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Interactive comment on "Cataclastic deformation of triaxially deformed, cemented mudrock (Callovo Oxfordian Clay): an experimental study at the micro/nano scale using BIB-SEM" *by* Guillaume Desbois et al.

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The paper "Cataclastic deformation of triaxially deformed, cemented mudrock (Cox Clay) : an experimental study at the micro/nano scale using BIB-SEM", by G. Desbois et al. is well written and well constructed, with a comprehensive rationale. It brings new insights on the deformation mechanisms active during experimental deformation of clay rich rocks from the deep underground (future) French nuclear waste repository. The work is carefully accomplished, thanks to edge cutting facilities (ion abrasion) allowing for preparation of high quality sample surfaces in order to access by SEM to the

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fine scale microstructures and particularly down to the scale of the clay matrix. The work is based on techniques which are now well established (for instance, by the first author) and that have proven to be the most valuable for investigation of the microstructures of finely devised materials as clays. Similar approaches have been successfully applied by some of the authors to investigate porosity evolution and the mechanisms of damage in experimentally and naturally deformed clayey rocks. But, the deformed samples are always investigated at post mortem conditions. Therefore, the experimentalists do not always have access to the history of loading and only the final stage at failure provides guidance for the choice of the investigation areas. The improvement proposed in the present work is to select the investigated areas based on the in situ monitoring of deformation in samples deformed in previous studies, using digital image correlation, allowing for determining the full strain fields. The latter technique allows for instance to find out the localization of strain and damage in the samples during the loading process and therefore to seek the corresponding microstructures in the appropriate areas. I don't have any major problem with the philosophy of the experimental approach, nor with the organization of the paper and I recommend its publication. I have however few comments that follow: lines 69-75: Some rephrasing for clarity and paying attention to the tense may be needed. It should be clearly stated the different types of geomaterials (in addition to salt and clay-rocks, carbonates sould also be mentioned) which were tested and the type of in situ observation techniques (optical, SEM and X-ray tomography). Line 97: Yang et al (2012) used optical microscopy (not SEM). Line 113: I never heard about "crystal plasticity" of clays and think it is not appropriate to speak so. "Crystal plasticity" term may be misleading as it usually stands for crystal slip (dislocation glide) in massive crystalline materials, which is clearly not the structure of clay. Besides, it may suggest that something is already known about the "plasticity" mechanisms of clay particles, which is also not the case. Something is also mistaken in the phrasing : ". . . crystal plasticity of clay, a the poorly known plasticity of nano-clay aggregates. . .".* Line 164: check the figure, there is a mistake in the captions/ labelling of fig.2: it is written "maximum shear stress field", but DIC cannot measure stresses!

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only strain! Also Fig. 3 repeats exactly a part of the synoptic figure 1, which small size makes it very difficult to read. It can probably be expanded and Fig.3 to be referred to this new Fig. 1, or something this way.

Similarly, Fig. 4 repeats the 3D strain field of the cylindrical sample already reported in Fig. 3. Some optimization in the presentation of these figures in order to avoid repeating several times the same elements would be appreciated. Line 247: Some precisions are needed. You state: "...fractures are not resolved by DIC". Yes, but this is only a question of 1) the resolution of the optical microscopy itself (camera, magnification, pixel size...), 2) the DIC local "strain gage length", or say the length scale of the marking contrasts and the specifically adopted procedure of calculation of strain from the displacement discrete field. Do not leave the reader with the impression that this is a general DIC limitation! Line 255: It must be clearly explained (probably well before this section) that the samples with 3D strain field measurements from Lenoir et al. were deformed in 2008! Since, we don't know how they were stored and preserved over nearly 10 years! This is what you probably call "slow drying", but state it more clearly and provide with more details about the way all the studied samples were stored; Line 290: The two previous DIC investigations can only indicate the local strain (compaction, shear, dilation...) at a given gage length, which is well above the inclusions size. Only your fine scale observations allow interpreting these strain fields in terms of mechanisms at the scale of the damaged inclusions. Anyway, you should recall the DIC "gage lengths". Line 313: "...3D and 2D digital image..." Finally, all my comments need only minor modifications and/or clarifications. All the best. A. Dimanov.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2016-131/se-2016-131-RC1-supplement.pdf

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