The Cryosphere Discuss.,
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Interactive comment on "The temperature change shortcut: effects of mid-experiment temperature changes on the deformation of polycrystalline ice" *by* Lisa Craw et al.

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The manuscript is well written, has clear objectives, and is of interest for the cryo-community. It shows progress in the field of deformation experiments on polycrystalline ice and explores the approach to reduce run-time by changing the temperature at a certain stage. The manuscript is presented in a logical structure and visualizations are well chosen.

Points which could be improved are a clearer use of terminology (microstructure vs. fabric and texture) and a stronger emphasis on the fact that the conditions of the experiments differ from polar ice sheets and are not easily transferable. Furthermore, I

suggest to use SPO data to quantify changes in the texture and to add (more) references to some parts of the discussion.

Furthermore, I suggest some technical corrections. All in all, the manuscript is of high quality and I recommend the publication of this manuscript in TC subject to minor revisions.

Specific comments:

1. Abstract p1. L.2 We can do this by… Is "this" referring to understanding the mechanical properties of flowing ice, modelling of the dynamics of ice sheets, or predicting the behaviour in the future? Please rephrase to enhance clarity.

2. Abstract p.1 l.4 conditions in ice sheets and ice shelves extend to low temperatures (<-5°C). The temperatures in the majority of ice sheets and ice shelves is well below -5°C, a lower temperature value might be more appropriate. See more detailed comment below.

3. Aims of the study are explained on p. 2. l. 32fff and on p.5 l.43ff. They differ in details (e.g., tertiary creep only mentioned on p. 2. l. 32fff), thus it might be appropriate to combine both sections into one, placed at the end of the introduction to increase readability.

4. P.2 l. 45 Primary creep: The definition is a little bit too simplified, maybe include some information from e.g., Faria et al. (2014b): "During the first creep stage, usually called transient or primary creep, the strain rate decreases rapidly. This deceleration is due to work hardening mainly produced by the load transfer from easyglide to hard-glide systems and the increasing strain incompatibilities between the grains, which build up internal stresses and localized heterogeneous strains (Wilson, 1986; Petrenko and Whitworth, 1999; Schulson and Duval, 2009; cf. Sect. 2.2), both clearly identified by the formation of the first dislocation walls and subgrain boundaries (Hamann et al., 2007; Sect. 4.1). Primary creep in ice extends to about 1% of strain, irrespective

of temperature or stress (Budd and Jacka, 1989), and a considerable fraction of it consists of a recoverable "delayed-elastic" strain (sometimes also called "anelastic" strain), implying that part of the deformation is recovered after the load is removed, in a relaxation process that can take several hours (Duval, 1978). Budd and Jacka (1989) report primary recoverable strains of 0.15% and 0.30% for isotropic polycrystalline ice at 10 C compressed at 0.2 MPa and 1.0 MPa octahedral stress, respectively. It is believed that the delayed elasticity of ice is mainly caused by the relaxation of internal stresses by dislocation back-gliding (Glen, 1975; Cole, 2004; Schulson and Duval, 2009)."

5. P4. L10fff: The impact of GBM is strong on texture (grain size, grain shape, SPO) and on grain growth and reduction (dynamic grain growth, see Steinbach et al., 2017), but not so much on CPO (fabric)(e.g., Llorens et al., 2016a, b). Terminology is not always used in the same way in earth and material sciences, so please define "microstructure" in the beginning. Otherwise it is difficult to distinguish between microstructure (glaciology: fabric + texture), fabric and texture (see also comment 8).

6. P. 5 l. 25ff: references for examples of other experiments, in-situ data, extrapolations missing.

7. P. 5 l. 43 it should be clarified that the systematic study is undertaken on laboratory ice.

8. It is mentioned on p. 9 l. 101 that SPO data was derived, but this data is not further used in the manuscript. Showing and discussing SPO data would be a good way to further quantify the microstructural changes, i.e. changes in the ice texture. In this case it is possible to visually analyse the microstructure of the thin section images, however, I would suggest to add SPO data to fully fulfil the statement of "quantifiable differences in the microstructure" as described in the abstract on p.1 l. 14. Otherwise, if only CPO-data is used to quantify changes I suggest to rephrase the wording to e.g., "quantifiable differences in the fabric" to avoid confusion.

9. For practical reasons the temperatures of the conducted experiments are rather high compared to temperatures in deep ice sheets (- 30°C - -20°C) (e.g., Dahl-Jensen et al., 1998, Mony et al. (2020) and, to a lesser degree, in ice shelves (-25 - -10°C for meteoric ice) (e.g., Rist et al., 2002). Temperatures of >-10°C are rather found in shallow, or the deepest parts of deep ice sheets, close to bedrock. There are studies on warm ice from glaciers (e.g., Hellmann TCD https://tc.copernicus.org/preprints/tc-2020-133/) so this shouldn't be mixed up. This is especially important since samples from set 2 did not match the desired outcome, thus there are still limits to the usability of this method. On p. 14 l.16 you state that the microstructure is "failing to match those conducted entirely at -10°C". In the conclusion on p. 14 l. 40 this is slightly emphasized by stating that the microstructure is "not [..] truly representative", please clarify this. Deformation mechanism maps might help to bring together different regimes (glaciers, ice sheets, ice shelves) e.g., RX diagram in Faria et al. (2014b), Frost & Ashby (http://engineering.dartmouth.edu/defmech/) and Shoji and Higashi (1978, https://doi.org/10.3189/S002214300003358X). This might go beyond the scope of the manuscript, but should be kept in mind.

10. Tests were conducted on laboratory ice only. Natural ice has different, and highly variable, properties regarding e.g., absolute impurity content and spatial distribution of inclusions (cloudy bands) etc., which are reported to affect the rheological parameters, and thus the deformation, of ice. It should be emphasized in the discussion and the conclusions that the discussed results are not as simply transferable to natural ice as currently concluded (p.15 l.45). The study is an important step forward, but more research is needed to verify the easy upscaling to ice sheets and ice shelves.

11. 2.1 Laboratory: After cutting and polishing the samples, were they left for sublimation? Please address this issue briefly in the text since this can have an impact on the texture (grain shape and size) and on the quality of the FA measurements.

12. P. 12 Fig 5: LC023 has visibly, and measurably, much smaller grains than LC021, LC025, and LC026 and a rather homologous bulk grain size. This should be briefly

mentioned in the final section of the results indicating the small-scale differences in polycrystalline ice.

13. I suggest to combine the first sentences on p. 13 l. 47ff or to add some references in the first sentence.

14. References needed in final discussion paragraph on p. 14 l. 17fff discussing GBM, BLG, and other lower-temperature experiments.

15. Fig. 4 and Fig. 5: What is the reason for showing the c-axis orientation of 5000 pixels rather than using the c-axes of the actual grains as described for deriving the CPO in section 2.3? The number of grains is lower than 5000 and CPO contour plots thus probably look different when referring to the grains.

16. Appendix p. 15 l.5f. What is the reason to remove elongated small grains? Is it motivated by the possibility of artificially introduced grains due to segmentation/grain reconstruction? Were images manually checked for such grains? Please explain.

17. Do you have ideas what could have caused the two troughs in strain rate observed at LC025 at -10°C? (Fig.3d)

References: D. Dahl-Jensen, K. Mosegaard, N. Gundestrup, G. D. Clow, S. J. Johnsen, A. W. Hansen and N. Balling. Past Temperatures Directly from the Greenland Ice Sheet. Science 282 (5387), 268-271. DOI: 10.1126/science.282.5387.268

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Technical corrections:

1. Author list: Jason Roberts4,6

2. wording sometimes not consistent e.g., behaviour and behavior, e.g. and e.g.,

3. Abbreviations sometimes inconsistent: CPO defined on p. 2 l. 29 and on p. 3 l. 52 GBM defined on p.4 l. 9 and on p. 13 l. 54. Shortly after introducing the abbreviation on p.4 l. 9 it is spelled out completely (p.4 l. 12)

4. Fig. 1 and Table 1: Units are placed after a comma, in the other plots units are encased by brackets

5. Suggestions for Fig. 2: grey temperature data could be a bit stronger, it′s not easy to see in a printed version 2a: the x 10ˆ-8 might be placed somewhere else to increase visibility

6. P. 4 l. 64 one bracket too much

7. P.8 l.72: (LC001, LC002 and LC009)

8. P. 8 l.94 higher

9. Fig. 4 and 5: there is enough space to enhance the font size of the text in the legend in the upper right (text in Schmidt plot and parallel to the density colour scale).

(Contour plots) instead of (Conotur plots)

10. Appendix A: p.15 l. 51 G60 or G50 data? The Fabric Analyzer G60 has already been used, but you probably refer to the FA G50 here.

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