Response to RC1 for Manuscript egusphere-2023-3146

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We thank the reviewer for the careful and critical review of the manuscript, as well as for the thoughtful comments aimed at improving it. We also extend our gratitude to all individuals involved (reviewers and editors) for dedicating their precious time to this endeavor. Below, we outline our responses to each of the reviewer's comments, denoted in blue text, while the original comments are presented in black. For ease of reference, we have assigned ID numbers to each of the reviewer's comments and to each of our replies. To begin, we offer several key statements as an introduction to our responses.

Key statements

[A0_1] Regarding the reviewer's suggestion on reorganization and convergence into a coherent story

In both the global statements and detailed points, the reviewer has frequently commented that the discussions need to be re-organized to have a clearer focus on selected results and observations that contribute something new to the "story." The reviewer suggests that many results and ideas should converge into a "story" with more precise comparisons with existing work.

We appreciate the reviewer's feedback and agree to attempt a reorganization to better emphasize some of the results. However, we also express a concern that focusing overly on a single narrative, or splitting the paper into multiple narratives, might not be the best approach for the current work. This is due to its numerous results and insights, as listed in the conclusion section of the preprint. We have concerns that this approach might lead to the omission of significant findings, given the novelty and breadth of our research, which relies on innovative methods or discoveries in our present work.

A possible middle ground could be to structure the discussion around a few central themes or questions that the research addresses, organizing related findings and insights under these themes. This approach can help achieve both clarity and comprehensiveness, allowing us to present a coherent narrative without omitting significant results. It also provides a framework that can help readers understand how different parts of the research contribute to the overall story.

For this possible middle ground, in the revision, we will focus on at least the following topics:
(i) Crystal anisotropy and ice sheet dynamics
(ii) Advantages of ice cores from dome regions in the central plateau area of ice sheets
(iii) Exploring deep ice rheology and microstructure
(iv) Advancing ice sheet dynamics: The development and role of crystal orientation fabric
(v) Contrasting mechanisms of ice deformation across depths: revisiting shear impact and COF variations in ice cores such as EDC and DF.

Readers will discover that information itemized from (i) to (iv) will be very informative to better understand the important topic (v).

[A0_2] The position of this work in terms of ice sheet dynamics: Advantages of ice cores from dome regions in the central plateau area of the ice sheets

The ice in the dome summit regions of central plateau areas on ice sheets offers an opportunity for studying ice deformation processes. In these regions, conditions are relatively simple because they are presumed to be least affected by simple shear deformation, typically caused by surface slope and gravity. Due to this simplicity, dome regions in the central area of the ice sheets are ideal for investigating how processes related to deformational progress, such as dislocation creep and recrystallization, develop in a relatively simplified stress-strain environment and under moderate temperature gradients. Among the drilling sites in Antarctica, it is presumed that, so far, only the DF and EDC sites meet these conditions. In fact, in the case of inland domes, ice deforms mainly through compression along the vertical axis. This has been confirmed by the COF measured along cores drilled at these sites, which exhibits a single-pole distribution of the COF. Similarly, this was confirmed in the Greenland ice sheet with the GRIP core (Thorsteinsson et al., 1997). It is noted that there are also local domes at the edge of the ice sheet plateau, for example, Talos Dome (Urbini et al., 2008). Such a dome is characterized by a lower elevation (of about 2300 m), a location facing the coast, an annual accumulation rate much larger than those of the central plateau (by 3 to 4 times), and suggested migration at least during the last few centuries. Such a dome has higher instability and is suitable for investigating the impact of more shear, in contrast to the domes in the central plateau area. The COF evolution for the Talos Dome core was presented by Montagnat et al. (2012) as mentioned by the reviewer.

[A0_3] On the claims of the presence of simple shear in Termination II within the ice sheet, as suggested by Durand et al.

In the present study, including that by Saruya et al. (2022b), we did not identify anomalous steps of c-axes clustering with increasing depth (as illustrated in Figures 7 and 8 in the preprint and Figure A0_3 in this document below). Instead, the COF exhibits fluctuations that are already apparent at a few hundred meters depth at Termination I. Thus, we found no indication attributable to the presence of simple shear. This argument differs, at least in part, from the scientific claims made by Durand et al. (2007; 2009), who suggested that the impact of shear was significant in explaining the anomalous strengthening of the c-axes cluster, especially at Termination II (refer to Figure 8). In contrast to shear, Saruya et al. (2022b) attribute small depressions of $\Delta\varepsilon$ values at MIS5 to the very low concentration of resolved Cl^- ions (which can substitute for the location of H_2O) in the crystal lattice, indicating that ice is harder. This explanation might apply to the EDC case because, at EDC, most Cl^- ions are lost to the atmosphere from snow compared to DF during the Holocene (i.e., the interglacial period), while at Dome Fuji, they are preserved as NaCl and in solid solution (Oyabu et al., 2020). These conditions suggest that ice becomes harder, preventing the COF cluster from strengthening at MIS5 as well. In other words, this could explain the significant depression of COF at MIS5 and a notable jump in the COF cluster in adjacent older ice at EDC. Additionally, further complexity arises because, at EDC, the surface mass balance (SMB) contrast between glacial and interglacial periods is approximately 20% larger than that of DF (Fujita et al., 2015; Parrenin et al., 2016), which will dilute Cl^- ions more at EDC and also complicate ice flow models.

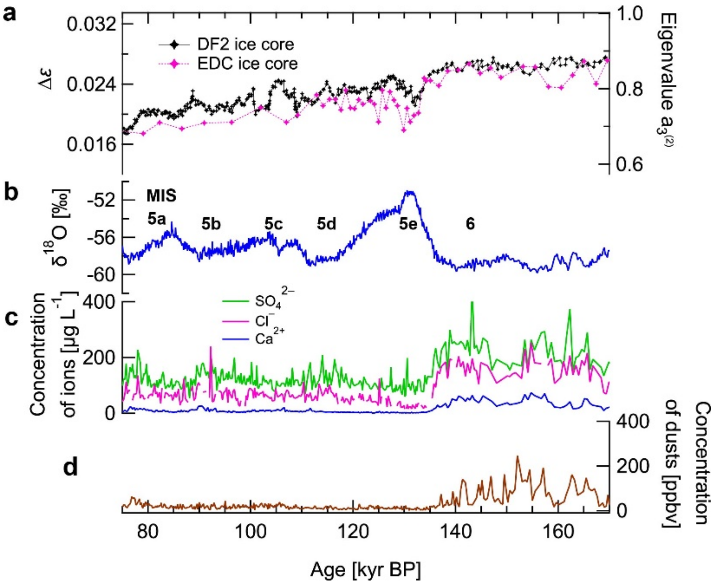

Figure A0_3: Modification of the Figure 8 in the preprint. COF data from the DF core and the EDC core are compared using a common age scale of the DF2021 age scale. (a) $\Delta\varepsilon$ presented as mean values for each 0.5-m segment (black markers for DF) or for each thin section (magenta marker for EDC). The right axis provides a scale for normalized eigenvalues. (b) Oxygen isotope ratios ($\delta^{18}O$) in the DF ice core. (c) Concentrations of Cl^-, SO_4^{2-}, and Ca^{2+} ions (Goto-Azuma et al., 2019). (d) Concentration of dust particles (DFICPM, 2017). For ice at EDC at MIS5, the normalized eigenvalues are smaller than those at DF.

[A0_4] On the roles of different methods in investigating COF

We utilized the thick-section-based Dielectric Tensor Method (DTM), as well as the thin-section-based methods of Laue X-ray diffraction and the automatic ice fabric analyzer. The roles and characteristics of each method were presented in Saruya et al. (2021, 2022). Using DTM, COF data are provided as eigenvalues with a high sampling frequency, high spatial resolution, and continuity, thereby offering statistical significance. However, this method does not yield data on the crystal axes of individual grains. Additionally, if a cluster of c-axes is significantly inclined from the vertical, deriving correct eigenvalues is challenging without knowledge of both the inclination angle of the c-axis cluster and its horizontal orientation. Moreover, the Laue X-ray diffraction method enables the clarification of detailed information about not only the c-axes but also the a-axes of each crystal grain. Furthermore, the automatic fabric analyzer, model G50, originally manufactured by Russel–Head Instruments, proves very useful for demonstrating how crystal orientation differs from one grain to another. The judicious use of these methods provides unprecedentedly rich information set on COF along ice cores.

In the revised manuscript, we will explain the role and complementarity of each method.

Reply point-by-point to each of reviewer's comment

General points

[R1] This paper provides analyses of physical properties (grain size and crystallographic orientations) measured along the deepest part of the Dome Fuji ice core, between 2400 and 3035 m depth. Measurements were done using the dielectric tensor method (DTM) on thick sections of ice, the Laue X-ray diffraction method and the automatic ice fabric analyser on thin sections. Based on these measurements, the authors provide an analysis of the deformation and recrystallization processes likely to take place in the deepest part of the ice core. They also observe and somehow quantify the impact of the shear component on the flow of ice below Dome Fuji.

Global comments:

[R2] This paper presents some valuable high resolution data of crystal orientation fabric (COF) in the deep part of the core where deformation heterogeneities are supposed to occur that could disrupt the dating signal and the radar echo sounding in the area.

[R3] They also provide some comparisons between three different means of measuring the COF that can be of interest.

[A1, A2, A3] Thank you for the comments.

[R4] Nevertheless, I did not clearly understand the necessity to have the three different types of measurement, especially for Laue X-ray diffraction and fabric analyser that are strongly redundant (except that X-ray diffraction provide the a-axes orientations that are known to be isotropic, so yes, it is interesting to double check but it is not used in the analyses).

[A4] Please refer to [A0_4].

[R5] The DTM method provides a higher resolution than classical diffraction or optical measurements nevertheless the method still requires making sections (thick ones), and, in order to be statistically representative, it must integrate a minimum number of grains, therefore it may be viewed as a moving average of the COF obtained from the other means from thin section?

[A5] We plan to address it in the revised manuscript. As for the statistical significance, depending on number of crystal grains in the beam, statistical significance is different. In some cases, a measurement at a point contains hundreds of grains, in another case, a beam contains smaller numbers (10 to 10^2). Then, moving averaging along the core is useful to gain more significance. A table below will be added to revised manuscript.

Table A5: Grain numbers included in a Gaussian beam or in a thin section

Measuring size and grain area	Dimension (mm)	0.1 cm^2	0.3 cm^2	0.5 cm^2	1 cm^2	2 cm^2	4 cm^2
Thick-section-based method							
DTM No.1	~16φ × ~40	353	68	32	11	4	1
DTM No.2	~38φ× ~70	3490	672	312	110	39	14
Thin-section-based methods							
Laue X-ray method	100 × 45	450	150	90	45	23	12
Optical method of microscopy and G50	90 × 50						

[R6] Overall, the part that analyses and discusses the results should be re-organised and strengthen by some more precise references to previous works.

[A6] Re-organization and more referencing will be considered one by one addressing each of specific comments.

[R7] This part appears as a “brainstorming”, with many results and ideas mentioned, but they need to be converged into a “story” that shows what comes out of the results, what it brings to the existing story about ice core analyses (with more precise comparisons with existing work), and/or what new story it tells (although I didn’t see so many new results or observations in this work).

[A7] Thank you for the comment. Please refer to [A0_1] above.

[R8] In particular, dynamic recrystallization along ice cores has been studied for a long time, both in terms of basis mechanisms that come into play and in terms of impact on the COF evolution. I think that COF evolution along Dome F ice core should be analysed regarding this existing frame. I provide some references in the detailed comments below but many other exist that the authors can refer to (since most of the references I provide are from my team… as it is more straightforward for me).

[A8] We agree that dynamic recrystallization has been investigated widely, as reviewed in papers or textbooks (e.g. Faria et al., 2014a, b; Humphreys & Hatherly, 2004; Poirier, 1985) or individual papers (e.g., De La Chapelle et al., 1998; Kipfstuhl et al., 2009; Montagnat et al., 2012; 2014; 2015 Stoll et al., 2021a, b; Weikusat et al., 2009). Exploring deep ice rheology and crystal properties, dynamic recrystallization is one of main focuses in this paper.
In the revised manuscript, we will add references widely including review paper, individual paper, and textbook.

[R9] Some interpretations are not rigorous enough and should be strengthen by some physical concepts or adapted references. This is particularly the case when are mentioned grain boundary sliding and “microshear”. Grain boundary sliding is a specific mechanism that occurs under specific circumstances that are clearly not encountered here (small grains, high level of strain, accommodating mechanisms at grain boundary, etc.). The only observation of a few grain boundaries with specific shapes is not enough to support grain boundary sliding (and in 2D!). See for instance the works done in the metallurgy community (e.g. Doquet & Barkia, 2016, Mechanics of Material, Linne et al., 2020, Int. J. Plasticity). I think this is not positive for the ice community to keep on mentioning GBS as an important mechanism although there exist so few evidence of it along ice cores. Especially since natural ice is characterized by very large grains, with very efficient accommodation processes that are grain boundary migration and dynamic recrystallization, there is no requirement of GBS to explain ice deformation along ice cores (even if it might take place in the specific conditions of the Goldsby & Kohlstedt, 1997 experiments on very fine-grained ice).

[A9] As indicated by the reviewer, there is no significant evidence to suggest that microscopic GBS via microshear is occurring. We understand that the occurrence of GBS under the conditions present in ice sheet ice (e.g., large grain size) is far removed from common knowledge in metallurgy. Even if the GBS were to occur, it would likely be limited to special areas where the conditions are met. In the revised manuscript, we will emphasize that the GBS via microshear is one of the potential mechanisms that can explain the observed microstructure (brick-wall pattern) in the impure layers. Since the microscopic GBS is not the focus of the present paper, we will provide a summary of this mechanism. It is worth mentioning that the brick-wall patterns were observed exclusively in impure layers, and microscopic GBS via microshear is a possible explanatory mechanism. Additionally, we will include quantitative discussions on grain shape (e.g., aspect ratio of grains). In the revised manuscript, we plan to revise Section 5.3.1 as follows:

5.3.1 Microstructures in the impure layer: brick-wall pattern and steady-state grain size

In this section, we discuss the Δε value variations, focusing on microstructures in the impure layer and dynamic recrystallization process. From microstructural observations, we identified interesting features in the impure layers (Figure 6): the crystal grains elongate, and their major axes incline away from horizontal directions. These features were not observed in surrounding less-impure (interglacial) layers. The aspect ratio of the short and long axis of a fitted ellipse in the impure layers is 1.9–2.0 (three thin sections), while that in less-impure layers is 1.5–1.7 (thirteen thin sections). Detailed microstructures are shown in Figure 12a. The same feature, known as the "brick-wall pattern" has been observed at the high-impurity ice layers of the EDML ice core in Antarctica (see Figure 1a for location) (Faria, 2009; Weikusat et al., 2017) and the NEEM ice core in Greenland (Kuiper et al., 2020), but it has not been reported in the EDC ice core. Weikusatl et al. (2017) mentioned that shear deformation is responsible for the brick-wall pattern in the EDML ice core. However, observations of the brick-wall pattern in ice cores are few and the mechanism is not clear. Faria et al. (2009) proposed microscopic grain boundary sliding (GBS) via microshear as a deformation mechanism for making the brick-wall pattern. Their idea was based on analogue experiments by Bons and Jessel (1999) using octachloropropane. Llorens et al. (2016) also discussed it based on the numerical simulation. According to Faria et al., (2009), a combination of high impurity content, temperature, and moderate stress causes microscopic GBS. These conditions could be achieved in the bottom part of the ice sheets (Faria et al. 2009). The formation of microshear bands creates new, flat GBs parallel to the shear plane, resulting in the brick-wall pattern. It is no clear that GBS occurs in ice-sheet ice, but this mechanism can explain the observations of microstructures. Regarding the influence of GBS on the COF development, Faria et al. (2009) suggested that GBS has no severe impact on COF. In our study, we observed significant variations in Δε values within the impure layers. Furthermore, the Δε value has an inverse correlation with the dust concentration within the impure layer. Assuming COF remains unchanged by GBS, the variations in COF observed in the impure layers at greater depths would have originated at shallower depths before the onset of GBS. We observe an inverse correlation between the Δε value and dust concentration even at depths shallower than 2400 m.

The brick-wall pattern appears to be less pronounced in deeper sections as shown in Figure 6e (sample from a depth 2909 m). Despite the high concentration of dust particles (although not as high as 2648 and 2759 m), there are no evident brick-wall patterns. Due to high temperature close to melting point, the pattern could be lost by dynamic recovery and *recrystallization. The brick-wall pattern will change with temperature and impurity concentration. Further investigations are required to clarify the mechanism of brick-wall pattern formation and development, and relation with temperature and impurity concentration. Although microscopic GBS can explain the microstructures in impure layers, it is not possible to determine from our observations whether microscopic GBS via microshear is really occurring or not. Further detailed observations and experimental verification are needed in future.*

Another feature in the impure layer is approximately constant (or small fluctuation) grain size (Figure 9d). In the DF ice core, a constant grain size is observed between depths of 2640 and 2675 m, where the concentration of dust particles is extremely high. This constant grain size in ice core is known as "steady state grain size" (e.g., Steinbach et al., 2017). It is believed that steady state grain size is achieved when normal grain growth counteracts rotation recrystallization regardless of the initial grain size (Jacka and Li, 1994). We believe that the steady grain sizes in the impure layers established themselves after these layers had reached deeper depths. When the impure layers were at shallower depths in the past, grain size would have been inversely correlated with dust concentration.

[R10] Many repetitions exist all along section 5. Maybe the authors should be more focused on the main message they want to provide, in order to avoid dispersion in the interpretations and some lack of rigor as just mentioned, but also to enable more focused and deeper comparisons with existing works.

[A10] Thank you for the comments. Please refer to [A0_1] above. This paper, as a result of applying innovative methods, has reached a wide range of conclusions, and in the process of its discussion, it inevitably has to mention the same thing occasionally, even within different sections, in an introductory manner. However, of course, the authors will attempt to improve it.

[R11] In general, to make reading easier, please avoid acronyms in section titles, abstract and conclusion.

[A11] Our views are as follows: For all acronyms, we have provided clear explanations when each abbreviation first appears in the manuscript. This approach is intended to avoid the repetition of lengthy expressions in the abstract. Examples include using "COF" for crystal orientation fabrics, "UP80%" for the uppermost approximately 80% thickness zone, and "LO20%" for the lowermost approximately 20% thickness zone. Without employing these acronyms, the readability of the abstract would suffer due to the repetition of lengthy terms. Regarding for the section titles in the paper, we used "COF" 9 times. This strategy allows readers to intuitively understand the abbreviation each time it appears, rather than navigating through lengthy expressions repeatedly. We ask readers to recognize the clear benefits of this approach.

Specific comments along the text

[R12] l. 24-25 (abstract): dynamic recrystallization is characterized by nucleation of new grains and grain boundary migration. The main basis processes have been described for a long time (see e.g. Poirier 1985, Humphreys and Haterly 2004) and please refer to the existing frame. Therefore this sentence is not clear “dynamic recrystallization related to this emergence”.

[A12] We will rephrase it to "migration recrystallization".

[R13] l. 43: what is meant by “dynamical analysis”?

[A13] We will rephrase it to "analysis of ice cores in terms of ice dynamics".

[R14] l. 79: “remains” to be done?

[A14] We will add “to be done”.

Part 3.1:

[R15] What about measurement of fabrics that depart from cluster-shape with the DTM technique? We have observed some, for instance along the NEEM ice core (Montagnat et al. 2014, The Cryosphere), likely resulting from a tension component of the stress field.

[A15] Thanks for the comment. DTM is useful to detect $\Delta\varepsilon$ values in the girdle type ice fabrics. We can refer to basic principle in Figure C1 in Appendix C, as girdle distribution of a-axes in the figure. This will be mentioned in the revised version.

Part 3.2 and 3.3:

[R16] Why is it necessary to perform all these different measurements? How complementary are they?

[A16] Please refer to [A0_4] above.

[R17] Do you have enough grains in the X-ray thin sections?

[A17] Please refer to number of grains extracted from each thin section in Figure 4. “Enough grains or not” will depend on requirement for statistical analysis. Also, please refer to [A5] above.

[R18] Why don't you measure the grain size from the automatic fabric analyser?

[A18] The grain size data were established immediately after the drilling based on observations of the ice core surfaces.

Part 4:

[R19] Figure 4 is of very low quality. Please improve it in the final version.

[A19] In revision, better quality file will be provided.

[R20] Content of appendix C is very often referred to and is highly necessary to understand the data. Maybe it should be put back in the main text.

[A20] Though there is this possibility, this is technical detail for correction. We will keep it in Appendix because more focus on science is the priority in the main text.

[R21] Part 4.4: the quality of the writing should be improved in this part.

[A21] We will attempt to improve.

[R22] What does "an axis orthogonal to the shear plane and the IACC will deviate" means?

[A22] It means that misorientation angle between an axis orthogonal to the shear plane and the IACC will appear and increase. Also, please refer to the explanations in Figure 9. In principle, it means the deviation between the IACC and the IAVL.

[R23] Part 4.5: to my point of view, the microstructure features analyses are based on too few grains, and on 2D observations of 3D mechanisms, and must therefore be taken with care unless they are statistically significant (for instance if the shape of grains have been characterized over a large enough number of grains and sections, etc.).

[A23] We consider the issue of statistical significance to be unavoidable in thin-section measurements for COF and microstructures. In the current manuscript, we present examples of noteworthy microstructures. Between depths of 2400 and 3000 m, we observed 15 thin sections using optical microscopy (and G50) and analyzed 43 polarized images (by Laue X-ray measurements). We identified brick-wall patterns in all impure layers, except in very deep sections. In the revised manuscript, we will include quantitative discussions on the grain shape in both impure and less-impure layers, such as the aspect ratio of grains. Furthermore, we will add the following sentence:

In the present study, we provide examples of noteworthy microstructures. Further investigations are necessary to thoroughly discuss the detailed evolution of these microstructures. Such analyses could include examining the misorientation angles of nucleated grains or sGBs, investigating the depth variation in the frequency and contribution of dynamic recrystallization, and exploring the mechanisms behind the brick-wall pattern observed in impure layers.

[R24] Moreover, extraction of the ice core leads to stress relaxations, especially around bubbles or clathrates in the deeper part of the core, that induce a lot of dislocations substructures (like subgrains). Care must be therefore taken when analyzing subgrain-scale features from these samples.

[A24]
Thank you for the general caution regarding the handling of ice cores. We agree with this point. Care must be taken to minimize post-drilling relaxation, or, should it occur, we need to detect it. For the Dome Fuji ice core, the cores have been preserved at a very cold temperature of -50°C at Hokkaido University, and later at the same temperature at NIPR. Under these cold conditions, we attempt to suppress (or slow down) the occurrence of relaxation phenomena. What we can do is maintain the ice under very cold conditions. To our knowledge, this is the best practice for handling ice cores in terms of temperature management within our community.

[R25] When recrystallization is dominated by grain boundary migration (such as in the bottom of the GRIP core, de La Chapelle et al. 1998 (J. Geophys. Res) or along the NEEM core, Montagnat et al. 2014, for instance), the grain boundaries are highly serrated, and it can be observed on a large number of grains, therefore with a statistical value that overcome the 2D sectioning effect, or the impact of stress relaxation.

[A25] In the DF core, we did not find such serrated features. Perhaps it is related to difference in conditions such as ice age and/or stress strain conditions.

Part 5:
[R26] As mentioned in the global statements, the discussion would need being re-organized, with a clearer focus on the selected results and observations that bring something new to the "story".

[A26] Please refer to [A0_1].

[R27] Overall, the authors should bring on numerous existing observations and analyses, especially for dynamic recrystallization and its impact on COF and microstructures, and its interactions with impurity contents. Most analyses and interpretations remain vague and not precise enough, some are not far from being wrong. See below for details.

[A27] We will provide our reply point by point below.

[R28_1] Part 5.1.1: a highly concentrated clustered COF is related either to deformation by compression, with no dynamic recrystallization (since dynamic recrystallization opens the texture close to 45° from the compression direction, see e.g. work of Jacka's team in the 80's and 90's or more recently Montagnat et al. 2009 PICR2, Montagnat et al. 2015, Frontiers in Earth Sciences), or to simple shear.

[A28_1] We agree that a highly-clustered COF is related either to deformation by compression, with no dynamic recrystallization, or to simple shear. We also agree that this concept was already established in late 80's. Related papers at this stage include, Azuma & Higashi, 1985; Budd & Jacka, 1989; Jacka & Budd, 1989. Additionally, Alley, 1988 compiled ideas at that time and gave a synthesis. There were many other papers related to this subject at that time and later time. The reviewer suggested us two papers, one entitled as "Recrystallization Processes in Granular Ice" and another "Analysis of Dynamic Recrystallization of Ice from EBSD Orientation Mapping". Our view is that these papers on laboratory measurements are surely related to this subject, but perhaps slightly far to use as key reference papers here.

[R28_2] In Montagnat et al. 2012 (Talos Dome ice core, EPSL), we have shown that compression alone could not explain a strong COF of the type the authors are measuring here, but that some simple shear is necessary. It seems to be the case here too.

[A28_2] Please refer to [A0_2] and [A0_3]. It is one of points which we will develop explicitly in the revised version.

[R29] Simple shear could be assisted by dynamic recrystallization (dynamic recrystallization) but it is less straightforward to distinguish since dynamic recrystallization strengthens simple shear textures (see Bouchez and Duval 1982, in Textures and Microstructures, or Journaux et al. 2019, The Cryosphere).

[A29] In the suggested papers of laboratory experiments under temperatures near the melting point, strain rate is larger than those in the Antarctic ice sheet by about 10^6 times or more. To discuss analogy with dome ice cores, extensive discussions (even papers) are necessary, which is beyond the scope of the present paper.

[R30] L. 287: dynamic recrystallization is not a deformation process! This is an accommodation process that reduces the local strain and stress heterogeneities and facilitated further deformation by dislocation creep.

[A30] We plan to repair expressions so that the paper gives no misunderstanding to readers.

[R31] Diffusional creep is very unlikely to occur along deep ice cores, first because the required densities of vacancies or interstitials are not met and also because it would not lead to the observed textures and dynamic recrystallization mechanisms.

[A31] We understand the reviewer's view. However, it seems also impossible to rule out this possibility conclusively.

[R32] L. 290-294: the relationship between climatic period (and impurity content) and microstructure has been studied a long time ago, and should be referred to here.

See for instance Duval and Lorius 1980 (EPSL), Weiss et al. 2002 (Ann. Glaciol, at Dome Concordia), Durand et al. 2006 (JGR). The role of GB pinning on GB migration and therefore on the dynamic recrystallization mechanisms has been widely documented and even modeled in some of these articles.

[A32] In Section 5.1.1 containing the L.290-294 in it, nothing was discussed on the relationship between climatic period (and impurity content) and microstructure. Relationship between climatic period (impurity content) and microstructure is discussed in other section (e.g., Sect. 5.2.3).

[R33] The sentence "We interpret the wide scatter as indicative of grain nucleation and subsequent growth" is very vague. How is this interpretation related to already known mechanisms of dynamic recrystallization?

[A33] We cite two sentences here. "*We suggest that evidence includes a wide scatter of dots with low values of $\Delta\varepsilon$ in figures, leading to smaller average $\Delta\varepsilon$ values in interglacial periods, accompanied by large SD values. We interpret the wide scatter as indicative of migration recrystallization, and grain nucleation and subsequent growth.*"
We meant simply here that the wide scatter was not caused by errors but this physical mechanism. Our views for recrystallization are given in several sections within this preprint, citing important papers. This location of the paper is not suitable for developing interpretation related to already known mechanisms of dynamic recrystallization.
In the revised version, we will attempt to make intended meaning clearer. In addition, along with [R32], we need to seek for a proper location to mention already known mechanisms of dynamic recrystallization.

[R34] About the scatter of the permittivity signal, I could also interpret it as being related to the small measurement step regarding the size of the grains, that is the characteristic scale of the COF pattern. I would expect a measuring step lower that this grain size to create this variability as each measurement only account for a low number of grains. Then, the effect of the variability is reduced by the moving averaging.

[A34] In the paper, we deliberately present raw data to show variability. Bulk values are provided as averages over 50 cm. Please refer to [A5], too.

- Part 5.1.2:

[R35] l. 315-316. It is very well know that dislocation creep and dynamic recrystallization mechanisms are temperature-controled mechanisms! No need to “speculate” it. Indeed, both dislocation velocity and grain boundary migration are temperature-dependent through Arhenius type of law…

[A35] We will repair this point in revision.

[R36] I don’t see why molecular diffusion is mentioned here?

[A36] We agree that our expression was not good enough. We intended to express thermal activation of molecular processes as follows. “In ice physics, it is well-accepted that the mass transport phenomena such as plastic deformation, the molecular diffusion process and recrystallization are highly temperature-dependent.”

[R37] l. 321-322. What is meant by “more effective as a condition to trigger active emergence of recrystallization”? A strong clustered texture will be hard to compress, and therefore dynamic recrystallization is necessary as an accommodation process (or fracturing at higher strain rates) to enable further deformation, but such a clustered texture is very easy to shear (when the cluster orientation is normal to the shear plane, which is the case here), and therefore deformation can occur without or with a limited amount of dynamic recrystallization. This sentence is too vague, and it strengthens the confusion mentioned before about the role of simple shear in the origin of the highly clustered texture.

[A37] We were incorrect on temperature of GRIP core ice. Thus, we need to correct statements at this part of the paper. As for l. 319-322 in the preprint, we will rewrite this part as,

“*In this hypothesis, at the bottom of the UP80%, the thickness of ice is approximately 10% of the original ice equivalent thickness at the time of deposition (Figure 9f). At this depth, the eigenvalue* $a_3^{(2)}$ *reaches about 0.93. In the absence (or faint presence) of shear stress, a strongly clustered texture will be difficult to compress or shear, thereby necessitating dynamic* *recrystallization as an accommodation process. This state of saturation of the c-axis cluster, along with the common temperature range may be more effective as a condition to trigger active emergence of recrystallization.*"

In the UP80%, we find no indication of shear. In the LO20%, weaker cluster of COF is not very favorable for shear.

[R38] The texture is also likely more clustered in the glacial ice because dynamic recrystallization is expected to be less active due to GB pinning.

[A38] Yes, it is one of points in our discussions.

[R39] l. 330-333: there is nothing new in this statement… please refer to existing work about dynamic recrystallization along ice core that I have mentioned before.

[A39] Based on the comments, we remove two sentences here. Please refer to [A8], too.

- Part 5.2.1:

[R40] L. 350-351: "The recrystallization processes require thermally activated molecular diffusion"… This sentence is very vague and likely incorrect. Recrystallization occurs mostly by dislocation mobility, either within the crystals (formation of subgrains, dislocation pile-ups, etc.) or at GB by enabling grain boundary migration (under the driving force of dislocation-based strain energy). I don't see why molecular diffusion is mentioned here.

[A40] We will rewrite first part of 5.2.1 as follows. We simplified description to avoid any misleading or misunderstanding.

In ice physics, it is well-accepted that the mass transport phenomena such as plastic deformation, the molecular diffusion process and recrystallization in are highly temperature-dependent (e.g., Petrenko & Whitworth, 1999). The conditions of ice sheets in Antarctica, in terms of temperature and stress, are located on a boundary zone between dislocation and diffusional creep on the deformation mechanism map (e.g., Duval et al., 1983; Goodman et al., 1981; Shoji, 1978). When ice is under temperatures close to the melting point in the LO20%, its viscosity is lower, and diffusion coefficients are higher compared to the colder ice, the rate at which recrystallization occurs also increases with temperature (e.g., Petrenko and Whitworth, 1999).

[R41] L. 356: The shear strain component is only mentioned here. It should be evoked much before, see previous comments about part 5.1.1.

[A41] Please refer to [A0_3]. In our view, shear is not necessary for explanation in the UP80% at DF and EDC.

- Part 5.2.2:

[R42] l. 369-370: how is the mechanism mentioned here possible? The different layers are linked together and cannot rotate or shear independently? Dislocation creep accommodated by dynamic recrystallization tends to align the c-axes with the vertical to the shear plane (see references mentioned before), and the observed texture is, to my point of view, related to the fact that the shear plane is not horizontal?

[A42] Please let us to explain. Yes, the adjacent layers are linked together, keeping continuity in the ice sheet. When they deform independently (accompanying rotate or shear), it cannot happen without occurrence of local shear between adjacent layers. When strain is still small, local shear at each layer boundaries will be small, sometimes being within grain scale strains. However, when strain grows, layers need to be unevenly deformed, showing features of apparent difference between layers. Such features include appearance of undulation of layers, wavy change of layer thickness from one location to another, or when it grows, it will form boudinage features. We find such features when we move away from Dome Fuji, for example. When we are away more, layer structure itself tends to be disrupted. We find many of such features near Dome Fuji with radar sounding. Dislocation creep accommodated by dynamic recrystallization tends to align the c-axes with the vertical to the shear plane (see Figure 10), and the observed texture is related to the fact that the shear plane is not horizontal. If planes are perfectly horizontal down to the ice sheet bed, no bias, which rotates cluster of c-axis meridionally to a certain direction, will occur. In this case, if recrystallization make c-axes well away from the vertical, these will rotate towards the vertical from all orientations. This is a scenario only if this ideal situation exists. In reality, the cluster axis of single pole COF incline both at Dome Fuji (present study) and at EDC (Figure 6 in Durand et al., 2009) in the LO20% zone. We hope our explanation here makes sense for readers.
Readers can also refer to Section 4.3 of Durand et al., (2007). We can find a view by these authors.

[R43] Fig.10: for me, fig 10d represents bending and not rigid rotation? Can a layer experiment rigid rotation independently of the mechanical constraints that surround it? ("system's rigid-body rotation", l. 372).

[A43] It is just a matter of scale of view. If an observer looks at the ice sheet around the subglacial trench in a horizontal scale of several kilometers, in this scale, ice is bending toward the trench. If an observer sees ice core scale of approximately several hundred meters, ice body is rotating.

[R44] l. 374-375: "Thus, in the circumstantial evidence for dominance of the simple shear…". This sentence appears useless. Why are the angles "inconsistent"? Do we need that for inferring simple shear at these depths?

[A44] First, as we have documented, in the UP80%, shear is not significant. In the LO20%, it is a matter of discussions. Thus, we need to discuss simple shear at these depths. A deviation between the cluster of c-axes and a vector orthogonal to the original layers occurs solely due to the presence of simple shear strain; there are no other possibilities. Therefore, the inconsistency of these angles indicates the dominance of simple shear. Please refer to [A0_3] and [A41], too. It would be very informative if angular relation between IACC and visible layer inclination will be examined in the EDC core. IACC is published as Figure 6 in Durand et al., 2009. If we can access layer inclination data, we can investigate.

[R45] Fig 10a,b,c: the sketches are not precise enough since it depends on the dominant stress, if it is compression or shear. In particular, 10c holds for compression but not for shear. If shear is parallel to the layers, c-axes will be perpendicular to them (with or without dynamic recrystallization).

[A45] Fig 10a,b,c simply sketches angle relation between the IAVL and the IACC as ice becomes deep. Driving force of the ice flow in the gravity in the vertical. There is a steep bedrock with an angle of about 45 degrees. Then, as ice moves closer to bed, simple shear will grow. Precision of the stress/strain configuration is not something which should be discussed here.

- Part 5.2.3:

[R46] There are many repetitions from previous parts, analyses/interpretations that are mixed with discussion… It goes in favor of the necessity to re-organise the discussion with a clearer story to tell, especially focusing on either the confirmation of existing works (that must be referred to) and/or on new interpretations (although they appear to be few in this work).

[A46] Please refer to [A0_1].

[R47] l. 435: about the impact of dust, work by Weiss et al., Durand et al., mentioned before suggest another explanation with dust impeding grain boundary motion during recrystallization, and therefore enhancing the impact of deformation versus recrystallization on the final COF. The final COF will be either less favorable for compression creep or more favorable for horizontal shear… Therefore not so straight forward!

[A47] We agree that the role of dust particles is not so straightforward. However, in our previous studies at the upper 80% of the DF ice core Saruya et al. 2022b, we found that the dust particles could have an impeding effect on the COF clustering. The mechanism is still unresolved. We suppose whether GB pinning occurs or not depends on the various factors, such as GB migration velocity, microparticle sizes, temperature and so on, as it was proposed in Durand et al., 2006. We could find both GB pinning and microparticles segregation at GBs in our observations in the deeper sections.

[R48] l. 441-445: please refer to existing work! Dust particles (and/or insoluble particles?) are mainly located at GBs. They may enhance dislocation production (it has not been clearly proved) BUT they pin GBs. In the works mentioned before (Weiss et al. 2002 for instance), it is shown that the pinning force lead to a critical grain size very close to the one measured in glacial ice.

[A48] According to the review by Stoll et al. (2021b) and a case study for NEEM ice core by Eichler et al. (2017), dust particles located not only at GBs but also at grain interiors and triple junctions. To the best of our knowledge, the role of microparticles in ice deformation (dislocation creep) is not well understood. Production of dislocation is one of the possibilities. In contrast, microparticles may act as sink of dislocation like GBs. There are many existing works regarding impurities. So, we will refer Stoll et al. (2021b) here since this article reviews the impact of impurities on deformation and microstructures of polar ice.

[R49] Durand, Gillet-Chaulet et al. 2007 (Climate of the Past) have modeled the impact of changes in ice viscosity with climatic transitions on ice flow. This work could be mentioned.

[A49] Regarding this, please refer to [A0_3]. We recognise that Durand et al. (2009) concluded that simple shear is the most important factor in the COF contrast between glacial and interglacial periods.

[R50] I think diffusion creep should not be mentioned here, as there are too few evidence of its likeliness to occur… Unless the authors can provide some NEW statements about it.

[A50] While there is no new information about diffusion creep, it is also true that diffusion creep cannot be ruled out. Azuma et al. (2000) suggested that the contribution of diffusion creep might be significant at depths with smaller grains.

[R51] L. 481: please refer to Weiss et al. 2002 and Durand et al. 2006 regarding Dome C.

[A51] Thanks for the comment. We will write this as,
The same trend is common in the Vostok, GRIP and EDC ice cores (Durand et al., 2006; Lipenkov et al., 1989; Thorsteinsson et al., 1997; Weiss et al., 2002).

[R52] l. 481-482: “we can hypothesize that the physical phenomena…” this sentence is too vague. Please be more precise. Which mechanism does what, and what does your observation bring “to the story” already written by previous works?

[A52] Thanks for the comment. To address this comment, we will write sentences including this as,

Azuma et al, (1999; 2000) reported that the grain sizes in the DF1 ice core in the UP80% is correlated with δ18O variation and concentration of impurities, which was confirmed in this study (Figures 5 and 9). That is, grain size, dusts and ions, and δ18O are all well correlated. The same trend is common in the Vostok, GRIP and EDC ice cores (Durand et al., 2006; Lipenkov et al., 1989; Thorsteinsson et al., 1997; Weiss et al., 2002). Thus, we can hypothesize that the grain growth determined by deposition of impurities are common across a wide area of the ice sheet.
We limited the statement to grain growth.

[R53] - Part 5.2.4: again, what is the effect of the grain size, and therefore the number of grains in the measured area on the standard deviation?

[A53] Please refer to [A5] and [A34]. DTM is a volume-weighted average within the volume covered by the Gaussian distribution of the beam. Therefore, the number of grains in the beam (at a spot) is different depending on depth (i.e., grain size). In the LO20%, total number of grains in a beam varies by an order of magnitude from 10 to 10^2. Thus, when grain is large, a spot measurement is not sufficient to derive statistics. However, when we perform moving average, such as over from 10 cm to about 50 cm, total grain number in the moving average is of the order of 10^2 to 10^3. Thus, by making moving averages, we can assume that both averaged value and the SD has no significant grain size effect (no large influence from limited grains) in the DTM data and discussion of COF development. In the revised manuscript, we will explain that the DTM has no grain size effect.

Part 5.3:
- Part 5.3.1:
[R54] To my point of view, this part reveals a misunderstanding of the deformation mechanisms of ice (maybe this is due to the way it is written?). As mentioned in my general statement, GBS should not be evoked here with so few proof of its existence. This is a mechanism that is very unlikely to occur along ice cores (large grains, low strain rate, low stresses, efficient accommodation mechanisms…) and that cannot be proven by a few 2D microstructure observations.

[A54] As indicated by the reviewer, there is no significant evidence that GBS via microshear is occurring. In the revised manuscript, we will emphasize that the GBS via microshear is one of the possible mechanisms that can explain the observed microstructure (brick-wall pattern) in the impure layers. We believe that it is worth mentioning that the brick-wall patterns were observed only in impure layers. Also, we will add quantitative discussions on grain shape (e.g., aspect ratio of grains). Also, please refer to [A9] above.

[R55] Microshear is mentioned, but I personally don’t know what it means! Any dislocation glide along a basal plane created a microshear…

[A55] Microshear deformation mechanism and characteristics of the microstructure are explained in Faria et al. (2009) in detail. Their interpretation was based on analogue experiments by Bons and Jessel (1999) using octachloropropane. Llorens et al. (2016) also discussed based on the numerical simulation. Please see also [A9 and A54].

[R56] How can GBS contribute to grain size reduction? How can it create new boundaries?? Please refer to the work of Ashby 1973 (Acta Metallurgica) for instance to better understand GBS. GBS is not a recrystallization mechanism! This is a deformation mechanism that is likely to take place in very fine-grained materials leading to superplasticity, in very specific conditions.

[A56] According to Faria et al. (2009), the formation of microshear bands creates new, flat GBs parallel to the shear plane, resulting in the grain size reduction by splitting the grain in two. This behavior is similar to rotation recrystallization (as stated in L549). Also, please refer to [A9] above.

[R57] To correctly interpret fig. 12, one would need the measurement of misorientations in order to be able to provide a clear distinction between subgrains, grains, and interpret them in terms of mechanisms at play. For instance, figure 12a could also present an example of some GB pinning by a subgrain as observed in Montagnat et al. 2015. In figures 12f and g, it is not straightforward to interpret the observations as grain boundary migration. The shape of the GB is not sufficient to my point of view. In figure 12i I do not see quadruple junctions but the effect of a 2D sectioning of a larger grain. Just to say that these figures can easily be "over interpreted" in line with our scientific prejudice.

[A57] In our measurements, it is difficult to discuss the misorientaiton within a grain. Thus, we provide these figures as possible examples of boundary migration based on the shape of GBs and sGBs. Microstructural examples of migration recrystallization with and without nucleation are shown in following articles: Fig.4 in Weikusat et al. (2009), JoG; Fig.7 in Kipfstuhl et al. (2009), JGR; Figs 5 and 11 in Faria et al. (2014b). According to them, for example, many sGBs and dislocation walls irradiating from a bulged grain boundary, a newly nucleated grain grows into the highly strained region characterized by numerous sGBs and dislocation walls. Our observations shown in Figure 12f,g are consistent to their interpretations. In Figure 12e,h, we could observe microparticles segregation and pinning. Thin lines adjacent to GBs are the reverse side GBs. So microparticles are segregated on the migration planes of the crystal grains (2.5D). Additionally, pinning by microparticles could indicate boundary migration (e.g., Stoll et al., 2021b). For these reasons, we interpreted that the boundary migration and migration recrystallization occurred. In Figure 12i, we interpreted the two locations indicated by the arrows as quadruple junctions. However, we cannot exclude the effect of a 2D sectioning. We will remove it. Instead, we emphasize that there are no evident brick-wall patterns (as shown in panel a and b) despite the high concentration of dust particles. We might integrate that description into Figure 6e.

In the revised manuscript, we will reconstruct microstructure images (Figure 12) to provide clear examples and add further explanation with appropriate references.

- Part 5.3.2:

[R58] l.570: the statement about the orientation of nucleus can not be proven with the presented results. Observations, with direct measurements, of nucleus orientations show that they are mainly oriented close to their parent grains (see e.g. Chauve et al. 2017, Phil Trans Roy Soc A).

[A58] In the revised manuscript, we will reconstruct "grain nucleation" images to provide clear examples and focus on the strain induced grain boundary migration with nucleation (SIBM-N) (nomenclature following Faria et al., 2014b).

[R59] l. 584: Figure 12 can not be used to prove the existence of GBM, please be more cautious in your statement. Some more proof is required.

[A59] Please refer to [A57] above.

[R60] Similarly, the only observations in figure 12 c1 and d1 of square-looking grains are not enough to demonstrate GBS since, first, they could also be a 2D sectioning effect, and second, more statistics is required to demonstrate GBS. Please be more cautious or remove.

[A60] We showed Figure 12 c1 and d1 as possible examples of nucleated grains. They have no internal structures. In the revised manuscript, we will reconstruct "grain nucleation" images to provide clear examples and focus on the strain induced grain boundary migration with nucleation (SIBM-N) (nomenclature following Faria et al., 2014b).

[R61] "The orientations of the c-axis in nucleated grains…" you do not have any direct observation of nucleus orientations, and some existing observations (e.g. Chauve et al. 2017, but maybe not only) are not in favor of such a statement. Please be more cautious or remove.

[A61] We will remove the sentence because we do not have direct observation of the nucleus orientations. In a situation that we find so wide scatter of c-axis, it is highly likely that such small grains with c-axis well away from the surrounding grains are nucleated grain, which appeared in the LO20%. Of course, we are not observing a moment when nucleation is occurring.

[R62] l. 585: "The presence of numerous sGBs implies high stored strain energy". This statement is wrong. Stored energy is related to the total dislocation density, during deformation, while sGBs only represent the population of geometrically necessary dislocations that remains after unloading! There exist not link between the density of geometrically necessary dislocations and the total dislocation density, and Lopez-Sanchez et al. 2023 (EPSL) have even shown that you can have a lot of strain in some given grains and NO sGBs remaining after unloading. Please remove.

[A62] We rephrase to "highly and heterogeneously strained region" with references Faria et al. (2014b) and Stoll et al. (2021a).

[R63] For the remaining discussion related to nucleation and dynamic recrystallization, please refer and mention existing work in order not to “propose” explanations that have already been given, but rather to “confirm” that the processes you observe are in phase with previous observations and analyses of dynamic recrystallization along ice cores.

[A63] In the revised manuscript, we will add references when we find it appropriate. We will state earlier knowledge as it is.

[R64] l. 596: please site Weiss et al. 2002 and/or Durand et al. 2006 closely related to what you observed instead of Cuffey and Patterson that is a general book.

[A64] We will refer to Durand et al. (2006) for the case study of EDC ice core and Stoll et al. (2021b) as a review paper.

[R65] l. 603: Please remove the following statement “Given the high concentration of dust particles, it is likely that the area experienced GBS via microshear” since, first, you have not enough proof for such an assertion, secondly, how can dust particle concentration be linked to GBS, where does that come from, and finally because the meaning of “GBS via microshear” is very uncertain!

[A65] We will modify it as follows:
There are no evident brick-wall patterns observed in panel a and b despite the concentration of dust particles is high. We suspect that the patterns could be lost by recovery and recrystallization due to high temperature close to melting point.

Part 5.4:
[R66] The zoning must be put in relation with what has already been observed and analyzed in other ice cores. For instance, Dome C, Talos Dome, NEEM ice cores. Along Talos Dome we have shown that some dynamic recrystallization was necessary to reproduce the observed COF in complement to dislocation creep (this latter would lead to too strong COF) (Montagnat et al. 2012, EPSL). Along the NEEM ice core we have observed the impact of shear on the COF in exact connection with the change in the climatic origin of the ice, for instance.

[A66] Considering the similar development of COF in each dome site core (DF and EDC in Fig.8; EDC and GRIP from Durand et al., 2009; depth of maximum COF cluster in Table 3), we believe that the zoning of DF ice core could be applied to EDC and GRIP ice cores. However, regarding the detailed zoning defined in the DF ice core depths at below 2400 m, it is difficult to discern in the EDC and GRIP core because the COF fluctuation is too large, and data are sparser.
We will not mention the NEEM ice core since it is not a dome core. We should not make complex mixing. Status of the Talos Dome is as we stated in [A0_2].

Part 5.5:
[R67] Information about COF and the resulting layering of ice viscosity along ice cores cannot be straightforward related to the dating of ice core or what is called "radioglaciology" (if I understood well?). See e.g. Durand et al. 2007 (Climate of the Past) or Buiron et al. 2011 about Talos Dome age-scale (Climate of the Past). Maybe some recent radar measurements studies could be mentioned here too.

[A67] Please refer to [A0_2]. In the present paper, dating or strain complexity at coastal ridge at the edge of the ice sheet plateau is not, at least, one of major subjects. Priority is low to develop statements on the Talos site. It will make discussions unnecessarily complex.

We already cited key papers that explicitly discussed radio echo free zone. If we find some paper suitable to add as citation in this context, we will do it. We continue to survey literatures.

Conclusion:

[R68] l. 689: I think it is not correct to mention here the "elongated shape" of crystals that has not been statistically characterized, and for which you can not quantify the relative occurrence. You could mention it in the text as an example of some local observations but not in the conclusion since it will give too much weight to this weak observation.

[A68] We will add quantitative discussion in the revised manuscript (main text or appendix). We investigated the aspect ratio of grains in impure layers and other (interglacial) layers.

[R69] l. 690: same comment about this mention of GBS and microshear that has not been shown to occur significantly in your observation, with strong enough statistics and scientific evidence, to worth being mentioned as an important mechanism unless with the aim to orient the reader interpretation.

[A69] We will not mention GBS via microshear in Conclusion because this mechanism is one of the possibilities of mechanism that can explain the microstructures (brick-wall pattern) of impure layers and there is not significant evidence.

[R70] l. 705: temperature also play a role in the observed mechanisms, not only strain. For instance, along NEEM we observed dynamic recrystallization high along the core that was not observed for the same amount of strain along Dome C or Talos Dome.

[A70] Temperature is also an important factor in controlling recrystallization process. We will add “temperature”.

Conclusive statement:
[R71] I recommend that major revisions are made prior to publication of this article. In particular, attention must be paid to providing sufficiently well-founded scientific evidence before concluding on the importance of a mechanism.

[A71] With an opportunity of the major revision, we will largely improve this paper. Also, please refer to [A0_1].

[R72] There is also a lack of reference to the work that have already been done for many years about ice core texture and microstructure measurements and their interpretation. This should be corrected.

[A72] We will add many reference papers widely.

[R73] At the end, the novelty of the study stands only on the fact that these deep core measurements have never been published. Care should be taken in the discussion in order to focus on what is potentially new or relevant for the community.

[A73] In the present study, we obtained many results and insights listed in the conclusion section in terms of layered structures and physical properties in the Dome ice core. Especially, knowledge of the deep sections is significantly valuable because there is limited information available so far. Although the discussion of physical processes (such as deformation mechanisms and microstructures) in deep Dome ice core has long been stalled since EDC studies by Durand et al. (2007; 2009), we believe that the new findings obtained by present study would activate debate again (with knowledge from EDML, East GRIP and Oldest ice cores).

Finally, we wish to express once again our sincere gratitude to the reviewer for dedicating the reviewer’s valuable time to review our manuscript and for providing critical insights aimed at its improvement.

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