Reviewer 1

Review of "Estimates of earthquake temperature rise…"

This work aggregates estimates of coseismic temperature measurements across several faults, and examines the general trends in the data, such as the relationship between thickness of the principal slip zone, displacement, depth and temperature rise. This analysis finds a general trend of increasing temperature rise with depth up to 4-5 km, and then relatively constant measurements, and no clear trend between the thickness or displacement and temperature rise. The work compares the energy budget of large and small earthquakes, and finds that frictional energy dominates the energy budget of small earthquakes, whereas the three components of the earthquake energy budget are generally similar in magnitude for larger earthquakes. I enjoyed reading this manuscript and suggest it may be published following some minor clarifications.

1) The abstract mentions, and Figure S2 shows that there is not a clear relationship between the PSZ thickness and the temperature rise or maximum temperature. However, perhaps I missed where you discuss this point in the manuscript? It would be useful to add a brief explanation of why such a relationship does not exist, although I imagine it has to do with the similar or constant slip weakening distance.
2) Line 83: "a fault can lead to generation". Perhaps change to: "to *the* generation"
3) Line 148: "Biomarker thermal maturation is strongly temperature dependent, and larger events with the greatest temperature rise will dominate the maturity signal (Coffey et al., 2019). Therefore, we assume that any biomarker heating signal present is a result of the largest earthquake the fault has experienced. As a result, frictional energy estimates from biomarkers on the faults compiled here represent the largest displacement events and likely are an upper bound on frictional energy." Perhaps this is a basic question, but I wonder about the influence of repeated events. If a fault slips 10 times with the same displacement of 1 m, for example, does the increase in temperature derived from the biomarkers indicate slip of 1 m, or of 10 m? It seems like it records only the slip of the largest earthquake, but I wonder why there is no apparent influence of overprinting, or multiple episodes of heating/slip.
4) Figure 2: How does the geometry/asymmetry of the PSZ influence the derivation of temperature rise? For example, does it matter if a fault zone has a gouge zone on only one side of the fault, as shown in (c), or on both sides?
5) Line 165: "Slip zone thickness is measured in the field (Figure 2c), however thickness may vary along a fault, and where possible we couple each temperature estimate with a measured thickness from the specific point along the fault that was sampled." In practice, do you measure the thickness of the PSZ at several points along a fault over some distance, and then average the values? Are there guidelines about the minimum distance along the fault over which to measure the thickness?
6) Line 210: "The portion of the energy budget that is the total change in energy less the radiated energy". I think I understand what you mean here, but the wording is a bit awkward.
7) Figure 3: It seems like the general trend described in the manuscript of increasing temperature rise and max temperature up to 5 km depth agrees with your data, or at least the symbols in the plots (the mean of the measurements?). However, one apparent exception seems to be the measurements at 3 km depth. It is difficult to tell which fault this measurement comes from, but I wonder if there is an explanation for this apparent deviation from the general trend, e.g., if the fault is in a subduction zone or another fluid-rich environment. More generally, it would be useful to label the symbols with the name of the fault that produced the earthquake.
8) Line 292: "The rest of the faults in this dataset have frictional energy that falls below 45 MJ/m2, with most below 26 MJ/m2, suggesting a tendency for frictional energy to

remain within a narrow range despite differences in displacement, depth, fault type, lithology, or ambient temperature". It could be useful to add constraints of the frictional energy from laboratory experiments and numerical models. For example, see:

Aubry, J., Passelègue, F. X., Deldicque, D., Girault, F., Marty, S., Lahfid, A., ... & Schubnel, A. (2018). Frictional heating processes and energy budget during laboratory earthquakes. *Geophysical Research Letters*, *45*(22), 12-274.

Passelègue, F. X., Schubnel, A., Nielsen, S., Bhat, H. S., Deldicque, D., & Madariaga, R. (2016). Dynamic rupture processes inferred from laboratory microearthquakes. *Journal of Geophysical Research: Solid Earth*, *121*(6), 4343-4365.

McBeck, J., Cordonnier, B., Mair, K., & Renard, F. (2019). The evolving energy budget of experimental faults within continental crust: Insights from in situ dynamic X-ray microtomography. *Journal of Structural Geology*, *123*, 42-53.

Zhao, Q., Glaser, S. D., Tisato, N., & Grasselli, G. (2020). Assessing energy budget of laboratory fault slip using rotary shear experiments and micro-computed tomography. *Geophysical Research Letters*, *47*(1), e2019GL084787.

Madden, E. H., Cooke, M. L., & McBeck, J. (2017). Energy budget and propagation of faults via shearing and opening using work optimization. *Journal of Geophysical Research: Solid Earth*, *122*(8), 6757-6772.

9) Line 330: "Weakening also explains the lack of relationship observed between frictional energy and depth for faults below 4 km (Figure 4)," Earlier you describe this depth at 5 km, so it could be worthwhile to change the value here for consistency, although perhaps the most correct is to write 4-5 km.
10) Figure 5: in the figure caption you switch between using E_G and G'. But I guess the data that you label E_G in the figure is actually G'? So perhaps change E_G to G' in the figure legend.

Reviewer 2

Coffey et al. 'Estimates of Earthquake Temperature Rise and Frictional Energy'

The authors present a comprehensive review of the current state of knowledge of co-seismic temperature rise with primary focus on estimates of co-seismic temperature rise with biomarkers. I think the paper will be a good addition to the existing literature on the subject and serve as a great reference material for future work on the energy budget of earthquakes. Besides collating data on co-seismic temperature rise on faults, the authors also present analysis of trends in the co-seismic temperature rise and frictional energy (as a dissipative energy sink) with depth and co-seismic displacement to provide insights on the difference in energy budgets between small and large earthquakes. The manuscript is well written and I only have minor comments that the authors should not have any troubles in addressing before their manuscript can be accepted for publication.

Comments:

Line 63: '… on the absolute shear stress level …'?

Line 87: The assumption that $\tau = \mu(\sigma - p)$ presumably only holds for aseismic ruptures. Once the fault is sliding dynamically, τ is allowed to be larger than $\mu(\sigma - p)$ due to the inertial term being comparable in magnitude. Might it be reasonable to just call τ the frictional strength instead of shear stress?

Line 94: But why would pore-pressures follow the hydrostat, would pore-pressures along fault zones expected to follow a steeper-than-hydrostatic gradient which would in turn allow fluids to travel up faults? Has there been any analysis of how fluid overpressures can impede pseudotachylyte formations?

Lines 150 – 153: I agree with Reviewer 1 that the authors should address the role of accumulated temperature change due to multiple earthquakes versus the temperature change due to the largest earthquake in modulating biomarker thermal maturity. If the thermal maturity is sensitive to the former, then a lot of the constraints on energy budgets of earthquakes might be potentially misleading.

Line 157: What is $U_{37}^{k'}$?

Eq. 4: Since, for cracks, $D \propto \Delta\sigma$, might it be better to write $\Delta W \propto \frac{\Delta\sigma^2 L}{E}$ as the potential energy (or the mechanical energy release rate) where L is the length of the fracture? Given stress drops of earthquakes are relatively constant, this shows a clear difference in potential energy between small and large earthquakes. I acknowledge, however, that this is a matter of taste.

Lines 203-204: The way the sentence is written, it seems the authors imply that fracture energy and work done to overcome frictional resistance are independent of each other. When imagining earthquake ruptures as shear fractures on pre-existing planes of weakness, the fracture energy comes about from the work done in going from peak to residual friction on the fault. In classical models, this does not involve any plastic deformation at the tip of the shear fracture unlike tensile fractures. For example, in Figure 1, the area shaded as E_F is the frictional work done while sliding at the residual frictional level σ_f on the fault. The work done E_G, on the other hand, can be associated with work done in overcoming the transition from peak (σ_0) to residual friction (σ_f) and is an essential implication of variation in friction levels with slip. So, fracture energy might be related to frictional work. Off-fault damage can of course include distributed plastic deformation.

This is also the reason I found Figure 6 a bit hard to understand. Why does the curve for EG not begin at the peak friction σ_0? Why is also the frictional work curve not at a constant level? I guess my problems are rooted in the way I have always interpreted Figure 1 as the energy budget for one earthquake event - once slip weakening is complete, the friction is held at constant residual level in this picture. I guess the authors want to make the point that the residual friction level decreases with increasing amount of slip but I fear that this might be misinterpreted with the authors' suggesting additional slip weakening over the slip-weakening already as part of E_G. Is it not true that the authors' results really imply a lower, constant, residual frictional level for larger earthquakes than for small ones? The boundary between EG and ER might be exponential in view of equation 3 for larger earthquakes given that the much larger peak-to-residual strength drop for dynamical weakening would dominate the fracture energy than that for conventional slip weakening (or rate-state style weakening) which dominate at lower slip rates. I may have misunderstood the content of the schematic, in that case it would be great if the authors could kindly clarify.

This raises one more question – it seems that dynamic weakening mechanisms really kick in at fast slip rates than at some critical amount of slip, at least in laboratory experiments. The authors' results would imply that the slip rate for smaller earthquakes is somewhat capped off a levels lower than those required for onset of dynamical weakening. Why is this the case? If one assumes that stress drops and rupture speeds of small earthquakes are similar to those for larger ones (implying that large earthquakes have a larger event duration), why should peak slip rates be different for smaller earthquakes. Of course, all of these discussions assume crack-like ruptures. I am wondering if there might be something inherently pulse-like about larger earthquakes which allows dynamic weakening to be more prevalent? I am not expecting the authors to address this query in this manuscript, it is just comment on an issue which seemed interesting.

Lines 210 – 214: I did not completely understand these sentences. Isn't the energy budget – radiated energy the fracture energy + frictional energy?

I would also request the authors to address the comments of Reviewer 1 in preparing the revised manuscript.

Path Bhattacharya

We thank the reviewers for their useful comments and suggestions. We have incorporated the majority of all suggestions and our responses to their revisions are below in blue.

Reviewer 1:

Review of “Estimates of earthquake temperature rise…”
This work aggregates estimates of coseismic temperature measurements across several faults, and examines the general trends in the data, such as the relationship between thickness of the principal slip zone, displacement, depth and temperature rise. This analysis finds a general trend of increasing temperature rise with depth up to 4-5 km, and then relatively constant measurements, and no clear trend between the thickness or displacement and temperature rise. The work compares the energy budget of large and small earthquakes, and finds that frictional energy dominates the energy budget of small earthquakes, whereas the three components of the earthquake energy budget are generally similar in magnitude for larger earthquakes. I enjoyed reading this manuscript and suggest it may be published following some minor clarifications.

1) The abstract mentions, and Figure S2 shows that there is not a clear relationship between the PSZ thickness and the temperature rise or maximum temperature. However, perhaps I missed where you discuss this point in the manuscript? It would be useful to add a brief explanation of why such a relationship does not exist, although I imagine it has to do with the similar or constant slip weakening distance.

Supplementary figure 2 only shows displacement not PSZ thickness. There will be a following paper that explores PSZ thickness, so this is not a focus of our paper. We have removed this point in the abstract to reflect that.

2) Line 83: “a fault can lead to generation”. Perhaps change to: “to *the* generation”
We have made the suggested change

3) Line 148: “Biomarker thermal maturation is strongly temperature dependent, and larger events with the greatest temperature rise will dominate the maturity signal (Coffey et al., 2019). Therefore, we assume that any biomarker heating signal present is a result of the largest earthquake the fault has experienced. As a result, frictional energy estimates from biomarkers on the faults compiled here represent the largest displacement events and likely are an upper bound on frictional energy.” Perhaps this is a basic question, but I wonder about the influence of repeated events. If a fault slips 10 times with the same displacement of 1 m, for example, does the increase in temperature derived from the biomarkers indicate slip of 1 m, or of 10 m? It seems like it records only the slip of the largest earthquake, but I wonder why there is no apparent influence of overprinting, or multiple episodes of heating/slip.

In Coffey et al. (2019) we modelled the effect of multiple earthquakes on biomarker thermal maturity. We observed that the measured thermal maturity is highly dependent on the largest displacement or temperature rise the fault has experienced. This is because during heating biomarkers undergo structural changes to become stable at higher temperatures. Therefore, once a biomarker experiences heating at some temperature it then requires higher temperatures to change once more from its new more thermally-stable configuration to one that is more stable at higher temperature. As a result, any subsequent smaller events will cause negligible biomarker alteration and will not affect thermal maturity.
The exception to this is if biomarkers repeatedly experience the same displacement. This was shown to lead to a cumulative effect on biomarker thermal maturity, due to spending more time at that temperature. However, because biomarker maturity is much more sensitive to temperature than time, temperature will always have the larger effect on maturity.

When we interpret biomarker thermal maturity, we make the assumption that the signal is a result of the largest earthquake the fault has experience. We believe that this assumption is reasonable as we would expect some variation in displacement between events at a particular site on a fault as shown by other studies (e.g., Nicol et al., 2016 who show that the single event displacement on an individual fault has a coefficient of variation of 0.4 ± 0.2). Nevertheless, it is possible if this assumption does not hold true then the frictional energy calculated from biomarker thermal maturity is maximum bound and we've made that clearer in lines 155 – 158.

4) Figure 2: How does the geometry/asymmetry of the PSZ influence the derivation of temperature rise? For example, does it matter if a fault zone has a gouge zone on only one side of the fault, as shown in (c), or on both sides?

When we model temperature rise, we assume that the material is the same on either side of the fault. If this not the case and the thermal properties (e.g., thermal diffusivity) of the material on either side of the fault are different heat may diffuse at a faster or slower rate away from the fault. We do not address this scenario in the context of this paper, but we have added a sentence stating this assumption in the thermal modelling section of the Supplement (S1)

5) Line 165: "Slip zone thickness is measured in the field (Figure 2c), however thickness may vary along a fault, and where possible we couple each temperature estimate with a measured thickness from the specific point along the fault that was sampled." In practice, do you measure the thickness of the PSZ at several points along a fault over some distance, and then average the values? Are there guidelines about the minimum distance along the fault over which to measure the thickness?

This varies on a case-by-case basis. Where a range is given in table 1 there are multiple measurements of slip layer thickness along the fault/across the fault zone. There is no minimum distance along the fault over which thickness is measured.

Where slip layer thicknesses are utilised for biomarker thermal maturity based estimates of temperature rise, we use a thickness that is specific to where each biomarker thermal maturity measurement was made to model temperature at that location for most cases. In the case where a specific thickness cannot be coupled to a thermal maturity measurement e.g. SAFOD (Coffey et al. 2022), slip layer thicknesses across the fault zone were measured and used to develop a distribution of thicknesses that can then sampled during thermal modelling. This range of possible thicknesses is then represented in our uncertainty estimates.

In cases like at Nankai (Sakaguchi et al.2011) the thickness reported is that of the structure in the core where the vitrinite reflectance anomaly was observed.

6) Line 210: "The portion of the energy budget that is the total change in energy less the radiated energy". I think I understand what you mean here, but the wording is a bit awkward.

We agree that the wording is awkward and we have rewritten and removed some of this text for clarity (in lines 215-222).

7) Figure 3: It seems like the general trend described in the manuscript of increasing temperature rise and max temperature up to 5 km depth agrees with your data, or at least the symbols in the plots (the mean of the measurements?). However, one apparent exception seems to be the measurements at 3 km depth. It is difficult to tell which fault this measurement comes from, but I wonder if there is an explanation for

this apparent deviation from the general trend, e.g., if the fault is in a subduction zone or another fluid-rich environment. More generally, it would be useful to label the symbols with the name of the fault that produced the earthquake.

The points at 3 km depth are from the central San Andreas Fault and the Punchbowl Fault. While we agree that labelling the symbols in Figure 3 would be useful, we think that doing this in a legible way would crowd the plot and make it confusing to the reader. The reader can determine the fault using Table 1. We have added text to caption of Figure 3, stating that the symbols are temperature means.

Given the error bars on the points in this plot, we disagree that the faults at 3 km depth are statistically significantly different than the rest of the trend.

8) Line 292: “The rest of the faults in this dataset have frictional energy that falls below 45 MJ/m2, with most below 26 MJ/m2, suggesting a tendency for frictional energy to remain within a narrow range despite differences in displacement, depth, fault type, lithology, or ambient temperature”. It could be useful to add constraints of the frictional energy from laboratory experiments and numerical models. For example, see:

Aubry, J., Passelègue, F. X., Deldicque, D., Girault, F., Marty, S., Lahfid, A., ... & Schubnel, A. (2018). Frictional heating processes and energy budget during laboratory earthquakes. *Geophysical Research Letters*, *45*(22), 12-274.
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Madden, E. H., Cooke, M. L., & McBeck, J. (2017). Energy budget and propagation of faults via shearing and opening using work optimization. *Journal of Geophysical Research: Solid Earth*, *122*(8), 6757-6772.

We’ve included constraints from laboratory studies in Lines 294 – 295.

9) Line 330: “Weakening also explains the lack of relationship observed between frictional energy and depth for faults below 4 km (Figure 4),” Earlier you describe this depth at 5 km, so it could be worthwhile to change the value here for consistency, although perhaps the most correct is to write 4-5 km.

We have changed this to ~ 5km for consistency and agreement with our data

10) Figure 5: in the figure caption you switch between using E_G and G’. But I guess the data that you label E_G in the figure is actually G’? So perhaps change E_G to G’ in the figure legend.

We have changed E_G to in the figure legend, added text to clarify what G’ is throughout the text, and refer to it consistently as the breakdown energy in the text.

Reviewer 2:

Coffey et al. 'Estimates of Earthquake Temperature Rise and Frictional Energy'
The authors present a comprehensive review of the current state of knowledge of co-seismic temperature rise with primary focus on estimates of co-seismic temperature rise with biomarkers. I think the paper will be a good addition to the existing literature on the subject and serve as a great reference material for future work on the energy budget of earthquakes. Besides collating data on co-seismic temperature rise on faults, the authors also present analysis of trends in the co-seismic temperature rise and frictional energy (as a dissipative energy sink) with depth and co-seismic displacement to provide insights on the difference in energy budgets between small and large earthquakes. The manuscript is well written and I only have minor comments that the authors should not have any troubles in addressing before their manuscript can be accepted for publication.

Comments:
Line 63: '… on the absolute shear stress level …'?

Added the word level

Line 87: The assumption that $\tau = \mu(\sigma - p)$ presumably only holds for aseismic ruptures. Once the fault is sliding dynamically, τ is allowed to be larger than $\mu(\sigma - p)$ due to the inertial term being comparable in magnitude. Might it be reasonable to just call τ the frictional strength instead of shear stress?

We acknowledge that shear stress may be higher than $\mu(\sigma - p)$ during coseismic slip but due to how terminology is used in the relevant literature (e.g. Di Toro et al. 2011; Aubry et al. 2018), we stick with using shear stress throughout the text to avoid confusion.

Line 94: But why would pore-pressures follow the hydrostat, would pore-pressures along fault zones expected to follow a steeper-than-hydrostatic gradient which would in turn allow fluids to travel up faults? Has there been any analysis of how fluid overpressures can impede pseudotachylyte formations?

We have not done analysis into how pore-pressures outside of hydrostat influence frictional energy or how they may influence the formation of pseudotachylyte. However, we note that this is an assumption and we outline this in the text. We also note that having non-hydrostatic pore-fluid pressure should not alter that frictional work obtained from biomarker studies, but the shear stress estimate and therefore displacement estimate needed to fit thermal maturity measurements may move points left or right on Figure 5.

Lines 150 – 153: I agree with Reviewer 1 that the authors should address the role of accumulated temperature change due to multiple earthquakes versus the temperature change due to the largest earthquake in modulating biomarker thermal maturity. If the thermal maturity is sensitive to the former, then a lot of the constraints on energy budgets of earthquakes might be potentially misleading.

We have responded to this in Reviewer 1's comments

Line 157: What is $U37k'$?

$U^{k'}_{37}$ is the alkenone unsaturation ratio used as a proxy for coseismic temperature rise in past studies (e.g., Rabinowitz et al. 2017; Rabinowitz et al. 2020). We have clarified what this is in the text.

Eq. 4: Since, for cracks, $D \propto \Delta\sigma$, might it be better to write $\Delta W \propto \frac{\Delta\sigma^2 L}{E}$ as the potential energy (or the mechanical energy release rate) where L is the length of the fracture? Given stress drops of earthquakes are relatively constant, this shows a clear difference in potential energy between small and large earthquakes. I acknowledge, however, that this is a matter of taste.

Equation 4 is a standard expression that explains Figure 1, so we are going to stick with our original equation. This more rigorously relates the available potential energy under the standard diagram based on dynamic fracture theory. In principle, the stress drop is proportional to average slip, however this proportionality depends on the geometry of the rupture.

Lines 203-204: The way the sentence is written, it seems the authors imply that fracture energy and work done to overcome frictional resistance are independent of each other. When imagining earthquake ruptures as shear fractures on pre-existing planes of weakness, the fracture energy comes about from the work done in going from peak to residual friction on the fault. In classical models, this does not involve any plastic deformation at the tip of the shear fracture unlike tensile fractures. For example, in Figure 1, the area shaded as E_F is the frictional work done while sliding at the residual frictional level σ_f on the fault. The work done E_G, on the other hand, can be associated with work done in overcoming the transition from peak (σ_0) to residual friction (σ_f) and is an essential implication of variation in friction levels with slip. So, fracture energy might be related to frictional work. Off-fault damage can of course include distributed plastic deformation.

This is our point exactly, and which is why we reference other work that have termed this "breakdown energy", which includes potential contributions from friction and fracture energy. We have reworded this paragraph and provided more text throughout the manuscript that provides additional clarity.

This is also the reason I found Figure 6 a bit hard to understand. Why does the curve for EG not begin at the peak friction σ_0? Why is also the frictional work curve not at a constant level? I guess my problems are rooted in the way I have always interpreted Figure 1 as the energy budget for one earthquake event - once slip weakening is complete, the friction is held at constant residual level in this picture. I guess the authors want to make the point that the residual friction level decreases with increasing amount of slip but I fear that this might be misinterpreted with the authors' suggesting additional slip weakening over the slip-weakening already as part of E_G. Is it not true that the authors' results really imply a lower, constant, residual frictional level for larger earthquakes than for small ones? The boundary between EG and ER might be exponential in view of equation 3 for larger earthquakes given that the much larger peak-to-residual strength drop for dynamical weakening would dominate the fracture energy than that for conventional slip weakening (or rate-state style weakening) which dominate at lower slip rates. I may have misunderstood the content of the schematic, in that case it would be great if the authors could kindly clarify.

The standard diagram assumes peak and initial stress are the same, which is what we do here, however for dynamic rupture the initial stress can be lower than peak stress resulting in some additional dissipation above the initial stress, which contributes to the breakdown energy which is inferred seismologically (e.g. Lambert and Lapusta 2020). We have no independent constraint on that, so we don't consider it here. We have added this thought to the paper, and have changed the standard diagram slightly to emphasize that breakdown energy goes to both friction and fracture energy.

This raises one more question – it seems that dynamic weakening mechanisms really kick in at fast slip rates than at some critical amount of slip, at least in laboratory experiments. The authors' results would imply that the slip rate for smaller earthquakes is somewhat capped off a levels lower than those required for onset of dynamical weakening. Why is this the case? If one assumes that stress drops and rupture speeds of small earthquakes are similar to those for larger ones (implying that large earthquakes have a larger event duration), why should peak slip rates be different for smaller earthquakes. Of course, all of these discussions assume crack-like ruptures. I am wondering if there might be something inherently pulse-like about larger earthquakes which allows dynamic weakening to be more prevalent? I am not expecting the authors to address this query in this manuscript, it is just comment on an issue which seemed interesting.

The experiments show that there is a slip weakening distance required for dynamic weakening, such as the term Dth in Di Toro et al.(2011) which attributes this to displacement needed to achieve temperatures where thermally-activated weakening processes kick in. Our frictional energy estimates show is that smaller earthquakes do indeed put more energy (in a relative sense) into frictional energy than larger earthquakes, so this squares with experiments.

Lines 210 – 214: I did not completely understand these sentences. Isn't the energy budget – radiated energy the fracture energy + frictional energy?

We have rewritten these lines to provide more clarity (Lines 215 – 222)

I would also request the authors to address the comments of Reviewer 1 in preparing the revised manuscript.

We have addressed the comments of Reviewer 1 and those responses are included in this document.

References cited here:

Aubry, J., Passelègue, F. X., Deldicque, D., Girault, F., Marty, S., Lahfid, A., ... & Schubnel, A. (2018). Frictional heating processes and energy budget during laboratory earthquakes. *Geophysical Research Letters*, *45*(22), 12-274.

Di Toro, G., Han, R., Hirose, T., De Paola, N., Nielsen, S., Mizoguchi, K., ... & Shimamoto, T. (2011). Fault lubrication during earthquakes. *Nature*, *471*(7339), 494-498.

Lambert, V., & Lapusta, N. (2020). Rupture-dependent breakdown energy in fault models with thermo-hydro-mechanical processes. Solid Earth, 11(6), 2283-2302.

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