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Solid Earth
Discussions

Interactive comment on "Micro-scale and nano-scale strain mapping techniques applied to creep of rocks" *by* Alejandra Quintanilla-Terminel et al.

L. Hansen (Referee)

lars.hansen@earth.ox.ac.uk

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It was a pleasure to read and review "Micro-scale and nano-scale strain mapping techniques applied to creep of rocks" by Quintanilla-Terminel, Zimmerman, Evans, and Kohlstedt. This manuscript outlines state-of-the-art techniques for analyzing the distribution of strain at length scales below the grain size in crystalline solids. As pointed out by the authors, these techniques are invaluable for determining the microphysical mechanisms of deformation and evaluating constitutive models used to extrapolate predictions of mechanical behavior from the laboratory to geological conditions. Small-scale strain mapping has been developed and used extensively in the metallurgical literature, but these techniques have seen little application to geological materials because of the need for extreme conditions during deformation (a notable exception that should be mentioned is ice, e.g., Grennerat et al., Acta Materialia, 2012). In the current manuscript, the authors have explored and documented the laboratory procedures and computational techniques allowing small-scale strain mapping to be used at high temperatures and confining pressures. This is certainly a worthwhile contribution suitable for publication in Solid Earth pending revision.

I have two general comments followed by some minor specific and technical comments.

First, it would be useful for me to have the benefits of the "Regular Grid" laid out more clearly. The text suggests that the technique requires images of the same region before and after deformation. If this is strictly true, then it needs to be stated more clearly and upfront in the manuscript. In addition, if before and after images are necessary, then it is a bit unclear to me why a regular grid is beneficial over random markers like those used in digital image correlation (DIC). There are several points in the text discussing DIC for which I've made specific comments below. The authors make the point that DIC requires continuous in situ imaging, but I think this phrasing is misleading. Most in situ techniques pause the actuators when imaging because the deformation actuators introduce too much mechanical vibration. Thus, there is a "before" and "after" image taken, similar to the technique described here. The similarities extend further. DIC use particle tracking or cross correlation to get displacement fields describing the motion of the particles from one image to the next. Much of the discussion of the Regular Grid technique sounds like a similar process. The centroids of a group of particles are compared in "before" and "after" images to determine the displacement of the centroid from one image to the next. Is there an additional benefit of the regular grid that I'm missing?

I do wonder if there is a way to implement the Regular Grid method without a "before" image. If true, I would see this as a major benefit of the method. Because the grid is, by definition, regular, you already know the original grid spacing and therefore could simply compare the deformed grid to a synthetic reference. Is something like this possible?

Second, I think the manuscript is lacking a discussion of a critical topic, the resolution of the techniques developed. Resolution is discussed in qualitative terms, primarily focussing on the need for finer grid spacings to evaluate smaller deformations. However, it would be valuable to quantify achievable resolutions for different experimental setups. For instance, what is the precision in locating the markers or in locating the centroid of a group of markers? What is the minimum strain that is resolvable with these techniques? What is the measured strain if a sample is put together but not deformed (this would provide an estimate of the noise floor for the strain measurement)? How does the strain resolution vary between reflected light and SEM? How does the strain resolution depend on n (in the n-point technique)? Addressing some of these questions would be very helpful in evaluating whether these techniques are appropriate for specific future applications.

Again, I think the manuscript is an important contribution overall and expect any revisions resulting from my general, specific, and technical comments to be relatively minor. I'm highly supportive of publication of a revised version in Solid Earth.

Sincerely,

Lars Hansen, Department of Earth Sciences, University of Oxford

Specific comments:

Line 26: Perhaps it is worth noting what is mean by "dominant". Are the authors referring to the process that contributes the most to the total strain? Or are they referring to the process that limits the strain rate? Also, what is meant by "deformation mechanism"? Does this refer to the individual strain producing processes listed in the first sentence of the paragraph? Or are the authors referring to combinations of processes, such as dislocation creep, which might consist of dislocations moving by glide and climb.

Line 36: This seems like a bit of a red herring. Yes, in situ observation is great, but I'm not really sure what it gains you other than the ease of not having to move a sample between apparatus at each strain step. Most in situ experiments are stepwise in nature anyway since imaging can't be carried out while actuators are moving.

Line 66: Where is the polymer in this context? How is it being transformed? This is detailed below, but it is a bit confusing at this point in the text when the method has not yet been explained.

Line 235: I think it is worth stating here what "continuous description" means and why it is necessary.

Line 240: It is worth noting that this technique requires images of the same region before and after deformation. Also, isn't this method just DIC but using laser speckles as the markers? If so, perhaps section 3.1.1 should come after 3.1.2.

Line 255: Again, the authors need to describe why continuous observation is a necessity.

Lines 308 to 317: The text here discusses measurement of strain first and then discusses measurement of the deformation gradient tensor. However, isn't this out of order considering the next section suggests F is determined first and then decomposed into strains and rotations?

Line 324: This section describes the analysis for 2-D strain inversion. However, earlier in the section (Line 288), the authors suggest 3-D strain can be calculated assuming the deformation is isochoric and symmetric about the sample axis. A little more description about this process would be useful. To assume the strain is symmetric about the sample axis, doesn't one of the 2-D principal strains need to be aligned with the sample axis? It seems unlikely this would be the case in the majority of situations. What seems simpler to me is to assume the 2-D principal strains are also principal strains in 3-D.

Line 372: It is not immediately clear from the figure how the measured strain can be partitioned into strain due to slip on boundaries, twinning, and intragranular deformation. A quick explanation would be useful, or at least a reference to Quintanilla-Terminel and Evans (2016).

Line 425: It is not clear from the text why EBSD (note that “beam” should be “backscatter”) is necessary to evaluate the strain from grain-boundary sliding. Is this just because grain boundaries need to be identified, or is there another explanation?

Technical comments:

Line 34: How about “temporal” instead of “time” to parallel “spatial”?

Line 568: Change “photography” to “photograph”.

Figure 5: What is profile 2? I don’t see it referenced anywhere. Also, there is a stray vertical white line in the lower right corner. I’m not sure what its purpose is.

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