Here below the Reviewer2 (R2) comments are addressed point by point by the authors (**A**).

[R2] -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.

[A] We specified the ranges of strain/shear rates, temperature and pressure for the cases listed.

[R2] -l. 88: Why did the two batches of gouge have different weight percentages?

[A] The two batches of calcite-dolomite gouge were made my weighting the equal amount of calcite and dolomite and then mixing it together. The difference in the calcite-dolomite ratio between the two batches could be due to little manual mixing of the gouge before the mineralogical analysis, or sampling of a slightly dolomite-richer portion.

[R2] -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.

[A] Since one of the aims of our work was to further explore the microstructural evolution under different deformation conditions starting from the work of Smith et al. (2017), we chose to perform the experiments at a very similar normal stress (17.3 in Smith et al., 2017 vs. 17.5 in our study) in order to minimize the change of variables.

We agree with the reviewer and since only one experiment was carried out at 26 MPa, we decided it exclude it from the dataset and focus mainly on the influence of slip rate, displacement and presence of water.

[R2] -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?

[A] As pointed out by Reviewer1, the creation of a very fine-grained (<<1 μm) slip zone in all water-dampened experiments which varies only in thickness with increasing slip rate, facilitates and promotes compaction during deformation. Although this can explain the compaction in the presence of water, it still poses a question of why same compaction history seems to be independent from slip rate, question to which we currently have no answer to.

Rempe et al. (2017, JSG; 2020, JGR) performed rotary shear experiments at slip rates of 0.001 ms^{-1} and 1 ms^{-1} on calcite gouges in both water-dampened conditions (using the same gouge holder as us) and water-saturated conditions with either fluid pressure- or volume-control. Their axial displacement data show that for the range of displacements investigated in our study (<0.4 m) the gouge thickness evolution appears to be very similar in the presence of water and independent of the drainage conditions. At displacements >0.5 m (which is past the displacement explored in our work, see e.g., Figure 4e in Rempe et al., 2020), dilation occurs in undrained experiments and is interpreted as pore fluid pressurization due to temperature increase in the principal slip zone during dynamic weakening.

[R2] -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.

[A] The gas emissions were recorded with a separate control system from the rotary shear apparatus. This means that the measuring had to be started manually and that makes it unable for us to constrain gas emission data with those collected with SHIVA (e.g., friction, slip, T). Therefore, data are plotted in time as they were recorded. The shift in time of the CO_2 peaks is due to the fact the measurements were started at different times (few seconds of difference) from the start of the experiments. We added a few lines in the main text to clarify this point.

"Since the acquisition system for gas emissions was separated and not synchronized with that of SHIVA, CO_2 emissions are plotted against time. Shifts in time of the peaks from different experiments is due to changes in the manual start of the data collection."

[R2] -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?

[A] Following both reviewers comments we rearranged the Figures, pushing the old Figure 7 to now Figure 11. We also removed the text that was present at the beginning of section 3.5.

[R2] -l. 224: What is this characteristic wavelength?

[A] We added the wavelength (i.e., c. 300 μm) of the boundary between the two microstructural domains in the main text.

[R2] -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.

[A] We added an inset in Fig. 4a showing the initial dilatancy for experiment *s1221* performed at 1 ms^{-1} and modified the figure caption.

[R2] -l. 286: What temperature does dolomite begin to decompose? This should give a minimum constraint on the temperature rise that occurred in the experiments.

[A] The temperature for the start of dolomite decarbonation is c. 550 °C and was presented in the Results section when describing the variation in mineralogy of the gouges. We added it again in the Discussion.

[R2] -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) .

[A] CO_2 emissions were detected under both room-humidity and water-dampened conditions. Therefore, a possible buffer effect of the decomposition reaction would likely not result in striking differences between the two cases. However, as noted by Reviewer 1, the occurrence of water could be an alternative, or partial, explanation of the lower temperatures observed due to a buffer effect during vaporization. We have added this part into the discussion section.

[R2] -l. 33: This should read “frictional behaviour of dolomite IS relatively poorly under- stood”.

[A] Corrected.

[R2] -l. 228: This should read “with displacements of. . .”

[A] Corrected.

[R2] 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.

[A] Corrected.