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Interactive comment on "Coseismic fluid–rock interactions in the Beichuan-Yingxiu surface rupture zone of the Mw 7.9 Wenchuan earthquake and its implication for the fault zone transformation" *by* Yangyang Wang et al.

Anonymous Referee #2

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Review of Wang et al., SE-2020-117 The manuscript by Wang et al describes a study of fault rocks formed in the near-surface of the Beichuan-Yingxiu fault, which produced a Mw 7.9 earthquake in 2008. The central claims of the manuscript, as I see them, are as follows: 1) that the mineralogy and geochemistry of the fault rocks varies systematically across the identified architectural elements (i.e. gouge, damage zones, protolith); 2) that the patterns of mineral transformation and apparent mass loss in the fault core/main gouge zone are driven by coseismic frictional heating and thermal pressurization; and 3) that the patterns observed in the surrounding damage zones are

the result of post-seismic ingress of both surface derived and hydrothermal fluids. Of these, I find that only claim (1) is supported by the provided data. The remaining claims are entirely unsupported. More troubling, it is my opinion that claims (2) and (3) are effectively assumed by the manuscript, and the data are then interpreted selectively to support them. It is my recommendation that this manuscript be rejected without additional consideration.

The quality of the presentation of this manuscript also leaves much to be desired. I understand and sympathize that the authors are likely not native English speakers, but that does not reduce the requirement that for a work to be publishable, it must first be understandable. I struggled greatly in trying to understand many of the key portions of this manuscript as written. Some of these areas are noted below with suggestions for improvement, but this is far from an exhaustive list.

The most significant issue with this manuscript, in my opinion, is in the sheer number of assumptions regarding fault behavior. Yes, this fault has produced surface-rupturing earthquakes, so coseismic deformation must play a role on some level, but that is only necessarily true for deformation. It is not an a priori requirement that any of the mineral transformations, mass loss, other fluid-rock interaction occurred coseismically, with or without frictional heating. Independent evidence for these things needs to be presented. I would argue that all of the observations presented by the manuscript could just as easily (and perhaps more parsimoniously) be interpreted to be the result of "passive" fluid-rock interaction occurring entirely within the interseismic period. I do not recall a single line of presented evidence that would indicate frictional heating, thermal pressurization, thermal decarbonation, or any of the other processes argued by the authors to control fault-rock formation.

The data, although somewhat poorly described, are interesting, and therefore have the potential to be published someday. I would suggest that the authors focus on constructing a new manuscript, which simply presents the available data set clearly, and in a manner that is untainted by a preconceived narrative. Then, discuss the potential mechanisms that could produce the observed trends. This would include significant discussion of the possibility that all of the observations are the result of passive fluid-rock interactions during the interseismic periods, unless independent, positive evidence can be produced to argue for a coseismic origin specifically.

Line Referenced Comments: 20-37: The role of fluids in influencing fault-rock mechanical and geochemical properties isn't at all debatable in my opinion. Fluids are a major factor, even if an incompletely understood one. The abstract overall is a bit convoluted and difficult to read. Part of this is due to the use of terms that are apparently specific to the studied fault zone (e.g. "upper and lower" damage zones, gouge "central-strong deformation region", etc). Finally, the ending sentence simply states that frictional heating, fracturing, and fluid-rock interaction affect the composition and mechanics of fault zones. Is this really the only new information provided by this work?

41: Fluid "action" cannot be "present", but fluids themselves certainly can be.

42: This definition of thermal pressurization is redundant. Sentence could be greatly simplified.

47: Suggest "Coseismic frictional heating may intensify fluid-rock interactions…", or similar. Your definition of fluid-rock interaction neglects the formation of cements/veins, sinters, pressure solution, etc which are major controls on fault rock hydromechanical properties. Suggest that devolatilization may be a better term than "deaeration".

57: Just macroscopically? Authigenic phyllosilicates are often nanometric in scale. This differentiation between macro and microscopic processes is kind of trivial. Both mineral alteration and geochemical enrichment/depletion occur at a variety of scales.

62: "Fluid" is misspelled in this line.

65-66: Probably true, but it would be useful to state how so here.

66-68: Again, I don't think the role of fluid-rock interaction in fault-zone development is debatable at all. Especially in sandstones, where a huge amount of work has been

conducted from the early 1990's until today. See work by Evans, Goodwin, Shipton, Williams, Fossen, Soliva, Balsamo, Storti, Eichhubl, Laubach, Mozley, and Petrie just to name a few.

68-72: This line states that a specific earthquake had never occurred before it occurred, which is of course true. Suggest rephrasing for clarity.

76-77: Fluid is misspelled again. I do not understand the differentiation that is being proposed here.

77-87: If I am honest, I cannot understand exactly what the authors are attempting to convey here. Based on what I can understand, it seems that most of this needs some corresponding citations. The questions at the end of this section are effectively "begging the question". The manuscript has not yet stated that these things have occurred in the exposed portions of the fault in order for the reader to wonder what their mechanisms and distribution may be.

88: I do not think an acronym is really required to describe one short word.

96: Same misspelling of "fluid".

104: Not sure what exactly "fresh" means in this case.

99-127: Section needs major rewriting for clarity. The only information I pulled out of this is that the fault is transtensional / oblique-slip normal with an apparent displacement magnitude on the order of 10 or so meters.

132: Figure 2 does not show/label any of the structural features described here other than the gouge layer, which is itself entirely obscured by the annotation.

150-151: How were they deposited? Smear, vacuum, gravity settling?

159: You have not defined reference intensity ratio (RIR). This method relies on "spiking" the sample with a known mass of corundum powder. Was this done? If so I do not see where that is described.

162-167: I do not think it is reasonable to push all of your XRF methodology descriptions to other sources. A summary here would be useful.

170: So you sieved out the >2 mm grains prior to grinding for xrd? Why?

173: Why not just specify what clay minerals were there rather than just one that wasn't?

174-175: What is this line trying to say?

183: The matrix?

193: Previously you said there was no smectite. Now there is illite/smectite, which is typically classified as a smectite.

216-220: I'm sorry, but a) where does fracturing come into this? b) how do we know necessarily that any of this is coseismic? c) what does an "open dynamic geological process" mean?

256: Without time, this cannot be a "rate".

289: Again, how do we know this is coseismic?

311: Again, not a rate.

325-327: The two clauses in this sentence effectively state the same thing.

335-344: Ok, so frictional heating could be a mechanism of transforming smectite to illite. Any direct evidence that it actually was? That transition does occur in the crust in the absence of frictional heating.

345-348: Seems like some citations for this assertion would be appropriate.

353-354: Or, it was just dissolved during fluid-rock interaction in an acidic environment, which you argued was the case locally in the last paragraph. The fact that the loss of carbonate minerals extends beyond the principal slip surface into the surrounding damage zone is yet more evidence that the process is driven by dissolution rather

than thermal decarbonization. Peak frictional heating temperatures on faults dissipate rapidly to low values in the surrounding rock.

389-390: With some exceptions, we typically think of chlorite authigenesis occurring somewhere in the temperature range of 150-200 C or greater. So, it would seem that this manuscript is invoking very hot fluids within the uppermost few meters of the surface? Seems this would preserve some evidence of boiling?

413-415: I have yet to see a single line of evidence indicating that the mineral transformation and elemental mass change occur coseismically or even shortly thereafter. Not one. Why is this narrative being pushed so hard? Why not simply describe the system, and discuss the potential ways in which it may have developed. This section effectively assumes the "answer" and interprets the data selectively in a way that fits the preferred narrative. That is not good science.

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