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Interactive comment on "Frictional properties and microstructural evolution of dry and wet calcite-dolomite gouges" *by* Matteo Demurtas et al.

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General comments

This is my first review of this manuscript. I note that the manuscript has already received publicly available comments from another reviewer, however I chose not to read these comments so that my own thinking on the manuscript was not influenced. I therefore apologize to the authors if I end up repeating comments that have already been made by reviewer 1.

This manuscript investigates the frictional behaviour and microstructural evolution of

mixed calcite-dolomite gouges over a range of slip rates (30 μm/s - 1 ms) under different water saturation states. As the slip velocity is increased towards coseismic rates the authors document the onset of dynamic weakening and the formation of a defined principal slip zone within the gouge. In room-humidity experiments, at slip velocities >0.1 m/s, they also observe the formation of well-developed foliation in the gouge, something which is commonly observed in natural fault gouges and thought to form by dissolution-precipitation reactions during aseismic creep. The results presented here, where foliations form at coseismic slip rates, suggest that caution should be taken when interpreting the slip history of natural carbonate-bearing faults as similar structures might form over a range of slip velocities.

The manuscript is well written and the results are logically explained. I have made some specific comments below that the authors should address, but this is a useful contribution to the field and I have no issue in recommending it for publication in Solid Earth.

John Bedford (04/12/20)

Specific comments

-l. 29-31: Could the authors be more specific about what they consider to be "low strain rates, high temperatures and high pressures" on line 29, and also "high strain rates, low temperatures and low pressures" on line 31.

-l. 88: Why did the two batches of gouge have different weight percentages?

-l. 106: Is there any reason why a normal stress of 17.5 MPa was chosen for this study? Also why was one test (s1324) ran at a normal stress of 26 MPa? The main goal of the manuscript appears to be to investigate the role of slip rate, displacement and the presence of water on the frictional behaviour and microstructural evolution of calcite-dolomite gouges, therefore it would be sensible to use the same normal stress for all tests in the study. Looking ahead to Figure 2, there is a possible normal stress dependence on the frictional response (the 26 MPa sample experiences a bit more weakening than the equivalent 17.5 MPa sample), however more than one test under different normal stresses would be required to constrain this relationship. It therefore seems a bit strange to include this test in the manuscript, at least without some further justification in the main text.

-l. 152: It is interesting that adding water makes the gouge compaction slip-rate independent. Do the authors have any explanation for this? Has it been reported in any previous studies?

-l. 162-164 and Fig. 5b: I'm not sure I fully understand this data. It's fine that the CO2 data are qualitative but why are they plotted against time in figure 5b – what is this time relative to? Also why are the CO2 peaks for the fastest experiments (1 m/s) later in time than the slower experiments (0.1 m/s)? I would intuitively expect any thermal decomposition and CO2 release to occur more quickly at faster slip rates.

-l. 174-179: It seems a bit unusual to me that authors include this text, and also present Figures 7 and 8, prior to their detailed microstructural descriptions (and associated figures: 9, 10 & 11) in the following subsections. The authors provide very detailed descriptions of their microstructures in sections 3.5.1 and 3.5.2, with the associated images being presented in figures 9-11. In my opinion it would make more sense to summarize these microstructures and how they differ with slip rate and water saturation (i.e. as shown in Fig. 7) after the detailed descriptions have been presented. In this way the summary figure will "wrap up" the detailed information presented in Figs. 9-11. Perhaps the authors would consider reordering the figures and text slightly?

-l. 224: What is this characteristic wavelength?

-l. 249: I can't see this initial period of dilatancy. Does it occur at the very start of the experiment, at less than 0.01 m of slip? If so it would be good to include an inset in Fig.4 to show this, similar to panels b and d in figure 2.

-l. 286: What temperature does dolomite begin to decompose? This should give a minimum constraint on the temperature rise that occurred in the experiments.

-l. 332-337: Could this discrepancy and low measured temperature rise be a consequence of thermal buffering caused by decomposition of dolomite? As decomposition reactions are generally endothermic they can limit the coseismic temperatures increase, as has been shown for decarbonation reactions (Sulem & Famin, 2009) and dehydration reactions (Brantut et al., 2011) .

Technical corrections

-l. 33: This should read "frictional behaviour of dolomite IS relatively poorly understood".

-l. 228: This should read "with displacements of. . ."

Table 1: Experiment s1324 is listed at a normal stress of 17.4 MPa, but I think this is a typo and should be 26 MPa instead.

References:

Brantut, N., Han, R., Shimamoto, T., Findling, N., & Schubnel, A. (2011). Fast slip with inhibited temperature rise due to mineral dehydration: Evidence from experiments on gypsum. Geology, 39(1), 59–62. https://doi.org/10.1130/g31424.1

Sulem, J., & Famin, V. (2009). Thermal decomposition of carbonates in fault zones: Slip-weakening and temperature-limiting effects. Journal of Geophysical Research: Solid Earth, 114(B3), B03309. https://doi.org/10.1029/2008JB006004

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