Review of the paper entitled "A multimillennial Alpine ice core chronology synchronized with an accurately dated Arctic Pb record" by Paolo Gabrielli and co-workers.

Overall comment: Whereas several points raised during previous reviews had permitted to improve and clarify the manuscript, there are still points that need to be considered as detailed below.

Introduction, Page 3, Line 8: Please specify that the ice-core record extracted from the Weißseespitze (3500 m, Austria) by Bohleber et al. (2020) covers some 5,900 years, and that in Western Alps the Dôme du Goûter record covers at least the last 12,000 years (Legrand et al., 2025). Reporting these previous findings here will permit to the readers to better understand the meaning of the first sentence in the introduction that "*your record is of one of the oldest Alpine ice core records"*. Reversely, it is not necessarily needed to refer again to Legrand et al. (2025) at line 13 (also page 3) for ^{14}C.

The crustal Pb contribution: Page 15, line 20: Please specify that the site-specific Pb to Rb mass ratio of 0.51 that you derived is reasonably consistent with the mean sediment composition (Pb/Rb of 0.14 from Bowen, 1966).
H. Bowen, *Trace Elements in Biochemistry* (Academic Press, New York, 1966).

The leaching question:

In the atmosphere, Pb is present in dust particles and also comes from various natural and anthropogenic sources often involving processes at high temperatures during which Pb is first volatilized and then condensed on pre-existing particles. This is true for main natural non-crustal sources of Pb (volcanoes and biomass burning) and numerous anthropogenic processes (liquid and solid fuel combustion, various metal smelters, pig iron and steel production, etc) (see Nriagu et al.,1979; Nriagu and Pacyna, 1988). As for many species crustal species, the crustal part of Pb is likely more refractory than components associated with combustion processes that are adsorbed onto smaller particles and so readily washed off during on-line acidification. It is therefore important when discussing the leaching question to know how abundant is the crustal fraction of Pb and its eventual change over the past. Since the difference between the two Pb Orles record is variable with time (see Figure 2 and my next comments on that below) it is important to show the contribution of crustal Pb to total Pb and its change along the records reported in Figure 2.

Nriagu, J. O. (1979). A global assessment of natural sources of atmospheric trace metals. *Nature*, *338*, 47–49.
Nriagu, J. O., & Pacyna, J. M. (1988). Quantitative assessment of worldwide contamination of air, water and soils by trace metals. *Nature*, *333*(6169), 134–139.

In the Supplementary text 2 (Methodologies adopted to determine Pb), you specify that "*In a more recent manuscript, these shorter timescales, from minutes to a few hours, were investigated (1.5-250 minutes) for alpine ice core samples (Münster et al., 2025). Those results show a 2-to-3-fold increases in the measured Pb concentration over that short time range and can explain the observed difference in absolute Pb concentration between Alto dell'Ortles cores #1 and #3 due to differences in the method dependent acid times applied.*" First, please specify that the recoveries derived by Münster et al., 2025 were done on CG ice cores that contain relatively large dust content (see the Table 2 in Münster et al., 2025) and thus these results you cannot be generalized to the all Alpine ice cores. Therefore, as already mentioned, it would be important to see how abundant is the crustal Pb contribution in the Ortles ice.

In your answers to the comments raised by the two reviewers on the leaching you said: "*While we think the acid leaching topic will be of interest in a future manuscript focused on past Pb sources and emissions, the acid-leaching process is of minor relevance in the context of this manuscript where only the shape of the record can affect the conclusions (revised chronology).*" I disagree since, as far as you show a Pb record in your manuscript, the reviewer is supposed to examine the validity of the reported data. Also, I am less optimistic than you here since your comparison (Figure 2) is reported as a log scale. In fact, the difference between the two Pb profiles sometimes (and for unknown reasons) are as large as the temporal variability.

You also said "*In any case, it is possible that differences might be larger at low Pb concentrations where the Pb leaching from mineral dust particles of relatively large size can become relatively more important*". Whereas I basically agree with that, the Figure 2 does not totally supports that: At 59-60 m depth in core 1 there are ~0.5 ng g^{-1} instead of ~0.12 ng g^{-1} in core 3, and at 69.5-70 m in core 1 there are ~ 0.09 ng g^{-1} instead of ~ 0.02 ng g^{-1} in core 3. So again, to clarify this point it would be more straightforward to report in Figure 2 (using a linear scale) a dust related species (Rb for instance that you used to calculate ncPb) instead of making speculation on what extent the variable difference between the two profiles is due to a variable amount of dust.

I saw in your answer that you envisage that "*Rb concentrations and Pb leaching are not necessarily linked*": This statement needs to be demonstrated (or documented with references). I don't really understand this argument since you used Rb concentrations to derive the ncPb fraction and we would expect that when dust is abundant the record indicates higher Rb concentrations. Please comment.

<u>The recent trend:</u>

To explain the large departure between Ortles and AN over the recent period you now specify that "*It is also important to note, that the Mt. Ortles glacier archive at the study site is temperate in the upper firn part only (down to 30 m depth; see Gabrielli et al 2012) and post depositional effects, causing a partial loss of the most recent Pb signal by melt-water runoff are possible (see e.g., Huber et al., 2024; and Avak et al., 2018, estimating up to 50% loss for the Pb signal)"*.

I am not totally convinced that the melting in the upper Ortles layers can explain the huge difference seen in your figure 8 after 1700 CE. First, Avak et al. indicated a rather weak effect on Pb (a loss from 5 to 50%) compare to others species such as Zn (70-80%) or Cd (85-95%) in the warm ice from the Grenzgletscher site. They concluded "Based on their immobility with meltwater percolation, we propose that Ag, Al, Bi, Cu, Cs, Fe, Li, Mo, Pb, Rb, Sb, Th, Tl, U, V, W, Zr and the REEs (Ce, Eu, La, Nd, Pr, Sc, Sm, Yb) may still be applicable as robust environmental proxies in ice cores from Alpine glaciers partially affected by melting. In contrast, concentration records of Ba, Ca, Cd Co, Mg, Mn, Na, Ni, Sr and Zn are prone to significant depletion". Based on your Figure 8, I calculated that we may expect an initial (prior to post depositional effect due to melting) ncPb concentration at Ortles at the surface of ~30 ppb to respect the rest of the record and its scaling to the AN record. Given the measured surface Ortles concentration of 1 ng g^{-1}; that means a depletion reaching a factor of 30. That a lot and far larger than what was claimed by Aval et al. (a factor of 2 as an upper limit).

Second, the difference is still a factor 20 in 1800 CE. How conciliate that with what was claimed in the previous Ortles paper (Gabrielli et al., 2016): “We concluded that this glacier probably represents a unique remnant of the colder climate prior to ∼ 1980, which has since been shifting from a cold to a temperate state.” Please comment.

Finally, since it is well-known that in temperate ice species like NH_4 and Ca are quasi-totally lost (Eichler et al., 2001) and because they were certainly measured in the Ortles ice core, it would be nice to document the absence of recent increase of Pb with a record of Ca or NH_4 over the two or three last centuries to assess the strong effect of melting after 1700 CE in the Ortles record.

Eichler A, Schwikowski M and Gäggeler HW (2001) Meltwater- induced relocation of chemical species in Alpine firn. Tellus B Chem. Phys. Meteorol., 53(2), 192–203

Conclusion: “*In conclusion, this revised more accurate time scale (CP2025/2) will allow to provide additional new detailed climatic environmental histories of Central Europe during the Holocene*”: That is overstated since the Holocene started at 10,500 yr BP not at 7,000 yr BP. So please modify the sentence since the Orles record did not extend to the Early Holocene.

I don’t understand the last sentence: “*In addition, CP2025/2 has the potential to become a reference chronology for multiple paleoclimate archives in Europe.*” Please be more specific (what do you mean with “reference chronology” ?).

Your answer to previous reviewers: There some points where you did not yet answer adequately to the reviewers (points for which I also agree):

Supplementary Text S2: Previous remark from one of the previous reviewers said that the first sentence is very misleading: “The Colle Gnifetti ice core (Mt. Rosa, Western Alps) is currently the oldest record from the Alps, dating back >15000 years (Jenk et al., 2009)”. This sentence gives the impression that the CG ice recorded environments (and/or climate) back to more than 15,000 years. This is however not correct: As argued by Jenk et al. (2009), while radiocarbon analyses of particulate organic carbon have indicated that Pleistocene ice is sometimes present in the bottom layers at CG, it is shown that prior to 3,000 years the climate d18O record was strongly disturbed by post- deposition liquid migration of 18O at the grain boundary of ice located in zones of strong strain-rate gradients above the inclined bedrock.”

Your answer is that you replace the word “*record*” with “*archive*”. That is far too vague and still incorrect: Whereas the ice deposited at the bottom of the CG glacier is as old as 15,000 years, Jenk et al. (2009) demonstrated that the ice did not correctly archived past climate and environment prior to 3,000 years BP, so again please modify your wording.

Units: Whereas you give now the formula to convert m into mwe, I also ask you to report on Figure 2 the depth scales using both m and mwe (since for most of the Figures and Tables the unit is mwe), so that the reader can more easily find the ages in your figure 5.

Figure S6: (comparison CG03-Ortles): At around 1550 CE the difference between the two profiles reached a factor of 10. In your answer you commented as “*While this discussion is* *out of the scope of this paper, we think that a factor 10 difference in absolute Pb concentrations is reasonable, considering different snow accumulation rates, distance, and transport path from the emission sources to the respective sites. Also, please see above the response related to CG."* I am not convinced: A difference by a factor of 10 between CG03 and Ortles for a species that is mainly present in the atmosphere as micronic or submicronic aerosol is a lot (at the scale of the Alps). Also, how you explain that the difference is sometimes as high as a factor 10 and sometimes far lower. Please comment.

I am not sure about the publication rules of the Climate of the Past journal but it seems to me that, as far you show Pb profiles, the Pb data (not only the tie points) has to be deposited in a data base (and not waiting your planned future publications). I let the editor to decide on this point.

End of the review