Reply to Anonymous Referee #2's comments

Dear referee,

We appreciate your careful review and comments on this manuscript. We took all your comments into consideration as follows. Our replies to your comments are listed below.

Sincerely,
Hanaya Okuda, Ikuo Katayama, Hiroshi Sakuma, Kenji Kawai

Comment 1

This manuscript presents results from friction experiments on Brucite gouge under a range of normal stress and saturation conditions. The results indicate that wet Brucite exhibits a low base friction coefficient and velocity-weakening frictional behavior under low normal stresses. The authors also present microstructural analyses to supplement their experimental results. They use these observations to suggest that Brucite could play an important role in hosting slow earthquakes in a hydrated mantle wedge.

The results are new, interesting and definitely merit publication. However, the authors make multiple strong statements that are not adequately backed up with evidence. Additionally, the manuscript in general and the introduction in particular relies heavily on slow earthquakes in the mantle wedge, without appropriately acknowledging deep slow earthquakes in the plate interface. Finally, the text has multiple grammar and writing errors which need to be corrected prior to publication.

Overall, I believe the manuscript must undergo some moderate/major changes prior to publication. I enumerate my recommendations by line/section below:

1. Abstract line 25-27: "Brucite …. Mantle wedge". This is one example of a strong statement that, in my opinion, is not adequately backed up by the results. This could be one explanation, yes. But to say that this is the 'only mineral' and explains the occurrence of slow earthquakes is bold.

Reply to Comment 1

Thank you for the comment. As your comment and MC1 from referee #1 which both highlighted that the manuscript lacked appropriate acknowledgement on deep slow earthquakes, we added explanations of slow earthquakes at mantle wedge depth in the first paragraph. Please refer to the replies to MC1 from referee #1 and Comment 6 from referee #2. We modified some parts of our manuscript to tone them down, For example, regarding the sentence "Brucite … mantle wedge.", according to the comment and DC5 from referee #1, we edited the sentence (lines 26–28 in the marked-up manuscript) to tone it down as follows:

(Before) Brucite is found to be the only mineral that has a low friction coefficient and exhibits unstable frictional behavior under hydrated mantle wedge conditions, explaining the occurrence of slow earthquakes in the mantle wedge.

(After) Among serpentinite-related minerals, weak and unstable frictional behavior of brucite under the hydrated mantle wedge conditions may play a role in slow earthquakes at the subduction plate interface in the mantle wedge.

Comment 2

2. Lines 44-49: This is a long sentence that should be broken down for readability.

Reply to Comment 2

We modified the sentence (lines 89–93 in the marked-up manuscript) as follows:

(Before) Despite a variety of previous experimental investigations of the frictional properties of antigorite and talc (Hirauchi et al., 2013; Moore et al., 1997; Moore and Lockner, 2007, 2008; Okazaki and Katayama, 2015; Reinen et al., 1994; Sánchez-Roa et al., 2017; Takahashi et al., 2007; Tesei et al., 2018), brucite has rarely been considered in previous studies compared with other serpentinite-related minerals, which might be due to the fact that it is difficult to detect brucite under natural conditions because of its fine-grained nature (Hostetler et al., 1966).

(After) Many previous experimental studies investigated the frictional properties of antigorite and talc (Hirauchi et al., 2013; Moore et al., 1997; Moore and Lockner, 2007, 2008; Okazaki and Katayama, 2015; Reinen et al., 1994; Sánchez-Roa et al., 2017; Takahashi et al., 2007; Tesei et al., 2018). However, brucite has rarely been considered in previous studies as it is challenging to detect brucite under natural conditions because of its fine-grained nature (Hostetler et al., 1966).

Comment 3

3. Lines 54-55: "Because Brucite is stable….mantle wedges." I understand that 'stable' here does not refer to frictional stability. However, because it is being used in the context of sliding friction, this makes for a very confusing sentence. I recommend some alternate wording to indicate the presence/detectability of Brucite.

Reply to Comment 3

We would use "brucite stably exists" or "thermodynamically stable" instead of just saying "brucite is stable" to avoid confusion. We also modified Discussion as well as Introduction.

Comment 4

4. Lines 56 – 62: "Only a few…temperature". This is a good introduction describing the friction of Brucite. However, it lacks any information about the stress state of these experiments in literature. Since, this manuscript is about the role of normal stress, it would be useful to mention the stresses used for these expts.

Reply to Comment 4

Per this comment, we added the stresses used in previous studies (line 120 in the marked-up manuscript) as follows:

(Before) Only a few previous experimental studies have been conducted on the frictional properties of brucite.

(After) Only a few previous experimental studies under high normal stress conditions of 100 or 150 MPa have been conducted on the frictional properties of brucite.

Comment 5

5. Lines 62-65: Another sentence that could be broken down to enhance readability.

Reply to Comment 5

According to this comment, we modified the sentence (lines 127–130 in the marked-up manuscript) as follows:

(Before) Because the friction coefficient of the serpentinite gouge can be lowered by approximately ~10–15 % due to the presence of brucite (Moore et al., 2001) in addition to velocity-weakening behavior of brucite under certain conditions, the frictional characteristics of brucite might affect earthquake nucleation processes at mantle wedges.

(After) The friction coefficient of a serpentinite gouge can be lowered by approximately ~10–15 % due to the presence of brucite (Moore et al., 2001). The weakness and velocity-weakening behavior of brucite under certain conditions might affect nucleation processes of slow earthquakes at the subduction plate interface in the mantle wedges

Comment 6

6. Lines 65 – 69: "Although….mantle wedges" These statements are made matter-of-factly but the *links between them are not clear to me and don't follow very easily. I don't understand why brucite is key to understanding slow earthquakes just because there have been observations of slow earthquakes at mantle wedge depths. This might also be a good spot to introduce slow earthquakes as a transitional phenomenon between dynamic seismicity and stable creep.*

Reply to Comment 6

Thank you for your comment, with which we agree. This part was moved to the first paragraph and modified according to referee #1's comment (MC1 in referee #1's comments; lines 38–51 in the marked-up manuscript). In the modified part, we first mentioned the classical opinion that the serpentinite in the mantle wedge might contribute to the aseismic behavior below the seismogenic zone's downdip limit. Then, we explained the occurrence of slow earthquakes in the mantle wedge, whose mechanisms are still debated but possibly related to the serpentinite layer. Thus, we focused on the frictional properties of serpentinite. These sentences would be followed by reviewing low effective normal stress conditions at the subduction plate interface, and previous experimental studies on serpentinite and brucite in the following paragraphs. After introducing the frictional behavior of brucite, we stated the importance of brucite for the slow earthquakes in the mantle wedge.

In addition to the modification suggested by referee #1 shown above, we added some sentences about the slow earthquakes according to this comment (referee #2 comment 6) in lines 131–138 in the marked-up manuscript.

Comment 7

7. Lines 70 -85: This paragraph is probably better suited near the introduction to the frictional behavior of Brucite. Right now, the authors talk about Brucite friction, then about the mantle wedge, then back to Brucite friction and normal stress.

Reply to Comment 7

According to the comment, we moved a part of this paragraph into the third paragraph (lines 68–79 in the marked-up manuscript) before the paragraph introduces previous studies on brucite friction.

Comment 8

8. Lines 83-85: Again, this role of Brucite is being boldly overstated at this point as the only conceivable possibility.

Reply to Comment 8

According to this comment and the comment DC18 by referee #1, we removed this sentence. Based on this study, we think that brucite may play a key role in the nucleation of slow earthquakes in the mantle wedge. In the Introduction, however, readers cannot access enough information to reach this conclusion; therefore, we removed the sentence from the Introduction.

Comment 9

9. Line 90: Is WAKO a company?

Reply to Comment 9

Yes, WAKO (FUJIFILM Wako Pure Chemical Corporation) is a Japanese company for chemical materials. We modified the text (lines 168–171 in the marked-up manuscript) as follows:

(Before) WAKO

(After) FUJIFILM Wako Pure Chemical Corporation

Comment 10

10. Line 94: What is the thickness of these gouge layers? Also, was there any mechanism to prevent the gouge 'paste' from squeezing out of the sides in the wet experiments?

Reply to Comment 10

At the steady state, gouge thickness was 400 and 150 μm for dry and wet experiments, respectively. As reduction of gouge thickness before the steady state was about 150 and 200 μm for dry and wet experiments, respectively (Figs. 5 & 6), initial gouge thickness was roughly 550 and 350 μm, respectively. We did not have any mechanism to prevent the gouge from squeezing out for the wet experiments; therefore, the gouge thickness for wet experiments is narrower than that for dry ones. We added a sentence explaining we did not have any mechanism in the Method section at lines 190–192 in the marked-up manuscript. We also stated this in the Result (lines 388–389 in the marked-up manuscript).

Comment 11

11. Line 100: How did you ensure/measure saturation? Presumably mixing the brucite with distilled water alone simply ensures that the gouge is wet, not that it is saturated and all the pore volume is filled with water.

Reply to Comment 11

Because we did not place the system in the vacuum condition, it is not easy to ensure the saturated condition in our study. Therefore, we should use "water-wet condition" instead of "water-saturated” condition. We modified the text as follows (line 190 in the marked-up manuscript):

(Before) water-saturated conditions

(After) water-wet conditions

Please also refer to the DC22 from referee #1.

Comment 12

12. Lines 125-126: Are these cohesion values from literature? If so, please cite your sources.

Reply to Comment 12

The cohesion values were obtained in this study; therefore, we modified the sentences of lines 208–210 in the marked-up manuscript as follows:

(Before) Note that cohesion was not considered because the cohesion stresses were 0.36 and 0.47 MPa for the dry and wet cases, respectively, which are much smaller than the tested normal stress conditions.

(After) Note that cohesion stresses were 0.36 and 0.47 MPa for dry and wet cases, respectively, calculated by linear regression of shear stress and normal stress of all the experiments. Because the obtained cohesion stresses were too small to affect the friction coefficients, the cohesion stress was not considered in this study.

Please note that this modification includes the DC24 by referee #1’s comment.

Comment 13

13. Lines 149-150: Not sure if I agree with this entirely. Yes, within the framework of the aging law in sliding of rough interfaces, the dc reflects asperity diameter in some fashion. However, in the context of gouge experiments with a given porosity, one may also consider the alternative interpretation of dc as the width of a localized shear zone (Marone & Kilgore, 1993) as an appealing definition.

Reply to Comment 13

Thank you for the suggestion. We agree with the comment that d_c may be related to the shear localization as proposed by Marone and Kilgore (1993). Based on our microstructural analysis, thickness of the shear band was 10 and 20 μm for the dry and wet experiments, respectively, indicating that the degree of shear localization for the dry experiment was higher than that for the wet experiment. Although we are not sure whether this difference in shear band thickness can quantitatively explain the difference in dc between dry and wet experiments, d_c values for wet experiments were larger than those for dry experiments, consistent with the relationship between the shear localization and d_c. Therefore, we added some sentences about the relationship between d_c value and shear localization in the Method section (lines 245–252 in the marked-up manuscript), and also added some sentences about the difference between dry and wet experiments in the Results section (lines 422–426 in the marked-up manuscript).

Comment 14

14. Lines 153-154: I understand the point the authors are trying to convey here, but this statement is very strong. Yes, there is an ongoing debate as to the choice of a 'right' constitutive law. However, stating that the value of a – b is 'more' critical needs additional evidence or citations. One might argue that the roles of dc, stiffness k etc. are just as important.

Reply to Comment 14

We agree with this comment. Further, referee #1 left a comment about this sentence (DC31). We would like to use $a - b$ because $a - b$ value is the essential component to control earthquake nucleation processes, but our sentence does not appropriately describe our aim. Therefore, we modified the text (lines 247–252 in the marked-up manuscript) as follows:

(Before) Although there are still debates on the choice of constitutive laws (Bhattacharya et al., 2015, 2017; Marone, 1998), the value of $a - b$ is more critical for seismic activities.

(After) Although there are still debates on the choice of constitutive laws (Bhattacharya et al., 2015, 2017; Marone, 1998), as all constitutive laws give the same result on $a - b$, we calculated the value of $a - b$ by using separately obtained a and b with the aging law. The focus of this study will be the $a - b$ value because it plays an essential role in the nucleation process of earthquakes. However, other parameters like d_c and stiffness are also important to the nucleation process, and therefore, those parameters should be assessed in future studies.

Comment 15

15. Lines 221-223: "The a-b .... Values." Why does an identical a-b value for up and downsteps necessarily imply that normal stress conditions control this? For a given set of velocities, a-b is independent of the normal stress anyway, so I don't understand this connection.

Reply to Comment 15

We agree with this comment and DC53 by referee #1. We cannot strongly say that the normal stress controls the $a - b$ values; therefore, we deleted the sentence and just stated that the $a - b$ values do not differ between upsteps and downsteps (lines 329–330 in the marked-up manuscript) as follows:

(Before) The $a - b$ values obtained for the upsteps and downsteps insignificantly differ (Figs. 4a and b), which implies that the normal stress condition mainly controls the $a - b$

values.

(After) The $a-b$ values obtained for the upsteps and downsteps insignificantly differ (Figs. 4a and b).

Comment 16

16. Lines 231 – 234: "Although…smaller than b1". Yes, b2 is often smaller than b1 and its effect is small, but is dc2 is also smaller? Often, in a 2 state veriable framework, dc2 >> dc1.

Reply to Comment 16

In this study, we observed that d_{c2} is larger than d_{c1} (Fig. 4) as commented. Because we mainly focus on the $a-b$ value in this study, we did not mention the large d_{c2} value in the original manuscript. However, we would like to note the value of d_{c2} as follows (lines 345–348 in the marked-up manuscript):

(Before) Although the second variables b_2 and d_{c2} were introduced in certain experiments (HTB575 and HTB598), their effects on the frictional characteristics are small because the b_2 values are much smaller than b_1 (Fig. 4e and Table S1).

(After) Note that the second variables b_2 and d_{c2} were introduced in two experiments (HTB575 and HTB598). However, their effects on the earthquake nucleation process, that is, $a-b$ value, are small because the b_2 values are much smaller than b_1, although d_{c2} value is much larger than d_{c1} (Fig. 4e and Table S1).

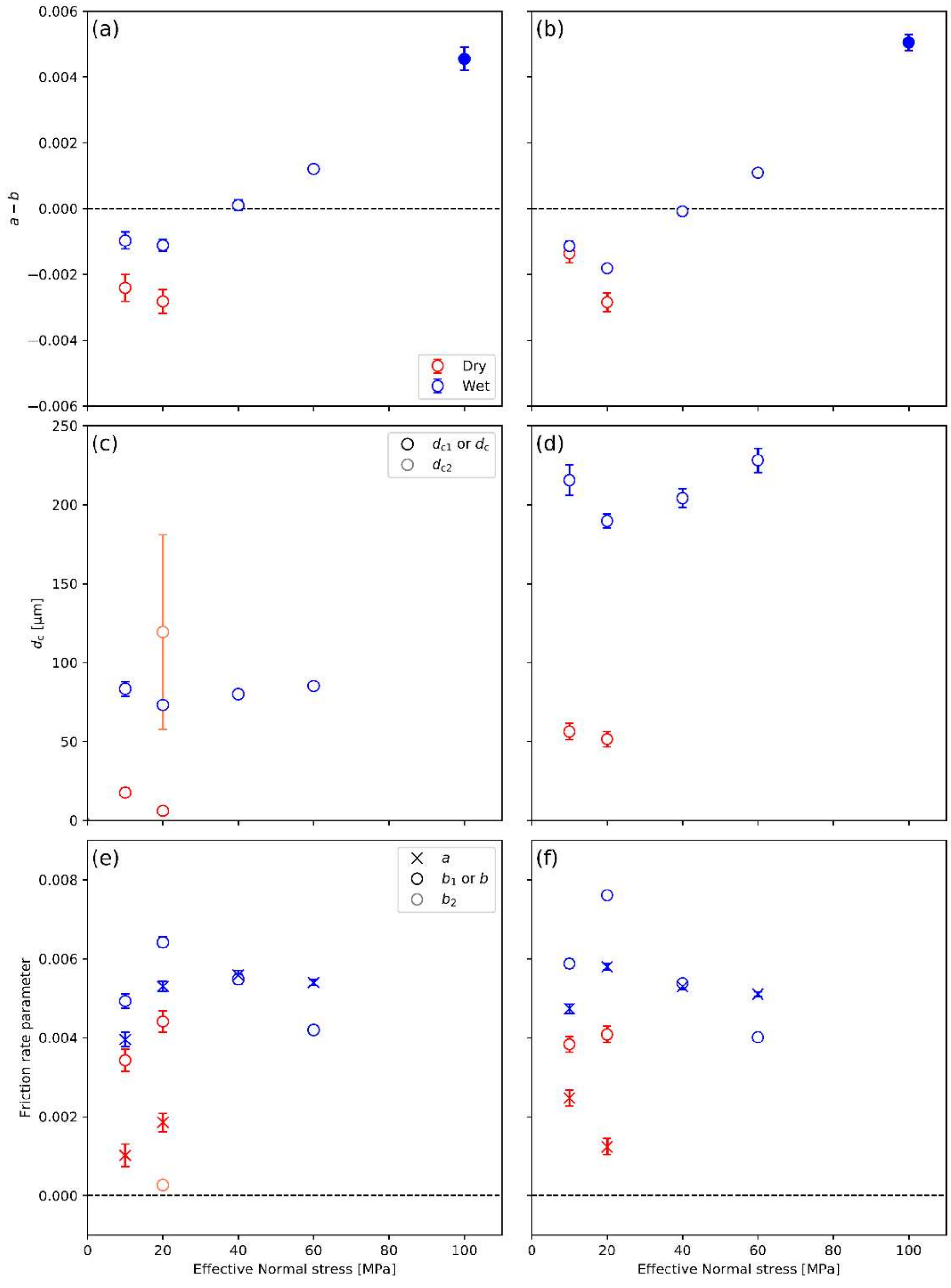
Comment 17

17. Figure 4: Please annotate that column 1 is all upsteps and column 2 is downsteps.

Reply to Comment 17

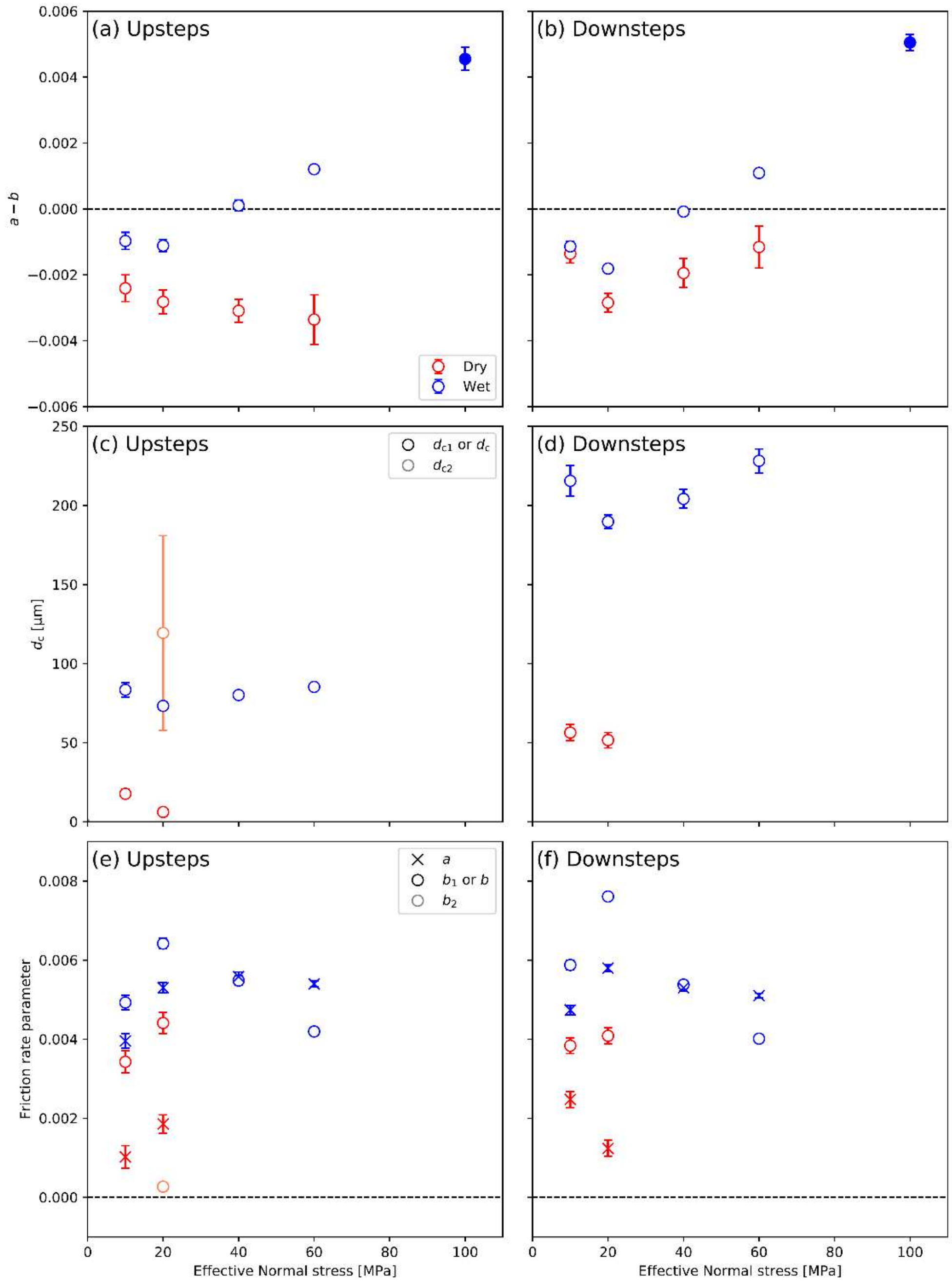
According to this comment and DC56 by referee #1, we added "upsteps" and "downsteps" in figure 4 as follows:

(Before)



(a)
(b)
(c)
(d)
(e)
(f)
a − b
Dry
Wet
d_{c1} or d_c
d_{c2}
d_c [μm]
a
b_1 or b
b_2
Friction rate parameter
Effective Normal stress [MPa]

(After)



Comment 18

18. Lines 257-268: Many of these observations are also consistent with recent works by Kenigsberg et al. (2019, 2020 – JGR) on clay-rich gouges although they document clear Y and P foliations. This may be worth noting.

Reply to Comment 18

Thank you for letting us know about those works. We added Kenigsberg et al. (2019; 2020) in lines 382–383 in the marked-up manuscript.

Comment 19

19. Lines 310-315: Based on your own sketches in Figs. 5-6, it appears that the deformation could well be accommodated, at least in part, by the Riedel shears. Since there appears to be no additional measurements of gouge compaction/dilation, it seems like the interpretive statements on the location and direction of deformation may be overstated. Also, by stating "constant gouge thickness", do you imply that there was no layer thinning observed in your experiments due to mass loss? This is very surprising.

Reply to Comment 19

Based on Figures 5 and 6, we drew arrows along the Riedel shears at the steady state, leading to some ambiguities that the Riedel shears were still active during steady state. However, according to the observations of crystal orientation at the steady state, we found the alignment of the platy particles only along the boundary shear, showing evidence of the contribution of boundary shear to the deformation at the steady state. Notably, we found no crystal orientation along the Riedel shears, supporting that the deformation is likely to localize along the boundary shear. Hence, we removed arrows along the Riedel shears from Figures 5d and 6c, which may have caused readers to infer that the Riedel shears were still active during the steady state. Please also refer to MC3 and DC67 from referee #1.

Regarding the constant gouge thickness, it is hard to quantitatively say that no mass loss had occurred. After some experiments, however, we "opened" the blocks and confirmed that the whole area of the block surface was filled with brucite gouge. Though a qualitative observation, we think this implies that the mass loss during experiments was little and, therefore, the effect on the experimental results may be negligible.

Comment 20

20. Lines 316-319: Not sure I understand this statement. Anthony and Marone (2005) refer to the smoothness of grain boundary contacts whereas I assume here you refer to boundary shears, which are more or less ubiquitous regardless of the smoothness of grain boundary contacts. I don't believe they are directly comparable as you have done here.

Reply to Comment 20

We referred to Anthony & Marone (2005) here because they compared the forcing blocks with smooth and rough surfaces in addition to the rounded and angular particles. In their study, friction coefficients became low when forcing blocks with a smooth surface were used, whereas friction coefficients became high when forcing blocks with a rough surface were used. Although it is not so easy to compare the smooth "block surface" and smooth "boundary shear," we think that smoothness of the boundary shear may play some role in the weakening of the brucite gouge; therefore, we mentioned the work by Anthony & Marone (2005).

Comment 21

21. Section 4.2 : This section is very well thought out and logically organized. I congratulate the authors on this effort. However, it does contain a number of grammar errors which should be thoroughly cleaned up.

Reply to Comment 21

Thank you for your encouraging comment. We appreciate your careful review despite our poor English. The referee #1 also suggested us to improve grammar. After we corrected all suggestions by referee #1, we used an English correction service to improve the manuscript further.