables, the main purpose the tower had over the years: measuring carbon dioxide fluxes, the Net Ecosystem Exchange, comprising of the Total Ecosystem Respiration and Gross Primary Production, and the latent heat flux.

As mentioned earlier in our Author Response, in the period overlapping with the online publication of the discussion paper under consideration, we wrote, submitted and published a paper about the long-term trends in Total Ecosystem Respiration (van der Molen et al., 2026k, https://doi.org/10.1111/gcb.70849). There we observe a discontinuity in the timeseries around the time of the change of towers and instrumentation. In the supporting information (https://onlinelibrary.wiley.com/action/downloadSupplement?doi=10.1111%2Fgcb.70849&file=gcb70849-sup-0001-Supinfo.pdf), we go at length to attempt to explain the discontinuity. We show that it cannot be explained by 1) a change in eddy size distribution with increasing measurement height: the frequency loss correction factors are only in the order of max 4% for u* > 0.25 m s^{-1} and hence cannot explain the larger discontinuity. (See figure below. See also our response to your comment to lines 491-493 and 494);

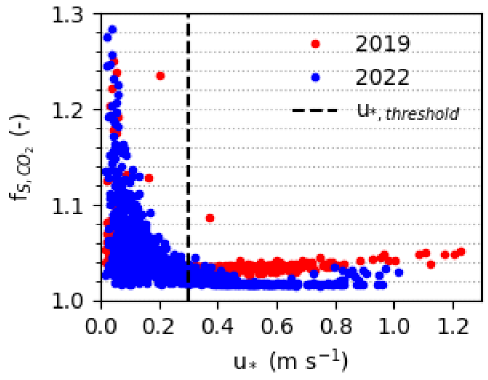

Figure: Spectral correction factors for nighttime CO_2-fluxes as a function of u_*, measured at 23 m in 2019 (on top of the first tower) and at 38.2 m in 2022 (on top of the second tower), both with the second tower's eddy covariance setup. In both years we used data in June and July. The vertical dashed line indicates the u_* threshold value.

2) by a change in flux footprint: the footprint is homogeneous and the respiration rate does not depend on footprint size (see figure below);

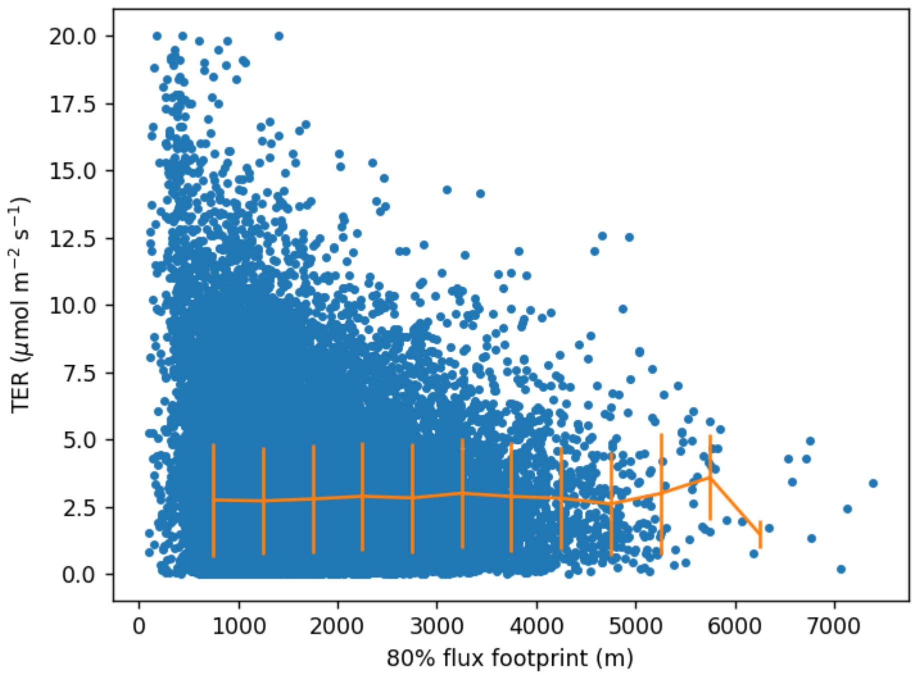

Figure: Total Ecosystem Respiration (TER) as a function of flux footprint distance in the second tower. The orange line with errorbars indicates the mean and standard deviation. Although the apparent variability in TER is larger with smaller footprint size, the mean and standard deviation are constant, because there are more data points at smaller distances.

Or by 3) a change in effective threshold u_* value with decreasing effective measurement height: implementing a variable threshold value does not change the fluxes considerably. Our conclusion is that we cannot yet explain the discontinuity with the second tower's dataset still short and we do not know if the observed trend continues.

Interestingly, from 15 May to 7 September 2019, before we built the second tower, we did a test run of the new tower's eddy covariance system in the first tower, alongside the first tower's own eddy covariance setup (Zhao et al., 2026). Both systems were installed at a height of 27 m, within 2.5 m from each other. In the figure below, we show three hourly mean fluxes with selection criteria qc_co2_flag = 0 or 1 (best), $u*$ > 0.3 m s^{-1}, global radiation > 10 W m^{-2} for daytime and < 2.0 W m^{-2} for nighttime data. Although not too many half hours pass the selection criteria, the agreement between the two measurement systems is satisfactory. Confusingly, the 2nd system now has somewhat smaller fluxes than the 1st tower system (as opposed to Fig. 1 in van der Molen et al., 2026k).

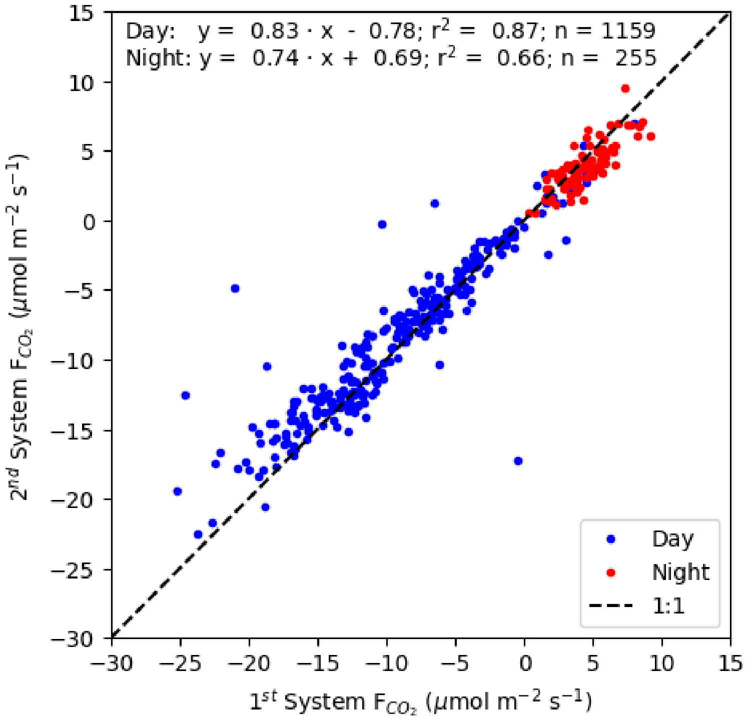

Figure: Comparison of CO_2 fluxes measured at the top of the first Loobos tower (27 m), with the 1st tower's eddy covariance system (x-axis) and the 2nd tower's eddy covariance system (y-axis), between 15 May to 7 September 2019 with selection criteria qc_co2_flag = 0 or 1 (best), $u*$ > 0.3 m s^{-1}, global radiation > 10 W m^{-2} for daytime and < 2.0 W m^{-2} for nighttime data.

Since the above discussion has been published, we do not want to repeat it in this paper, but indeed we will explicitly refer the readers to van der Molen et al., 2026k.

With respect to your request to 'Enlarge the section on the continuity/legacy between first and second towers', we agree that a comparison is most important, although attempts to explain apparent discontinuities quickly become lengthy. In ESSD data articles, 'any interpretation of data is outside the scope of regular articles' (https://www.earth-system-science-data.net/about/aims_and_scope.html).

Considering that continuity of timeseries is important and that the paper needs to remain concise and focused, we propose to:

- Refer the reader to van der Molen et al., 2026k for a presentation and discussion on the TER timeseries
- Include the above figures of spectral correction factors as a function of u_*, the CO_2 flux as a function of flux footprint distance and the comparison of CO2 flux between the two systems in the first tower in section 3.2 with a description and discussion
- Include a figure showing the continuity of GPP and LE (see below)

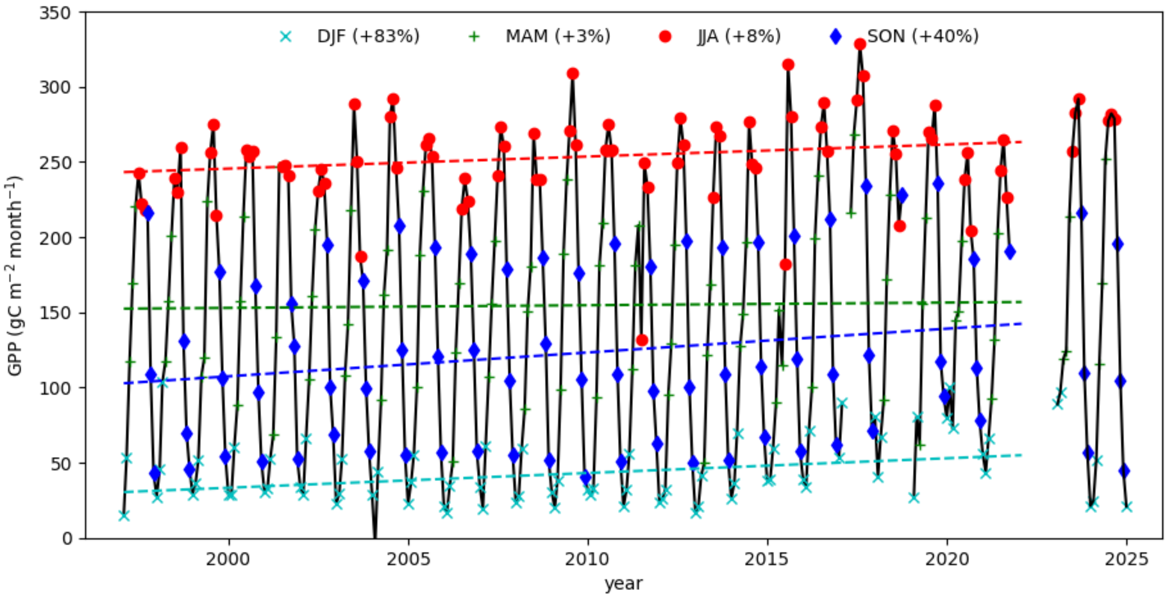

Figure: Trend in GPP per season. The markers show monthly sums. The colours combine the months into seasons as indicated in the text within the figure. The dashed lines indicate the linear regression lines of seasonally mean GPP. The percentages shown in the legend indicate the change in GPP from 1997 to 2021 relative to 1997. From 1997 to 2021, the GPP from NEE partitioning is shown (Zhao et al. 2026). The post 2021 data represent GPP from the ICOS FLUXNET half-hourly product from 2023 to 2024 (van der Molen et al. 2026l).

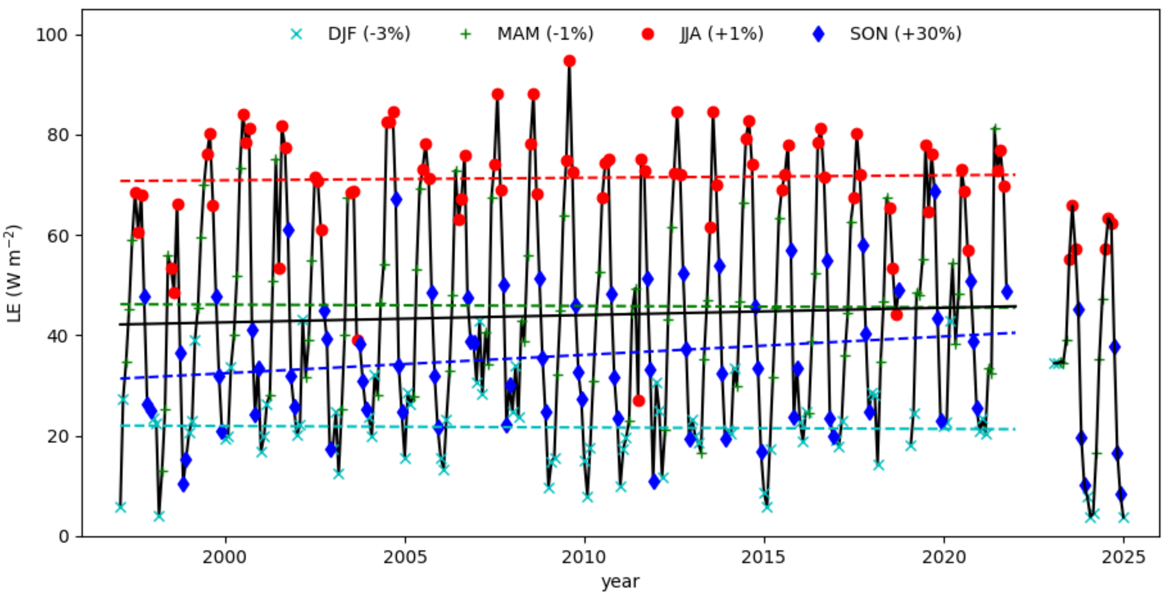

Figure: Trend in Latent Heat flux (LE) per season. The markers show monthly means. The colours combine the months into seasons as indicated in the text within the figure. The dashed lines indicate the linear regression lines of seasonally mean LE. The percentages shown in the legend indicate the change in LE from 1997 to 2021 relative to 1997. From 1997 to 2021, the gapfilled LE from Zhao et al. (2026) is shown. The post 2021 data represent LE (LE_F_MDS) from the ICOS FLUXNET half-hourly product from 2023 to 2024 (van der Molen et al. 2026l).

- Leave detailed in-depth analysis and interpretation of (dis)continuities to subsequent science papers. This may actually be something to address on a somewhat larger scale as part of a community effort within FLUXNET and/or ICOS.

With this, we hope we have addressed your request appropriately.

We modify section 3.2 into: (N.B.: The original text can be found in the submission).

‘3.2 Continuity of NEE timeseries from the first tower to the second

The second tower (since 2021) is intended to continue the first tower measurements (1996-2023) and to build a continuous long-term timeseries. Since the measurement heights and instrumentation (open path and closed path gas analyser) were different, it is relevant to verify if the fluxes measured by the systems in the two towers match. Unfortunately, the gas analyser in the first tower suffered a breakdown for a long period after the second tower’s eddy covariance system was installed. However, we do have a 2 month period of overlapping data in between 28 July and 29 September 2021. Despite a small distance (< 20 m) between the two towers, the flux footprints of the two towers overlap and cover the same vegetation type. The total ecosystem respiration does not vary with footprint distance (Fig. 2). Fig **Error! Reference source not found.** shows the level to which the two datasets agree. The diurnal cycles show much agreement in the four days shown, even at nighttime, when below sensor storage could be different for the two systems. With a r^2 of 0.74 there is some random variability between the datasets, but the slope is close to 1 and the offset small relative to the magnitude of the fluxes. Quality flags quantify the quality of the flux data (Vitale et al., 2000). Here only data with quality flag 0 have been selected, representing the best quality data. Selecting data with quality flags 1 and/or 2 will result in a significantly worse match. Since the second tower was not yet labelled as an ICOS site, we use our own EddyPro results in this comparison. Fig. S.3, however, shows that there is a near 1:1 correspondence between our own EddyPro calculations and the ICOS ETC L2 and Near Real Time (NRT) results. Differences could occur due to differences between sensors, different heights of the sensors, affecting flux footprint, frequency corrections, and different flux processing.

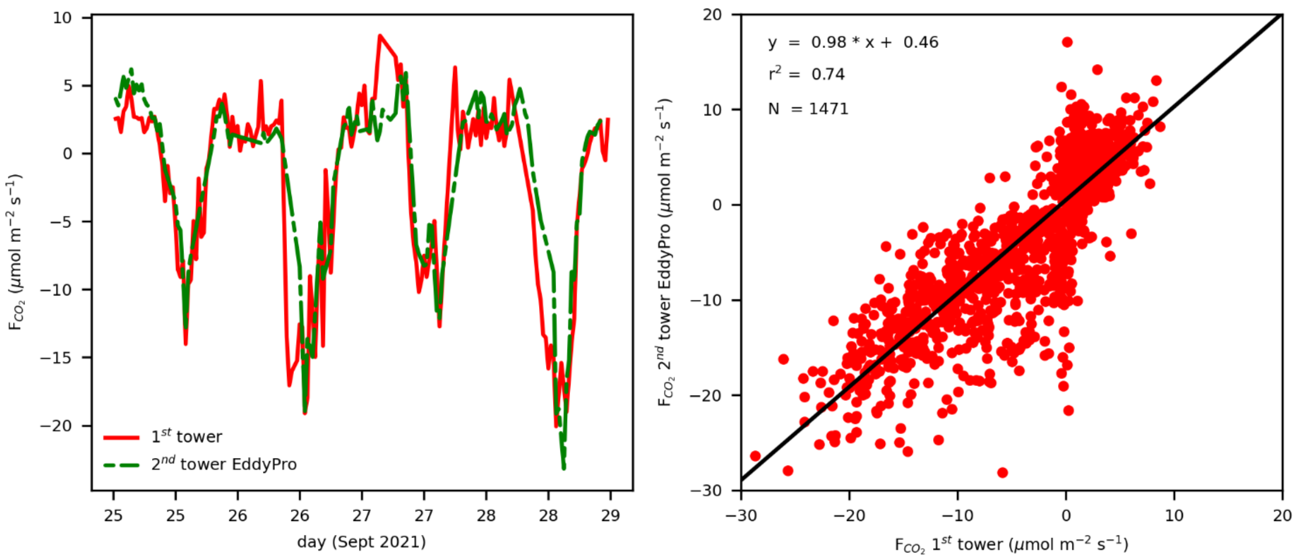

Figure 15. Comparison of the eddy covariance CO_2 fluxes measured by the first tower (Zhao et al., 2026) and the second tower. Left: the diurnal cycles over a subset of the two month period. Right: the correlation between the two datasets. Here only data with quality flag 0 have been selected.

Looking at a longer timescale, van der Molen et al (2026k, Fig. 1), however, show a discontinuity in monthly mean total ecosystem respiration around the time of the transition from the first to the second tower. In the supporting information, they attempt to explain the discontinuity by 1) a change in eddy size distribution with increasing the measurement height from z = 27 m to z = 38.2 m (In the first tower, z-d changes from 16.8 m in 1997 to 13.5 m in 2021, in the second tower z-d is 24.7 m, where d is the zero-plane displacement height, taken as 2/3rd of the canopy height): the frequency loss correction factors are only in the order of max 4% for u* > 0.25 m s^{-1} (Fig. Figure) and hence cannot explain the larger discontinuity; 2) by a change in flux footprint: the footprint is homogeneous (Fig. **Error! Reference source not found.**, section **Error! Reference source not found.**) and the respiration rate does not depend on footprint size (see Fig. 2); or by 3) a change in effective threshold u_* value with decreasing effective measurement height: implementing a variable threshold value does not change the fluxes considerably. Their conclusion is that they cannot yet explain the discontinuity with the second tower's dataset still too short to know if the observed trend continues.

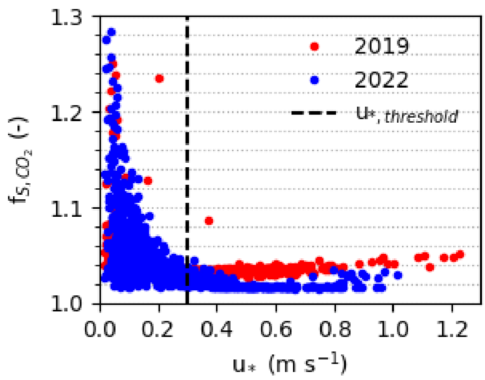

Figure 16. Spectral correction factors for nighttime CO_2-fluxes as a function of u_*, measured at 23 m in 2019 (on top of the first tower) and at 38.2 m in 2022 (on top of the second tower), both with the second tower's eddy covariance setup. In both years we used data in June and July. The vertical dashed line indicates the u_* threshold value.

Interestingly, from 15 May to 7 September 2019, before we built the second tower, we did a test run of the new tower's eddy covariance system in the first tower, alongside the first tower's own eddy covariance setup (Zhao et al., 2026). Both systems were installed at a height of 27 m, within 2.5 m from each other. In Fig. Figure , we show three hourly mean fluxes with selection criteria qc_co2_flag = 0 or 1 (best), $u*$ > 0.3 m s^{-1}, global radiation > 10 W m^{-2} for daytime and < 2.0 W m^{-2} for nighttime data. Although not too many half hours pass the selection criteria, the agreement between the two measurement systems is satisfactory. Confusingly, the 2nd system now has somewhat smaller fluxes than the 1st tower system (as opposed to Fig. 1 in van der Molen et al., 2026k).

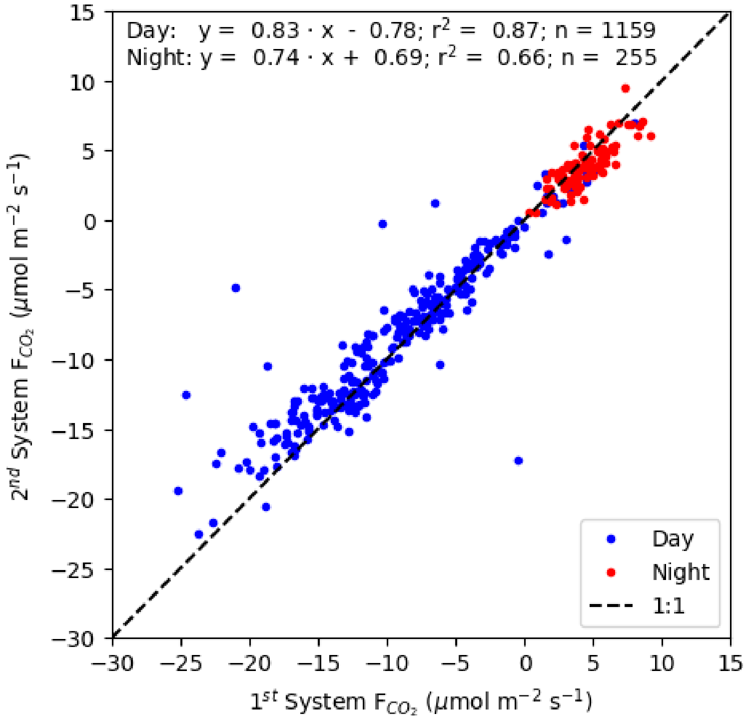

Figure 17. Comparison of CO_2 fluxes measured at the top of the first Loobos tower (27 m), with the 1st tower's eddy covariance system (x-axis) and the 2nd tower's eddy covariance system (y-axis), between 15 May to 7 September 2019 with selection criteria qc_co2_flag = 0 or 1 (best), u^* > 0.3 m s^{-1}, global radiation > 10 W m^{-2} for daytime and < 2.0 W m^{-2} for nighttime data.

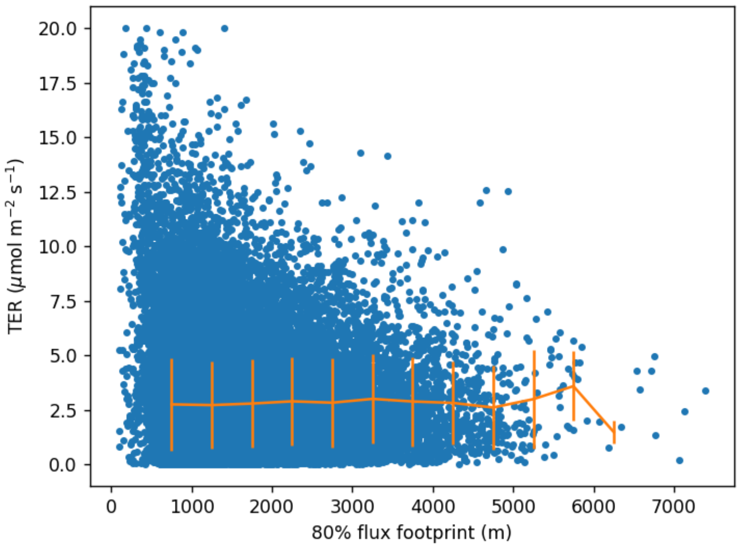

Figure 2. Total Ecosystem Respiration (TER) as a function of flux footprint distance in the second tower. The orange line with errorbars indicates the mean and standard deviation. Although the apparent variability in TER is larger with smaller footprint size, the mean and standard deviation are constant, because there are more data points at smaller distances.

In addition to the trends in TER (van der Molen et al., 2026k, Fig. 1), we show the corresponding trend in Gross Primary Production (GPP, Fig. Figur). The discontinuity observed in TER is not visible in GPP. A positive trend is visible in GPP, particularly in fall. GPP is clearly lower in the dry years 2003, 2013 and 2018. Fig. Figure shows the corresponding trends in latent heat flux (LE). Here again a discontinuity is observed after the transition to the second tower.

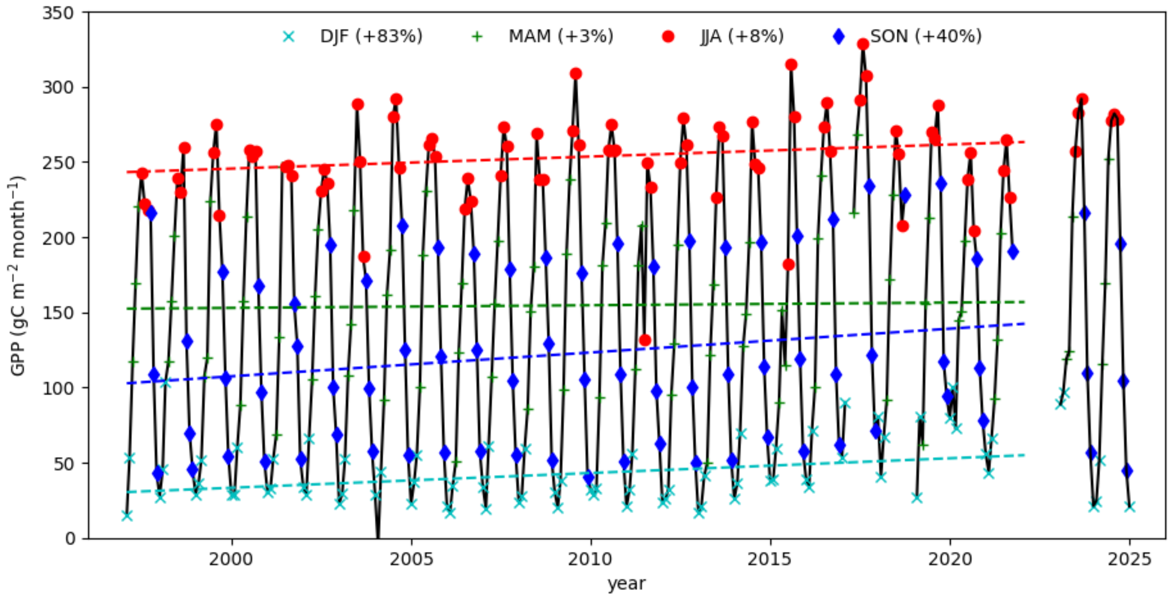

Figure 20. Trend in GPP per season. The markers show monthly sums. The colours combine the months into seasons as indicated in the text within the figure. The dashed lines indicate the linear regression lines of seasonally mean GPP. The percentages shown in the legend indicate the change in GPP from 1997 to 2021 relative to 1997. From 1997 to 2021, the GPP from NEE partitioning is shown (Zhao et al. 2026). The post 2021 data represent GPP from the ICOS FLUXNET half-hourly product from 2023 to 2024 (van der Molen et al. 2026l).

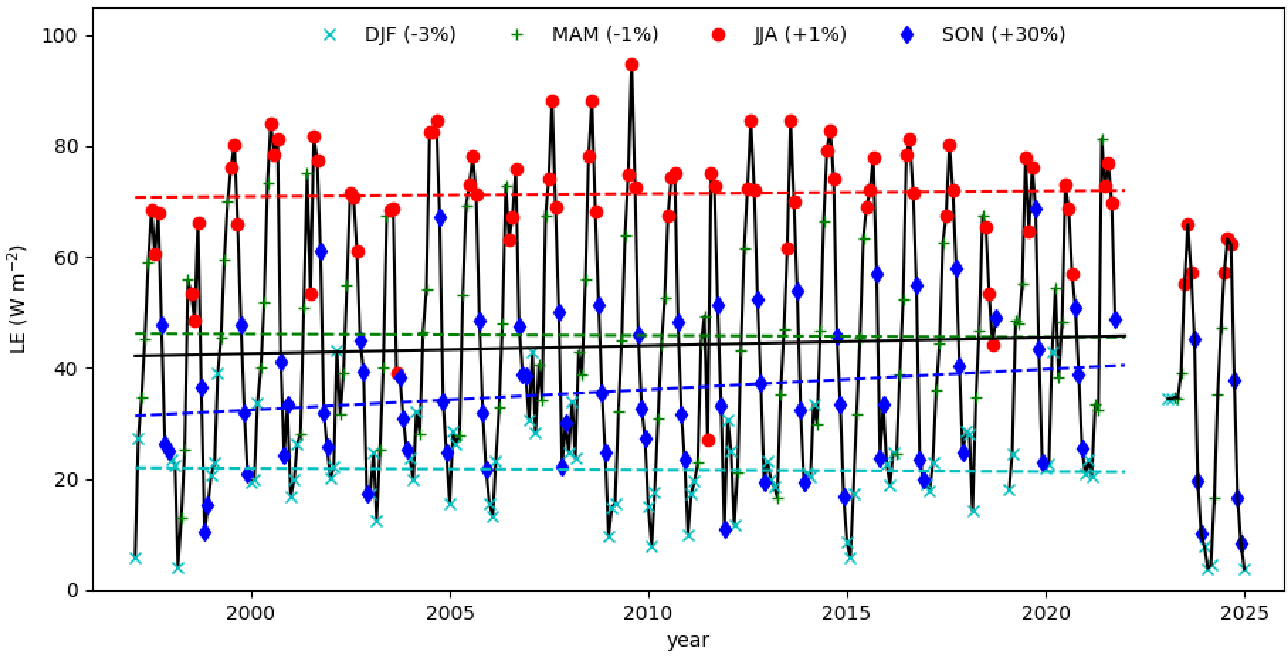

Figure 21. Trend in Latent Heat flux (LE) per season. The markers show monthly means. The colours combine the months into seasons as indicated in the text within the figure. The dashed lines indicate the linear regression lines of seasonally mean LE. The percentages shown in the legend indicate the change in LE from 1997 to 2021 relative to 1997. From 1997 to 2021, the gapfilled LE from Zhao et al. (2026) is shown. The post 2021 data represent LE (LE_F_MDS) from the ICOS FLUXNET half-hourly product from 2023 to 2024 (van der Molen et al. 2026l).

These results combined suggest that the flux measurements of the first and second tower correspond to a high degree when comparing half-hourly data, but when looking at long-term trends, additional work is necessary to explain apparent discontinuities in TER and LE.’

5. In order to make it more immediate for the reader to see the difference between first and second periods measurements, some of the findings published in Zhao et al 2026 could be reported here (e.g. table 10).

Please refer to our response to your comment to line 556bis.

6. better clarify what has been done for ammonia and N deposition at Loobos, and what is only at the regional scale: in some parts this seems a key measurement, but it is only shortly mentioned in the text.

Please see our response to your comments to line 690 (now line 792). There we explain that the experimental ammonia deposition measurements are reported in Melman et al. (2026, https://doi.org/10.1016/j.agrformet.2026.111358) and that we have applied for funding to measure ammonia and nitrogen oxides deposition on a continuous basis.

7. Increase the consistency between the outlook list, which I see as the milestone achieved or to be reached with the work done at Loobos, with what reported in the text, in particular expanding the parts that are reported as outlook but not explored in the text.

Please see our response to your comment to line 690 (now line 792), where we revised the Outlook section according to your suggestions.