

General comments

Ardoin et al. present a comprehensive investigation of gas composition (N₂, O₂, Ar, CO₂, and CH₄), water isotopes, and DNA data from the basal ice and underlying sediments of the Camp Century and GRIP ice cores. The authors suggest that CH₄ produced in subglacial sediments can diffuse into the overlying basal ice, where it may undergo partial oxidation to CO₂. They further suggest that the elevated CH₄ and CO₂ concentrations observed in the upper sections of basal ice are likely due to mechanical mixing. Finally, authors suggest that CH₄ consumption by methanotrophs is likely in the basal ice in Camp Century, whereas evidence for methanotrophic activity in GRIP appears limited.

The dataset is valuable particularly within the context of the special issue, as it revisits a unique legacy core that reached the ice–bed interface and applies modern analytical approaches. The study also has potential to contribute to ongoing discussions on subglacial carbon cycling and the interpretation of extreme gas signatures in debris-rich basal ice.

Overall, I find the data set interesting and potentially valuable. However, in its current form, the manuscript is very difficult to read. I do not recommend publishing this manuscript in a journal as prestigious as ‘The Cryosphere’, unless the authors can address significant issues described below and thoroughly revise the manuscript.

Specifically, the authors need to clearly state the central research questions their study aims to answer. The current presentation lacks a cohesive narrative, making it challenging for the reader to follow the progression of ideas and the connection between different sections. Furthermore, the broader implications of their findings are not sufficiently explained, leaving the reader to question the true contribution of this work to the field of cryospheric sciences. Without a more focused and clearly articulated argument, supported by a well-structured and engaging presentation, the manuscript fails to meet the high standards expected for publication in ‘The Cryosphere’.

* I note that as the DNA analysis falls outside my primary area of expertise, I do not provide detailed comments on the DNA-related methods or interpretations. My review focuses on the gas composition data, physical and chemical processes at the ice–bed interface, and its implications for subglacial greenhouse gas dynamics.

Specific comments

Abstract

The abstract reads primarily as a technical summary of the methods and analyses performed, rather than clearly conveying the main contribution of the work to existing previous knowledge. The authors should more explicitly explain the background information of the research topic, the general problem of what this study is addressing, and implications of the new finding on, for example, subglacial carbon cycling and/or greenhouse gas dynamics.

Line 34: “There is no evidence of CH₄ consumption by methanotrophs at GRIP, suggesting that variations of bed conditions, ice dynamics and the nature of the organic material control the fate of CH₄ produced in the subglacial environments.” The logical flow would be clearer if the abstract first explain that methanotrophic activity is likely in Camp Century, contrary to GRIP.

Introduction

The introduction currently lacks necessary background information that would help readers follow the manuscript. For example, it would be nice to briefly explain that ancient atmospheric air can be trapped and preserved as air bubbles in ice. This is important before discussing anomalous greenhouse gas concentrations trapped in basal ice. Also, previous studies conducted on basal debris-rich ice are not sufficiently described. Providing a summary of what has been found in previous studies and what remains unresolved would help convey the significance of the present study. Finally, the central scientific question of this study is not clearly stated. That is, it is unclear what the main research objective is. It will be nice to explain why greenhouse gas dynamics across the sediment-ice interface matter and what knowledge gap this study aims to address.

Line 39–40: This sentence stating that ice sheets were not considered contributors to the global carbon cycle seems inconsistent with the following sentence, which emphasizes recent findings of active carbon cycling and greenhouse gas emissions from the Greenland Ice Sheet.

Line 43: It will be nice to provide quantitative information on CH₄ emissions from subglacial runoff and indicate how significant these emissions are relative to global CH₄ budget.

Line 62: The phrase “influence CO₂ and O₂ concentrations” is unclear. Specify where these CO₂ and O₂

concentrations are being modified. Atmospheric air? Basal ice? Subglacial sediment?

Line 79–84: This part would fit better in the “Materials and methods” section. In addition, the term “Unit 5” is not clearly defined. Please clarify what “Unit 5” represents and ensure it is properly introduced before being used.

Materials and methods

Line 94: Please explain how “debris-free”, “debris-rich basal ice”, “basal ice”, and “bed materials” were defined. It is currently unclear whether these terms are based on visual observations, sediment content thresholds, stratigraphic boundaries, or previously established classifications.

Line 99: Please define “GrIS” at its first occurrence before using the abbreviation.

Line 101: At the surface, there is snow not ice. Please remove “ice” from “ice surface”.

Line 105: The term “cold bed conditions” needs clarification. Does this refer to the absence of partial melting?

Line 112: “describe” to “described”.

Line 114: It is unclear how the 1 SD reproducibility was determined. Was it calculated from a combination of SD of internal standard measurements and triplicate analyses of each sample?

Line 124: Please clearly define what “Unit 2” refers to before using the term. It may be helpful to introduce both Unit 2 and 5 together earlier in the manuscript.

Line 166–171: It is unclear how the concentrations of O₂, Ar, CH₄, N₂, and CO₂ were measured in the bubble free ice (BFI)? Does this imply that residual gas remained in the BFI? Is BFI is not actually bubble-free?

Line 168 and 209: “milliQ” to “Milli-Q”.

Results

Line 253: The order of figure explanation is not consistent. Figure 3 is discussed before figure 2, which interrupts the logical flow. Please consider reordering the figures so that they are introduced consecutively.

Line 253–255: In the “results” section, the focus should be on presenting the findings of this study rather than

summarizing previous work. In addition, it is unclear why ice crystal diameters and c-axes orientations should be described here.

Line 258: Why is Fig. 2c referenced here?

Line 268: Are clotted debris visible in Fig. 1e?

Line 272: It is unclear of “top 1.5 m”. Does this refer to 1.5 m above the ice-bed interface?

Line 275–276: The phrase “exceeding those of both Holocene and Eemian ice” requires clarification. Are you referring to Holocene and Eemian ice from GRIP? What is the range of $\delta^{18}\text{O}$ values for Eemian ice from GRIP?

Line 276–277: Please provide a reference supporting the statement that elevated $\delta^{18}\text{O}$ values are consistent with ice formed at the ground surface in the absence of an ice sheet.

Line 280: “At Camp Century, $\delta^{18}\text{O}$ depth variations oscillated between glacial and higher than Holocene values”
Do you mean glacial and Holocene values from Camp Century ice?

Line 285–286: Are the water isotope values derived exclusively from basal ice, or do they include all debris-free ice and debris-rich ice?

Line 291: The regression lines for GRIP and Camp Century are difficult to distinguish in the figure. Please consider using different colors.

Line 293: This section is difficult to follow in its current form. It may improve readability to divide it into subsections (e.g., 3.2.1 Total gas content, 3.2.2 Greenhouse gases, 3.2.3 Major air (N_2 , O_2 , and Ar)).

Line 295: Figure B5 is referenced before Appendix A and figures B1 to B4 are introduced. Please revise the order of references to maintain a logical and sequential flow.

Line 316–317: This sentence is difficult to understand. What is being contrasted here? Clarify the comparison and make the intended contrast explicit.

Line 324: Please check “ $40\,794 \pm 2\,217$ ppm”. Is a comma missing?

Line 334: “ 2.2 ± 0.1 ” missing “%”?

Line 350: The x-axis labels for CH_4 in Figure 3d are difficult to read.

Line 362: “Fig. A1” Again, please ensure that figures are discussed consecutively and introduced in a logical order.

Discussion

Line 406: Respiration is mentioned earlier at line 388 as a potential in situ process, but it is not discussed in this paragraph. If in situ respiration is considered important as you mentioned at line 388, please explain on how it may influence the observed gas signatures. If not, earlier statement should be clarified or revised to avoid confusion.

Line 416: Remove the unnecessary space in “ice- bed”.

Line 426: This is confusing. In line 410, in situ CH₄ production is suggested as possible. However, in line 426, it is stated that the data do not support methanogenesis.

Line 427: The term “incomplete” needs clearer explanation. What exactly is incomplete? Please explain what is meant and why it is considered incomplete?

Line 428: “Figs. 1d, j” appears to be incorrect. There is no panel “j” in Fig 1.

Line 440: Please provide the r^2 and p values for the regression line to support the stated relationship.

Line 445–446: I am not fully convinced by the statement that there is compelling evidence for mechanical mixing. Please clarify what specific observations or quantitative evidence support this interpretation.

Line 472: Please provide the typical range of gas content in meteoric ice for comparison. Including reference values would help readers assess whether the observed concentrations are anomalous. In addition, ice does not form directly at the surface of the ice sheet. It forms at depth, typically 50–100 m below the surface, after snow compaction and firn densification.

Line 486–494: This paragraph is difficult to follow due to the dense description of ice fabric observations and radar interpretations. Consider restructuring the paragraph and more clearly explaining how these observations support differences in deformation history between Camp Century and GRIP. In addition, it is not clear how this paragraph relates to the discussion of gas loss; please clarify the intended connection.

Line 523: CH₄ accumulation is attributed to biological activity, whereas at line 426–428, the authors state that methanogenesis is incomplete and not supported by the data. These statements appear inconsistent.

Line 526–529: This paragraph does not seem closely related to Section 4.2.2 (“Gas expulsion at the ice–bed interface”) and, as written, it is likely to confuse readers. I suggest either removing it from this section or revising

it to make the connection to gas expulsion explicit (e.g., by clearly stating how this information supports the interpretation of gas loss/expulsion at the ice–bed interface).

Line 530: There is no red curve in Fig. 5d.

Line 563–569: Some of the sentences in this part could be moved to line 546 to better support and clarify the proposed hypothesis of selective removal of N₂ gas. Presenting the explanation immediately after introducing the hypothesis would improve the logical flow.

Line 592: Provide the molecular size of CH₄ and Ar and related references.

Line 652–654: The link between the transformation of labile organic matter and reduced methanotrophic activity at GRIP is not fully explained. Please clarify how the availability of organic carbon directly constrains methanotrophy in this context. Also, the statement that basal ice at GRIP likely contains less bioavailable carbon appears speculative.

Line 656: In lines 650–654, the authors suggest that reduced availability and quality of organic carbon may limit methanotrophic activity at GRIP. However, in Line 656, the thermal state at the base of the ice sheet is described as the primary control on the preservation of a methanotrophy signature. It is not clear how these two factors are related.

Line 657: The term “warm-bed setting” requires clarification.

Conclusion

Line 683: Was in situ respiration discussed in detail earlier in the manuscript? If respiration is considered an important process influencing gas composition, it should be more clearly discussed before being mentioned in the conclusion. Furthermore, in lines 28–29 (Abstract), CO₂ accumulation is attributed to the oxidation of CH₄, without consideration of respiration as a potential contributing process.