

RESPONSE TO ANONYMOUS REFEREE #3

1. MAIN COMMENTS

The paper is clearly written and most parts can be followed even by those readers, who do not have a background in stick-slip dynamics. To my mind, the paper is near publication quality. Most of my comments are minor as specified below. The main point of criticism concerns a better presentation of the model. Although the spring-sliderblock-cartoon is more than familiar to most people, it will still provide clarification in the current paper if the different elements (e.g. pulling velocity, spring) are labeled according to the Whillans Ice Stream scenario (e.g. GPS velocity, elastic moduli). Since the Whillans stick-slip motion is such peculiar phenomenon, presenting the model this way would help the reader better grasp the essence of the processes, which the authors model.

Good idea. We have added a spring-slider schematic to Figure 2.

2. SPECIFIC COMMENTS

- (1) *At several instances throughout the paper, the authors mention the state evolution distance L . Although this quantity is formally defined on Lines 19-20, what makes it so important? What would be the implications of higher/lower values of L ?*

We have added a discussion of these issues to Section 5.2.

- (2) *Page 5257, Lines 1-5: It may help the reader to know from the beginning that the signals of a single tremor stick-slip source are never observed on more than one station.*

We agree that this is an important point. In trying to rewrite this section, we found it difficult to explain that the signal is not observed on multiple stations before having described the actual signal first. For this reason we have not made this statement in the mentioned paragraph (Page 5257, Lines 1-5).

- (3) *Page 5258, Lines 9-10: "Recursion halts when the time between peaks in the remaining time series approaches 10 s." is not clear to me.*

Because of noise in the data, simply using all peaks results in an under-estimate of amplitudes. To avoid this, we make a vector of peaks, and then calculate a second vector by applying the peak finder to this vector of peaks. This is done repeatedly until the spacing between peaks is about 10s. We have modified the text to reflect this point.

- (4) *Page 5259, Lines 14-15: “This loading occurs within the ice column which causes most motion during large-scale slip events to occur in the ice rather than in the earth.” This seems to contradict Figure 2 suggesting that the till side is the more compliant material on the bimaterial stick-slip fault planes.*
The net motion after many seismic cycles consists of a translation of the ice, with no net motion of the till.
- (5) *Page 5263, last line: A references seems necessary here.*
We have added an appropriate reference.
- (6) *Page 5264: If I understand correctly, then the definition in Equation 16 is motivated from Equation 14. It would help to comment on this.*
Yes, Eq.(16) is motivated from (14). We have commented on this in the manuscript.
- (7) *Page 5265: Why is there no reference for Equations 17 and 18?*
We have added an appropriate reference.
- (8) *Page 5266, Line 23: What are the “elastic components” and the “strength term”?*
We have clarified the terms to which we refer.
- (9) *Page 5268, Equation 21: I may have misunderstood something, but I am getting an extra R^2 when trying to reproduce this equation.*
Good catch! This equation should have G_* in place of k . We fixed this typo and verified that it didn’t propagate into any other equations.
- (10) *Page 5269, Line 4: Specify that D is measured with GPS.*
We have clarified this point.
- (11) *Page 5270, Line 2: Include “ L ” after “state evolution distance”.*
We have added this improvement.
- (12) *Page 5271, Line 27: Explain “coordination number $C = 9$ ”.*
We have clarified this definition.
- (13) *Page 5275, Lines 25-26: “ a stiffening bed implies a shift towards more stable conditions”: can this be shown with the inequality in Equation 19?*
Yes, this can be inferred from (19). We have clarified this point in the text.
- (14) *Page 5275, Lines 27-28: “Independent observations . . .”: Which observations are being referred to? Reference needed?*
Appropriate references were already included, and we have clarified the explanation in the text.

3. FIGURES

- (1) *Figures do not seem to appear in the order they are mentioned.*
The figures now appear in the order in which they are referenced.
- (2) *Figure 1: Symbols and legend font should be larger. Highlight the “third red dot” directly in the figure. Caption: “red dots shows” [to] “red dots show”.*
We have made all of these recommended improvements.
- (3) *Figure 2: This figure should be annotated better: “15 minute duration” of what? What are the red bars in the Panel B pictures? What do the arrows represent? Displacement or velocity? Creep or strain rate?*
We have improved the annotations in the caption of Figure 2.
- (4) *Figure 3: I suggest directly labeling Panels A and B as “Observation” and “Model”.*
Figure 4: The subscript font in Panel A’s y-label is too small.
We have made both of these changes.

RESPONSE TO ANONYMOUS REFEREE #2

1. MAIN COMMENTS

Two comments from the reviewer prompted us to substantially clarify several aspects the Whillans Ice Plain tremor episodes. The first point, mentioned in two related comments, concerns the range of expected parameter values:

Page 5269 Section 6.3: I think more extensive discussion of parameters and their influence on effective pressure is required. Figure 6 is for just one set of values, how much can this line change given “reasonable” values?

Page 5270 equation 24: As in section 6.3, provide a range of estimate for L assuming reasonable parameter values.

This comment prompted us to make several changes:

- All parameters are now calculated in time series and shown in Figure 5.
- We have made histograms of all inferred and observed parameters to show their variability. These are shown in Figures 4 and 5.
- In calculating the full temporal evolution of effective pressure, we found that our previous estimate of effective pressure was too large by a factor of 3 to 10.
- We have also added additional discussion in Section 6.3 that clearly describes how we arrive at a range of possible bed shear wave speeds.

The second major criticism addressed Section 7, which concerns the variation of seismic particle velocity amplitudes between single- and double-wait time events:

Page 5271 Section 7. In my opinion, this section could be left out of the paper since it doesn't attempt to explore a larger dataset. While the observation of variation in G with wait time is intriguing, the double wait time events on 1-19 and 1-26 show no such behavior as the double wait-time event on 1-14.

This criticism motivated several changes:

- We revisited the data and established a more firm observational basis for the attributes of double wait time events. The result is found in updates to Figure 4, and Figure 5, which now show histograms of the inferred and observed parameters.
- We have included discussion of the statistical significance of differences between double and single wait time events. We find that there is a statistically significant difference in tremor episode seismic particle velocity amplitudes and that this difference is not significantly related to inferred slip per event.
- We have re-written the beginning of Section 7 to more be more concise.

2. OTHER COMMENTS

- (1) *Page 5256 Line 1: "Low" is a relative term, can the authors provide a reference for comparison.*
We have changed the wording of this paragraph.
- (2) *Page 5256 Line 25: Only low-tide events are skipped*
We have rephrased this paragraph and included this point.
- (3) *Page 5257 Line 8: I'm not sure the phrase "nearly every event" is useful, can the authors be more exact?*
We have clarified this language.
- (4) *Page 5258 Line 27: Provide a brief statement comparing to WIS ice-stream scale stick-slip where it has been shown double wait time events have been shown to slip further.*
We have added an appropriate reference to clarify this point.
- (5) *Page 5259 Line 19: Provide reference*
We have added referred the reader to a later section where this phenomenon is discussed in greater detail.
- (6) *Page 5260 Line 6: I think there should be some statement here such as "...assuming all motion occurs during stick-slip events".*
We have added these exact words to the manuscript.
- (7) *Page 5261 Line 1: "...Q for ice..."*
We have added these exact words to the manuscript.
- (8) *Page 5261 Line 6: Why 315 for Q when above you say the range is 400-1000?*
The idea is that $Q = 315$ is the value at which attenuation would become important. We have clarified this point.
- (9) *Page 5263 Line 4: Provide a reference.*
We have added an appropriate reference.
- (10) *Page 5263 Line 10: Or for a constant rupture velocity seismic amplitudes are only dependent on slip.*
We have updated the text.
- (11) *Page 5265 Section 5.2: There should be a reference here on rate-state friction, perhaps to a Dieterich paper?*

We have added a reference before Eq. (17).

- (12) *Pager 5265 Line: Equation 18. To avoid confusion, should μ be used instead of f for friction since f is already used in equation 3?*

We agree and have made this change.

- (13) *Page 5268 Last Paragraph. This section should be expanded to explain in more detail to relationship between G and density and shear-wave speed, since there is not a unique relationship between the two.*

We have expanded this paragraph with more detail describing how the range of bed shear wave speeds was derived.

- (14) *Page 5270 Section 6.4: Should this section come before 6.3 since it is needed in the estimates of effective pressure (equation 19)?*

We prefer to keep the current ordering because most readers will be more familiar with effective pressure than state evolution distance.

- (15) *Line 5271 Line 3: "...they have similar average slip per event..."*

We have added these exact words to the manuscript.

- (16) *Line 5271 Line 14: I think 14 MPa and 18 MPa are reversed.*

Yes, these were reversed and we have made the appropriate correction.

3. FIGURES AND TABLES

- (1) *Table1: What is the bed shear wave speed?*

We have not included G_b in this table because it is estimated and not held fixed.

- (2) *Figure 1: The stations need to be labeled!*

We have labeled station BB09 since it is the only station from which we plot seismic data. We feel that labeling all other stations in the figure would not significantly enhance the figure in the context of the manuscript.

- (3) *Figure 4: This is the fundamental not interevent frequency.*

We have changed this language for consistency, although we note that the fundamental frequency generally has the interpretation as the interevent frequency.

- (4) *Figure 4 and 5: Would these figures be better combined with a 5 panels in one column? This would make it easier to directly compare the different panels.*

We have substantially altered the layout of these two figures.

RESPONSE TO ANONYMOUS REFEREE #1

1. SOURCE RADIATION PATTERN

The calculation of seismic amplitudes in this paper relies on the assumption that the seismometer is situated vertically above the seismic source. In that case there is no P- wave radiation and S-waves only contribute to the signal. However, I find it very difficult to imagine that the observed wave field should consist mainly of this contribution. As tremor is widespread as stated by the authors and observed at many seismic stations, it should be unlikely that the seismometer sits in any case directly above the source. If the seismic source was only 800 m laterally away, S-wave radiation would be zero and the seismic signal should be dominated by P-waves. Known glacier thickness compared with P-S travel time differences can in fact better constrain the position of the seismic source with respect to the seismometer. I would therefore recommend to additionally show one of the seismic signals where separate P and S-waves can be seen. This helps to validate the assumption made in your calculation.

P-S times have been analyzed by been done for this dataset by Winberry et al. (2013) in their Figure 3. They find P-S times $\sim 0.3s$. When $\nu = 0.33$ as is the case for ice, $c_p = 2c_s$. The observed P-S time therefore suggests an epicentral distance of 1200 m. At this epicentral distance, the reviewer is correct: P-waves should dominate in the seismogram instead of S-waves. If basal ice is more elastically compliant then this number could be more like 900 m.

Uncertainty in the epicentral distance and p- versus s-wave arrivals manifests itself in two ways. First, the uncertainty in assuming an incorrect epicentral distance will result in an error that is mapped directly into our estimate of the bed shear modulus. From Eq. (20) of our manuscript, we estimate that $G \approx 21.5 \pm 6.0$ MPa. If we instead take the epicentral distance to be 1200 m, then our estimate instead changes to $G \approx 27.9 \pm 7.8$ MPa. If the waves are assumed to be shear waves, then this difference corresponds to a difference of wave speeds of only 15 m/s or about 10%.

A more significant source of error is the error associated with potentially confusing P- and S-waves. Till has a large difference in P- and S- wave speeds. Unfortunately, it is not clear how to correct for this given the data that are available. Because it is not clear where the seismometers lay in the focal sphere it is possible that the stations are nodal for either p- or s-waves.

Given the data available, we do not think it is appropriate to fully simulate the propagation of P- and S-waves. We have modified the text of the paper (after Eq. 6) to reflect

this understanding and point out possible bias in our source parameter estimates that arise from our assumptions.

2. SOURCE DIMENSIONS

The described source process should be ubiquitous at the glacier bed or at least close to asperities. I assume that these asperities have larger dimensions than the calculated fault size of a few meters. How do signals from a larger area contribute to the seismic signal observed at one station and how may this influence the signal amplitude and shape?

As noted by Winberry et al. (2013), stations sometimes show multiple families of gliding spectral peaks, suggesting that more than one tremor patch is contributing to the overall tremor signal. Modeling multiple, possibly interacting tremor patches is beyond the scope of this paper. Furthermore, the events that we analyze in detail have seismograms/spectra (e.g., Fig. 3) that are dominated by one tremor source.

Assuming an asperity of the order of a few tens to one hundred meters, the observed seismic pulses may result from the superposition of P-and S-waves radiated from that area.

This is correct; see discussion above regarding P and S waves.

In Fig. 3A, there are several gliding frequency bands visible that must stem from a different source that produces different overtones gliding differently. How similar are tremor signals at the different stations. Can their variety be explained in terms of the model proposed?

These signals appear to be the superposition of another tremor patch. In other data (not shown) these tremor bands appear as low as 1-2 Hz. Multiple clear spectral peaks are seldom clearly visible for this source. Given the relationship $D = V_s/f_0$ of Eq. (3), these likely have slip as great as 1 mm. We have noted the existence of this other source at the end of Section 3 of the manuscript.

3. SEISMIC AMPLITUDES

For calculation of maximum amplitudes of the tremor over time, you recursively find the highest amplitude peak in a 10 s window, meaning that you take the highest amplitude of one in a hundred peaks given a recurrence period of 0.1 s. From the seismogram example it seems that there is also amplitude variability of the order of 30% within an individual tremor sequence. How would you account for this variability as compared to the 30% larger amplitudes observed for double wait time events? It is unlikely that material properties or aseismic behaviour change at short time scales so there should be a different process that affects amplitudes. If you averaged the maximum amplitudes of all peaks in a tremor sequence (instead of taking the envelope), would the double wait time events still produce larger average amplitudes? That would strengthen your observation and rule out

that there is larger amplitude variability within the tremor signal. The observation that these double wait time events produce larger seismic signals is very intriguing and therefore it would be great to expand on the description of this phenomenon.

This criticism inspired us to experiment with a different amplitude metric to verify that our amplitude measurements were not biased by our recursive peak finding method. The results indicate that even for a very simple measure of amplitude, the median of the absolute value of the trace, there is still a distinct difference in amplitudes between single and double wait time events. We describe this in the last paragraph before Section 7.1.

4. TECHNICAL COMMENTS

The abstract contains a few very technical expressions that make it difficult to understand for non-specialist readers. Examples are “state evolution distance” or “tremor seismic particle velocity amplitudes”.

We agree that “state evolution distance” is a rather technical term, but one that is quite important and interesting to those studying friction. Because of its importance, and the lack of an easy way to explain it within the space limitations of the abstract, we have chosen not to modify how we use the term in the abstract.

With regard to “tremor seismic particle velocity,” we removed that term from the abstract and instead now describe this as the tremor amplitude as recorded by seismometers. We retain the more precise terminology in the text, where it is clearly defined.

The seismic signal is described as being tidally induced, occurring twice a day at low or high tide. If both high and low tide can cause the signal, there should be four tremor episodes per day possible. Could you clarify this? (page 5256).

The tides beneath the Ross Ice Shelf are unusual in that the diurnal component of the tides is significantly more pronounced than the semidiurnal component. We have clarified this point.

The Poisson ratio in equation 13 is assumed to be 0.25 resulting in simplifications. However in Table 1 you use a Poisson ratio of 0.33 for ice and 0.49 for bedrock. How does that affect the validity of equation 13? Or vice versa what would be the consequence of using a Poisson ratio of 0.25 throughout?

In the limiting case where one material is much more rigid than the other, G_* becomes independent of the elastic properties of the more rigid material and $G_* \approx 2G_{\text{compliant}}$ for $\nu = 1/4$. When Poisson’s ratio is chosen to represent ice ($\nu = 0.33$) and till ($\nu = 0.49$), the resulting effective patch shear modulus is $G_* \approx 3.5G_{\text{compliant}}$. We have changed the description surrounding Eq. (13) to reflect these points.

Fig. 1 Fig. 1 is not referred to in the text before Fig. 2.

The figure number ordering has been made consistent.

Fig. 1 shows 4 red dots, not three. It is therefore unclear which station is meant with BB09. Label this station as it is important. For clarity it would be better if all station symbols were coloured according to the sampling rate. The tremor stations could be additionally circled, boxed or otherwise highlighted.

We have made changes to improve the readability of Figure 1.

Fig. 3 A/B Explain the dashed white line in the caption and maybe mention the other gliding frequency bands stemming potentially from a different source.

We have made these changes.