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Interactive comment on "Uniaxial compression of calcite single crystals at room temperature: insights into twinning activation and development" *by* Camille Parlangeau et al.

Anonymous Referee #2

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Dear editor,

I read the manuscript submitted for publication in Journal Solid Earth and entitled "Uniaxial compression of calcite single crystals at room temperature: insights into twinning activation and development".

The authors present a new experimental study on the nucleation and development of twinning in single calcite crystals subjected to uniaxial compression stress at room temperature. While the samples are being deformed at constant strain rate, optical and electronical pictures are also taken. The paper builds on the analysis of the mechanical data and pictures produced throughout the deformation of the samples. I enjoyed

reading the paper as it provides interesting new data and associated discussion. It also raises interesting questions. However, I have some comments that the authors may want to consider.

In this study, single crystals were used to get rid of the influence of grain boundaries and size distribution on the development of twinning as highlighted by Newman (1994). This is an interesting idea to me but I am a bit disappointed by the fact that the authors do not discuss this point thoroughly in the manuscript. A question I still have is how can these results be extrapolated to natural conditions in which grain size distribution is obviously much more complex? Starting with the simplest situation seems to be an interesting idea but they could maybe discuss in more details the implications of this (over?-)simplification.

As clearly stated by the authors, twinning activation is mainly dependent on differential stress and grain size. In this study, 4x4x8 mm3 and 3x3x6mm3 single crystals were used, which are much larger than most crystals found in natural rocks. As the authors used two different grain sizes, I thought that they could discuss this influence of grain size on the development of twinning. This point is slightly discussed in section 3.2.4 in which the authors compared their results to previous studies. Why not compare their own different experiments first? Side question: their results seem to compare to previous results obtained by Turner et al (1954) on unconfined samples: Do the authors think that confinement might play a role on the development of twinning, contrary to what is claimed in the introduction? One idea might be that twinning is actually slightly associated with microcracking which is highly dependent on confining pressure.

The results obtained in this study would allow the authors to draw the evolution of total twin thickness as a function of axial strain. Did they have a look at this correlation? It may help them decipher whether twinning is associated with microcracking or not (and even maybe try to quantify strain due to each of these two micromechanisms if they are associated).

Interestingly, the authors mention that the duration of stress application has a great impact on twin lamellae thickness (line 30, page 6). To me, this may imply that making a creep experiment would be of interest, also since natural conditions may be closer to constant stress conditions rather than constant strain rate deformation.

Finally, stress-strain curves show multiple small stress drops. Are these stress drops associated with microcracking or twin nucleation or not?

As a conclusion, these experimental results seem to be interesting to me but I think that the authors could go deeper in-depth in their conclusions, maybe after performing a few additional experiments.

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