First of all, we would like to thank the anonymous reviewer for her/his time and for the valuable comments.

**Major comments**

In this study Laemmel et al. presented ~5 years' worth of very high quality Δ14CH4 and Δ14CO2 measurements from Jungfraujoch, Switzerland. Excluding reconstruction from ice cores/firn air (e.g., Hmiel et al., 2020) and sporadic atmospheric 14CH4 measurements conducted during limited duration measurement campaigns (e.g., Sparrow et al., 2018) this study represent the first Δ14CH4 measurements from clean background sites for the purpose of long-term global atmospheric monitoring probably since the Barring Head measurements from New Zealand presented by Lassey et al. (2007) almost 20 years ago. Due to the lack of measurements, for years we've been using +350‰ as "background" Δ14CH4. This study presents a very welcome update to that value, and very interestingly the authors found that atmospheric Δ14CH4 have been increasing since 2019 probably due to the proliferation of PWR (pressurized water reactor) nuclear power plants.

Beyond the novelty of the Δ14CH4 measurements, this study also shows that the Bern Radiocarbon laboratory (LARA) can produce state-of-the-art high quality atmospheric 14C data that are comparable to the background measurements made within the ICOS network and other international laboratories that handle background air samples. The passive sampling system using a large volume container (JASS) is very innovative and provides a blueprint for large volume sampling that I hope can be adapted by other global monitoring observatories. Given the study site (in Central Europe), interference from nuclear power plants is unavoidable, and I think the authors did their best to correct for the influences of local nuclear power plants. The paper is very well written; I highly recommend the paper for publication and congratulate the authors on a great manuscript/study.

We are thankful for this positive comments.

I just have several general comments out of genuine curiosity for additional details that I think will help other readers, as well as some slightly nitpicky comments regarding choice of references, mostly in the introduction section (see minor comments).

1. In line 346-348 the authors mentioned: "*The standard deviation of the Δ14CH4 value for all 40 CH4 measurements over 15 months for each PAB bottle (so about 80 CH4 measurements in total) is 8 ‰ (Fig. S4a,b), which is lower than the instrumental uncertainty of a single Δ14CH4 measurement (12 ‰) indicating the satisfactory temporal reliability of our Δ14CH4 measurements*." Similar mention is repeated for Δ14CO2 in line 438-441. First, I would replace "*temporal reliability*" with "*long-term reproducibility.*" I'm also interested in how the instrumental uncertainty/single sample precision is determined. For this, hopefully the authors are willing to share the composition of samples/standards for the 39 position MICADAS "wheel" (I guess you don't use wheel in MICADAS, so batch/magazine/cartridges). How many OX-2s are used for normalization and how many replicates of the surveillance gas (the PABs cylinders) are put in each sample batch. What was the average stdev of however many (4,6,8,10) primary OX-2 standards used for normalization within a single sample batch, and whether the average stdev of primary standards was bigger/smaller than the single sample precision derived from counting statistics. Also, were the samples measured together in small amounts of 39 position sample batches/magazines (like within 6-7 batches at the minimum to account for the 130-something samples and 80 surveillance samples, or are they more spread out over larger amounts of sample batches. Knowing the composition of the 39 position sample within a single MICADAS batch will give us a good sense of the inter-batch/magazine reproducibility.

We are thankful for this comment. Concerning the use of "long-term reproducibility" instead of "temporal reliability", we agree with the reviewer and made the changes. Concerning the information about instrumental uncertainty/single sample precision, we added them in the section 2.2.

2. Moving to integrated and automated sampling with the JASS system is clearly very beneficial in terms of sampling consistency and reducing labor needed to do the grab samples. But I have a couple of follow-up questions regarding the JASS system. The grab samples clearly show some highly elevated Δ14CH4 (>420‰ cutoff) – likely given the sporadic nature of 14CH4 emissions from PWR venting. Would it be fair to say/acknowledge that with integrated sampling it is harder to correct for potential elevation from sporadic PWR venting? The footprint-based correction relies on annual, quarterly, and monthly NPP emissions – this might not capture the sporadic emissions from venting so there's still some upside to grab samples. Second, in line 132-134 the authors mentioned that *"air sampling should occur passively, i.e., without any mechanical pumps in contact with the sampled air, thereby minimizing the risk of contamination from membrane outgassing."* This is a noble principle to strive for, but is it fair to also acknowledge that one still needs to transfer the sample from the large volume container into the transport container? Reading the manuscript, in line 169-171 the authors wrote: *"At this stage, the transfer pump was evacuating the tank and after ~30 s of line flushing, a bag was connected to the line for the transfer of the sampled air. After about one hour, enough air was transferred to the bag for further 14C analyses."* So, the sample air is still in contact with mechanical pump, arguably even longer (1hr) compared to the 22-55 min needed to compress enough air during the manual grab samples (line 124). As such, the benefit of JASS sampling scheme in terms of sample cleanliness is only valid if the KNFN022 pump used for grab samples is dirtier than the KNF N922 pump used to transfer the sample from the large volume reservoir to the transfer bag.

We are grateful for these comments. Concerning the discussion grab vs integrated samples, we added some comments in the *Discussion* section. Concerning the sample cleanliness regarding the pumps, as correction, we exchanged the order of both principles (line 135) and added "as far as possible" for the passive sampling. As mentioned, taking grab samples took 20-55 min; with the JASS, it is possible to passively sample air over 84 hours (6 hours over 14 days) and transfer it into a bag within one hour. So calculating the ratio between pumping time and sampling time, we got 1 for the grab sampling method (pumping time = sampling time) and 1/84 ~ 0.01 for the JASS method meaning that the potential influence of the pumping time on the sampling time is much lower for the JASS method. Moreover, we chose the KNF N922 pump especially for its high chemical resistance because of many mobile parts in PTFE or PTFE-coated (eg. pump head and diaphragm).

3. Between the Δ14CH4 with >420‰ cutoff, Radon filter, and Radon filter + footprint based nuclear correction (Table 1) – I personally think that the Radon filter is the most straightforward/foolproof representative of a well-mixed background mid-latitude Δ14CH4, given the sporadic nature of 14CH4 emissions from PWR that is not captured by emission inventories. The authors take a more agnostic approach to this, which is fine, but I think at least the Radon filtered data should also be on Figure 4. I will also welcome an additional discussion if the authors have any inclination one way or another regarding which of the 3 annual averages they think is most representative of a true mid-latitude background value for 14CH4.

We are grateful for these comments. We added the mean annual Rn-filtered Δ14CH4 values to Figure 4. To complete Table 1, we extended the first column with annual average of raw Δ14CH4 values. Furthermore, as a remark, in the first version of the manuscript, $\Delta 14CH4^{Rn\text{-}filt}$ values were Δ14CH4 values lower than 420 ‰ with corresponding Rn <= 1.5 Bq $m^3$. In this new version, we

simplify the definition of $\Delta 14CH4^{Rn\text{-}filt}$ by only using the Rn-filtering (Rn <= 1.5 Bq m3); the values selected remained finally the same. In the conclusion part, we added a quick discussion about the different annual averages.

4. Since the footprints are already made, it would be interesting if the authors could show comparison of footprints/catchment area for the grab samples vs. integrated JASS sampling, maybe as a supplementary figure. Either pick two footprints that are somewhat representative of the average for the two sampling schemes, or sum the footprints and provide a contour of influence for the two sampling schemes.

We are grateful for this suggestion. We included a concentration footprint analysis for the different sampling strategies into a supplement (new Fig. S2). The analysis confirms the expectation that integrated nighttime sampling improves the representativeness of the sampled air mass as compared to biweekly grab sampling. We added a reference to the new Figure in section 2.1.

**Minor comments**

Line 30: "*biweekly*" is ambiguous as it can literally mean twice a week or once every two weeks. Be more specific and change it to either "*once every two weeks*" or "*fortnightly*".

In the whole manuscript (figures included), we replaced "biweekly" by "*fortnightly*".

Line 34: Add "*continued*" before "*dilution*." This is just to draw the contrast that the continued decrease in 14CO2 is pretty much expected and has been a trend that's been observed in all global sites, unlike the increase in 14CH4, which is a novel discovery of this study.

We added "continued" before "dilution".

Line 37: *"... the mean nuclear 14C contribution to our individual measurements was estimated to be 7 ± 9 ‰ ..."* This is slightly misleading, as from my understanding not all measurements are corrected for local nuclear contribution, only measurements with high Radon. I would instead say something along the line of "*To assess the nuclear 14C contribution to our individual measurements, we use a combination of in situ Radon-222 measurements and Lagrangian particle dispersion model convolved with bottom-up inventory of 14C emissions from nuclear power plants*." Probably also put this sentence before line 31 "*Over this period, Δ14CH4 values showed an increase...*"

We are thankful for this comment. We agree that not all measurements are corrected for local nuclear contribution, but only measurements with low radon values (and not high radon values as mentioned by the reviewer). In the abstract, we removed the figures "7± 9 ‰" and added the sentence suggested by the reviewer. Furthermore, in the conclusion, we refined the sentence presenting these figures by adding "$^{222}$Rn-filtered" before $\Delta^{14}CH_4$.

Line 47: The Hmiel et al. (2025) citation here is not appropriate. Please cite the original Law Dome ice core CH4 mole fraction paper (MacFarling Meure et al., 2006).

We are thankful for this comment; we updated the citation.

Line 52: A more appropriate citation for the 14C half-life of 5700 years is probably a combination of (Kutschera, 2013) – where the compilation of previous half-life measurements were made, (National Nuclear Data Center, Brookhaven National Laboratory, www.nndc.bnl.gov), and the most "recent" effort by Roberts and Southon (2007).

We are grateful for this comment; we updated the citations.

Line 61-62: "*Atmospheric 14CO2 have a long history. Since the first measurements focused on the atmospheric 14CO2 bomb peak in the 1950-60s…*" The most appropriate citation for this statement, even before Levin et al. (1985) is obviously the original Rafter and Fergusson (1957) paper.

We are grateful for this comment; we updated the list of citations.

Line 69-72: "*Since the bomb peak in the middle of the last century, Δ14CO2 values have been declining mostly due to the emissions of 14C-free fossil fuel CO2, which depletes the atmospheric Δ14CO2 signal (Levin et al., 2010).*" I understand what the authors meant to say, but this is not 100% correct, atmospheric 14CO2 is coming down from the bomb peak also (or even mostly) because of uptake by plants and the surface ocean. Maybe cite something like the Oeschger et al. (1975) paper or something more recent regarding the partitioning of land/ocean sink and revise the sentence accordingly.

We agree with this comment; we updated the sentence and added the citations of Oeschger et al. (1975) and Graven et al. (2024).

Line 85: Consider giving acknowledgements to the very first pioneering atmospheric 14CH4 measurements by Lowe et al. (1988) and Wahlen et al. (1989).

We agree with this comment and added a sentence in that sense with both citations.

Line 94-95: "*Fujita et al. (2025) estimated 30% lower global CH4 emissions from the fossil-fuel industry compared to previous isotope-based studies*." Clarify that the "previous isotope-based studies" specifically referred in this sentence are d13C-CH4 stable isotope only inversion like Basu et al. (2022) which does not include 14C consideration.

We are grateful for this comment; we updated the sentence accordingly.

Line 128: I don't quite understand the use of UTC throughout the manuscript - I think this can be a bit confusing because JFJ local time is not UTC, and when trying to sample the free troposphere local time matters more and absolute time. I would recommend just using local time and mention (UTC+1/UTC+2).

We are thankful for this comment. We understand that the use of UTC can be confusing but we still would like to let it like this. We chose to use the Raspberry Pi module controlling the JASS in UTC to avoid any datetime issues because of local time change (UTC+1/UTC+2). In addition, the whole modelling work was done on UTC basis. In total, UTC is used only 3 times in the manuscript and we believe that the readers will be fine with that. We added the local time information "(i.e. 08:00 and 12:00 local time, UTC+1/UTC+2)" at the end of the second paragraph in section 2.1.

Line 213: *"… and in equation 29 in Stenstrom et al. (2011)."* I'm a big fan of Stenstrom et al. (2011), but unfortunately it is not appropriate to cite as this was an internal report that was not peer-reviewed.

We understand this comment and removed this citation.

Line 227-229: *"In this study, Rn was used as a proxy to distinguish atmospheric conditions mostly influenced by free tropospheric conditions from conditions mostly influenced by the planetary boundary layer (i.e., with recent land contact)."* This sentence is a bit awkward, I would just say something along the lines of *"In this study, Rn was used as a proxy to distinguish free tropospheric samples from those originating from the planetary boundary layer (i.e., with recent land contact)."*

We are thankful for this comment and replaced the original sentence by the suggested one.

Line 288: I think "*convolve*" might be the more correct mathematical term instead of "*convolute*". Footprints are convolved with flux to produce mole fraction enhancement at receptor site.

We are thankful for this comment; we replaced "convoluted" by "convolved".

Line 452-453: *"The offset in 2019 was probably due to a small fossil contamination in our early Δ14CO2 measurements that was remedied during the year."* Not that I don't believe this explanation, but as it stands by reading this manuscript alone it is not very clear that the "true" European background Δ14CO2 in 2019 is closer to the ICOS/Heidelberg values than the LARA values. Maybe show/at least mention Mace Head (MHD) measurements as independent dataset to support the idea that LARA sampling stream was slightly contaminated in 2019?

We are grateful for this comment. We added annual mean $\Delta^{14}CO_2$ values from the Mace Head (MHD) Atmospheric Research Station for 2019-2022 in Table 2. The good consistency of ICOS JFJ and MHD data in 2019 and 2020 strengthens the idea that our early Δ14CO2 measurements in 2019 were slightly biased by fossil contamination that was corrected afterwards.

Line 526: Very minor thing, I might have missed it but I think "ΔΔ14CO2" is not defined. Just say "*difference between ICOS sodium hydroxide-based integrated sampling vs. our JASS system*" to reduce jargon.

We are grateful for this comment; we added (ΔΔ14CO2) in the caption of figure 6 and replaced ΔΔ14CO2 by the proposed sentence.

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