

## ***Interactive comment on “Synchrotron FTIR imaging of OH in quartz mylonites” by Andreas K. Kronenberg et al.***

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Author Comments to Reviews of "Synchrotron FTIR imaging of OH in quartz mylonites" by Kronenberg et al.

We thank both referees for their careful reading of this manuscript, their encouraging remarks, and for excellent ideas to prepare a more concise paper. We have redrafted our manuscript following the referee's suggestions. In the following, we respond to the referee's comments and criticisms, followed by a description of the changes we made to the manuscript.

Where comments are specific, we state our specific changes to the manuscript. Where more general recommendations are made, such as shortening the manuscript, we indicate the reductions in words for particular sections, but do not enter entire sec-

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tions of the manuscript here. We are prepared to submit the newly revised, abridged manuscript as the review/discussion proceeds.

#### Comments/Responses to Review & Comments of Jed Mosenfelder (Referee)

This is an exhaustive, painstaking study that represents a very useful contribution for people trying to correlate OH contents of quartz to deformation textures. The contoured maps look great and I found much of the discussion about deformation mechanisms and mechanisms for fluid infiltration in section 5 intriguing.

Now for the slightly negative take, I hate to see all of this good stuff buried in a paper that, in my opinion is very long and can surely be shortened by removing redundancy. For instance, nearly 1/3 of the introduction should just be removed, because it is a summary of the results and really doesn't belong in the introduction (it's unnecessary foreshadowing!). On the other hand, much of the methods section should be concatenated with the introduction, because it is not a technical description so much as a justification for why using synchrotron FTIR is useful, which is the whole point of the paper. I know that a lot of my comments are nitpicky and some are almost as wordy as the paper itself, but I hope these suggestions will be constructive for improving the presentation of the paper.

**Authors' Response:** We had not intended to write such a long manuscript and have shortened the manuscript, particularly the abstract, introduction and methods, removing redundancy and deleting information that is not necessary.

**Changed Text of Manuscript:** We have attempted to shorten the manuscript, primarily the abstract, introduction and methods sections. The abstract has been shortened (from 527 words) to 404 words. The introduction has been condensed (from 914 words) to 684 words. As suggested, we have combined some of the contents of the introduction and methods sections (mostly moving content from the introduction to the methods section). We agree that some of the more technical content is more appropriate to the methods section. The overview of methods (beginning of Section 3) has been reduced

(from 405 words) to 288 words, and description of IR plate preparation (Section 3.1) has been reduced (from 715 words) to 585 words. Section 3.2 on FTIR spectroscopy has been cut by 202 words, Section 3.4 on IR plate thickness and OH quantification has been cut by 91 words, and Section 3.5 on IR Mapping was cut by 44 words.

Referee Comment:

1. Line 136: put IR in parentheses here. Same on line 147: put FTIR in parentheses. Then, you can shorten the manuscript non-trivially by using the acronym "FTIR" without saying "spectroscopy" every time, which is redundant and kinda grating. Speaking of grating, practically no one these days uses a grating spectrometer for IR so I think it is ok to say "FTIR" when you are being general, e.g. you can use just "FTIR" instead of "IR spectroscopy" in line 172.

Authors' Response: This is a good idea and we've adopted this suggestion.

Changed Text of Manuscript: We've defined Infrared (IR) and Fourier transform infrared spectroscopy (FTIR) and used these through the rest of the manuscript. However, there are instances where reference is made to the IR source, IR beam, FTIR instruments, and IR plates, which are not condensed to just IR or FTIR.

2. Line 179-196: I feel quite strongly that this verbiage can be eliminated, as alluded to above. It's a summary of your results, which is not needed in an introduction. In fact, reading something like that just makes me want to skip reading the rest of the paper!

Authors' Response: Agreed. Nothing is lost as this is just alerting the reader to what is found in our results section.

Changed Text of Manuscript: Text deleted as suggested.

3. Section 2: the geologic background section is also very long. I guess your readership laps this stuff up though so if you feel it is really necessary to review all of these previously studied details of the two field areas, then ok. One thing I wonder is whether you could get away with using "MT" and "MCT" as acronyms throughout the paper to

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reduce the character count and eye fatigue associated with repeating Moine Thrust and Main Central Thrust so much.

Authors' Response: On the one hand, this manuscript describes new methods of quantifying water contents in naturally deformed rocks with higher resolution than has been possible before. So this would suggest that the selected mylonites need not be described as thoroughly as we have done. However, some may read our contribution for the relevance to tectonics, mechanical properties at natural crustal conditions, and the potential role of water weakening in the middle to lower crust. We prefer to give the tectonic context of these shear zones so that the geologic significance of our OH maps is not lost. We have adopted the suggestion offered here of defining acronyms MT and MCT for the Moine Thrust and Main Central Thrust, respectively, as we use these terms frequently in the text.

4. Line 207: better fill in citations for OTHERS? Here

Authors' Response: Sorry, this was an error, essentially left-over text alerting ourselves to look up references.

Changed Text of Manuscript: References were added and our own notes deleted.

5. Line 292-327: On the one hand, much of this was already said in the introduction and should not be repeated here (e.g. the long list of references citing people who used FTIR in the past). On the other hand, some of the schpiel here could be moved to the introduction as I say above in my summary.

Authors' Response: As suggested, we have eliminated redundant sections. Our reworking of the Introduction, Selected Mylonites, and Methods sections has moved tectonic and geologic background forward and technical details back to the Methods section. As such, infrared studies of fault rocks could be placed in the Introduction as suggested, but we chose to place it in the Methods, given that need to describe spectroscopic methods.

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Changed Text of Manuscript: More detailed/technical description of prior IR studies, originally intended as background for the reader in the Introduction has been moved and combined with sections on IR spectroscopy of the Methods section.

6. Line 329ff: WOW this is the longest sample preparation I have EVER read! While much of this could be interesting to technical wonks such as myself, at the risk of repeating myself ad nauseam I think some of it could be reduced. To me, the most interesting steps involve your methods for mechanically supporting the samples, not the entire sequence of grit sizes you used. You can also say in maybe just one or two sentences that you checked thicknesses using optical methods.

Authors' Response: We accept this criticism and have cut back on the methods section, particularly the details about IR sample preparation.

Changed Text of Manuscript: The section on IR plate preparation (Section 3.1) has been reduced from 715 to 585 words, retaining information on support of the IR plates but condensing information on grinding and polishing.

7. Line 404-411: I don't think this detail is necessary. I hope people who use these machines already understand that they can collect data out to 7400 cm<sup>-1</sup> (actually I wonder sometimes if they do), but who cares? It is totally irrelevant to your study, you just need to say what range you collected (saved), and the resolution and number of scans (512 or more I guess). And you also don't need to say you normalized the spectra, that detail can go in figure captions.

Authors' Response: We condensed this section as suggested.

Changed Text of Manuscript: These two sentences have been condensed, giving just the relevant IR collection and storage information.

8. line 435: I am feeling un-experty here but I am not sure I have ever seen an IR source being described as "isotropic" or "anisotropic". The light that comes into your sample is not polarized and frankly I am not sure what you are getting at here. Incidentally, Ross-

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man doesn't like using the word "pleochroic" (I guess "dichroic" is indeed appropriate for uniaxial quartz) in this context as it implies the presence of color ("chroic"), which we do not see in the infrared part of the spectrum. But I am not so nitpicky myself, believe it or not, even though I have stopped using it in my own papers.

Authors' Response: Rereading this text, we realize that we weren't clear on usage of isotropic/anisotropic. We meant that incoming radiation is approximately unpolarized (ie. no hint of more radiation with preferred E-vector orientations). What we're trying to get at here is that some (anisotropic) OH absorption bands can vary with crystal orientation while others (isotropic absorptions) do not.

Changed Text of Manuscript: We have dropped the use of "dichroic" and have replaced "nearly isotropic" by "unpolarized".

9. Line 484ff: the odd thing about this section (which I know is important to the paper) is that the spectra you refer to in figure 2c really don't have such bad fringes! In later figures you show more extreme examples. I wonder if you want to reorganize the figures, discussion, and/or figure call outs to make your point more strongly.

Authors' Response: We agree that the interference fringes for  $\sim 100 \mu\text{m}$  thick samples (Fig. 2) are not bad, and we describe them as having "manageable size for thicker samples." Indeed, interference fringes only become large enough to prevent our ability to measure absorption bands when sample thicknesses are much smaller ( $< 25 \mu\text{m}$ ) as shown in Fig. 4. In the methods section (3.2) on FTIR that first calls out Fig. 2, we only mean to point out the similarities and differences between spectra collected with a conventional FTIR and with the synchrotron-FTIR system. In this case, the difference is the size of interference fringes associated with different IR sources for a common sample thickness. We did not attempt any conventional FTIR measurements on ultrathin samples, as we did not expect to measure any absorption bands at all. So, our best comparison of fringes for a given sample thickness are restricted to the thicker samples. In Methods Sections 3.3 and 3.4, we focus on the useful aspects

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of the interference fringes. Indeed we found that we could determine local IR plate thicknesses for every spectrum using the synchrotron-FTIR system.

We present the trouble with interference fringes in the Results Section 4.2, which first calls on Fig. 4, where we show that these fringes become very large for ultrathin IR plates, and our ability to measure absorption bands in finely recrystallized grains (which require ultrathin IR plates) is limited. We prefer to retain this organization, as a description of methods (of IR measurement with different systems, and determination of IR plate thicknesses) and later description of results (both the ability to measure spectra with very small apertures, IR mapping, as well as disappointing results and limitations of the methods).

10. Line 535: strictly speaking you are not accurately separating the absorbance of mica from fluid inclusions in quartz. No way. That's because the bands overlap (they are broad bands with full width half maximum positions that overlap), so if you just draw a line like you did in figure 3a then you are underestimating the integrated absorbance of the mica. The proper way to do it is to actually fit the bands with gaussians and/or lorentzians, etc. I can't believe it matters to your conclusions and I can't believe you would want to go through the heartache of properly fitting the bands, but I also don't think you should claim that you are ACCURATELY subtracting the absorbance of the mica. This is the kind of thing I did in my dissertation 20 years ago when I was extremely naive.

Authors' Response: We accept the criticism of our "straight line" approximation to distinguish OH absorbances of micas and fluid inclusions, and change our statement that we are confident in separating absorbances due to micas and fluid inclusions to satisfaction with a first-order separation. Further, as anticipated by the referee, we're not inclined to do more sophisticated fitting (by way of Gaussian or Lorentzian functions), since we ultimately must perform this operation for 660-4950 spectra that make up a single IR map.

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Changed Text of Manuscript: We have revised the original text on original lines 562-563, “We have confidence in our ability to separate OH absorption bands due to fluid inclusions in quartz and due to micas to “We are satisfied by our ability to separate, to first order, the OH absorption bands due to fluid inclusions in quartz and due to micas”. Elsewhere we have described the separation of integrated absorbances as approximate.

11. line 546: maybe I am blind but I am not seeing a band at 3815  $\text{cm}^{-1}$  and would be curious to know what it is, it seems like a very high frequency for O-H.

Authors’ Response: Thanks for pointing this out. This is a bit of a mystery. This small “bump” in the spectrum shown in Fig. 3c is surely not actually an OH band. We do not always see it in all spectra of the MCT muscovite grains, and given its very large wavenumber, perhaps it must be explained as some type of harmonic.

Changed Text of Manuscript: We deleted reference to this band as an OH absorption band.

12. line 571: I don’t think you need to cite TWO textbooks for something so well established as the Beer Lambert law (I would have neglected to cite any here)

Authors’ Response: Sure. This was overkill. Changed Text of Manuscript: We’ve arbitrarily retained our reference to the earlier text of Stuart et al. (1996) and deleted the other.

13. Line 604: this entire discussion is a little goofy, because inherently you also do not know the path length for the inclusions so of course there is no way to quantify the amount of mica present. I suggest trimming this paragraph down.

Authors’ Response: Mostly, we wanted to explain that the contours of our IR maps differ for OH due to fluid inclusions in quartz and OH of micas. But we agree, it’s hopeless to try to quantify the mica contents of OH represented by these bands.

Changed Text of Manuscript: We have cut back on this description (from 138 words) to



65 words.

14. line 665ff: I wonder if you want to shout out to your figures here where the contouring with cool and warm colors is shown. It might put your figures out of order but it makes sense to me anyway to draw the reader's attention here to your pretty pictures.

Authors' Response: Given that this text is part of our Methods Section (3.5 on Mapping OH bands), we'd prefer to keep the maps in the Results Section as a group (4.4 on High-resolution Imaging of OH).

15. Line 700ff: It is kind of "no wonder" that you see the same bands when using synchrotron IR as opposed to a bench top FTIR, so I think you could reduce a lot of this verbiage. I mean, were you seriously expecting to see different O-H vibrations? Of course not.

Authors' Response: Sure, we didn't expect that the IR spectra would be fundamentally different, but we did learn that signal-to-noise was better than we expected at 10  $\mu\text{m}$  and that interference fringes were stronger, which in retrospect is obvious, but we hadn't anticipated.

Changed Text of Manuscript: We continue to make comparisons between spectra measured with conventional and synchrotron FTIR, focusing on differences in OH contents at different scales of measurement, but we have cut back on the obvious similarities in OH bands, as suggested.

16. line 754: the method of tilting the plate by 45 degrees is curious to me. I assume you compensate for the change in path length by doing simple trigonometry here but maybe you want to actually state that for clueless readers like me? I can't believe I am actually asking you to add something to this paper ;-)

Authors' Response: This method of reducing fringes from spectra was much more successful in a previous study (Kronenberg and Wolf, 1990) using a Bomem FTIR with a conventional globar source (before I was aware of any IR microscopes). In that study,

IR path lengths were estimated from Si-O bands selected for their nearly isotropic character, thereby tracking path length within quartz, no matter the direction. Going back to the earlier study, I don't think that we fully understand how internal fringes were reduced, as reflections from perfect top and bottom polished surfaces reflect just as well at some arbitrary angle. It may be that some form of incoherent scattering is involved (perhaps at internal flaws). Given that tilting did not reduce fringes much using the synchrotron/FTIR system of this study, we did not pursue this method further.

17. Line 768ff: this seems to be an interesting by-product of your study that could be quite useful to people in this field and so I wonder whether you want to draw attention to it with a sentence or two in the abstract.

Authors' Response: We agree. However, we worry that this will be buried in this study, no matter if it appears in the abstract or not. Oddly it's a combination of preparing polished sections so thin that image resolution is enhanced (even though we would have thought undulatory extinction in quartz would be undetectable - we prepared them only for the IR, not for microscopy), and the easy image manipulation of Photoshop (or other image software) to enhance undulatory extinction through contrast and brightness. Given what we've learned, we intend to prepare samples this way for future microstructural studies, and expect that it will have more impact when the scientific results of the study are tied to the methods. We appreciate your idea of highlighting this unexpected means of studying optical microstructures.

18. line 810: imaging, not imagining, unless you are Disney. No shoot, that's imagining ;-)

Authors' Response: In the heading no less! OK, that's a glaring typo.

Changed Text of Manuscript: Corrected. Thanks!

19. Line 950: Seaman spelled wrong here

Authors' Response: Another typo. Thanks for the correction!

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Changed Text of Manuscript: Corrected

20. Line 999: copy editing trivia here but I am not sure why Holger's paper is underlined

Authors' Response: We're not sure either. "Stünitz et al., in press" is not underlined in the original Word document we prepared, but we recognized at the time that it would likely come out before this manuscript.

Changed Text of Manuscript: This citation now reads "Stünitz et al., 2017" in the text and this reference has been updated in the References Section.

21. line 1030: well, this is an interesting idea but it is a purely hypothetical concept and I cannot think of any practical way to do this so I am not sure it should be in here. What is the material you are thinking of that has the same index as quartz and yet is IR transparent AND can somehow magically bond to the sample (by melting? seriously? Not gonna happen for CaF<sub>2</sub> or BaF<sub>2</sub> etc. There is no material I can think of with a low melting temperature, such as a plastic, that will also have a high refractive index) so there is no air gap, without risking affecting the OH content? Nutty.

Authors' Response: We agree that it is a bit novel to suggest unproven methods for future studies, rather than just sticking to the methods we already developed and which succeeded in our study. Yet, we are not able to gain ready access a synchrotron-FTIR system to test these "left-over" ideas, now that the Brookhaven FTIR system is down (temporarily we hope). In addition, even if there were no shortage of synchrotron-FTIR systems, our experience with synchrotron beamtime allocation for short intense periods of time (two sessions, one for 2 days and another for 4 days) was that we were caught unprepared for such surprises as large interference fringes in our carefully prepared thin samples, and other unwelcome surprises. In the case of reducing the interference problem, all that we could try was tilting and changing IR objectives at hand.

The reason we have chosen to discuss unproven methods is that we'd like to prepare would-be synchrotron-FTIR researchers for challenges and as-yet unresolved prob-

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lems, and pass on any ideas that might overcome these problems. The criticism here is certainly warranted, since we have not identified a substrate with the desired melting and optical properties. However, we're not sure it's time to give up. In particular, there is a broad class of salt-like materials known as ionic liquids, which have simple cations and complex (big molecular) anions, which melt a little above or below room temperature. This class of materials is popular with some chemists as media and catalysts for protein reaction, folding and unfolding. Studies of refractive index have even been done on ionic liquids, and some have larger  $n$  values. Finding the right ionic liquid with a "hot plate" melting point, refractive index of  $\sim 1.5$ , and lacking OH bands will take a real search and access (or synthesis), but it may not be impossible.

22. line 1042: I like this visionary idea better but it sure is going to get expensive with all those sections you want to look at ;-)

Authors' Response: We justify listing all three potential solutions to reducing interference fringes by the same arguments as above. Don't get us wrong, we like the Mosenfelder-Rossman idea, and had we known about it during our synchrotron beamtime, we would have tried it. Still, given the expense of synchrotron beamtime, FIB preparation of sample surfaces may not be unreasonable.

23. section 5.1, 5.2, and 5.3: the geological implications for sections 5.1 and 5.3 are important and the technical discussion in 5.2 sort of sits uncomfortably between them. I would consider just moving the sections around so that 5.1 and 5.3 are together (maybe at the end rather than the beginning? I dunnow).

Authors' Response: Good idea. Thanks for this suggestion!

Changed Text of Manuscript: Sections 5.1 and 5.2 have been switched.

24. figure captions, general comment: wow, they are way too long (particularly figure 3). Much of this information repeats what is said in the text. My general philosophy about figure captions is that you should try not to repeat too much of what is said in

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the text and try not to have too much interpretation within the figure caption - the goal is to just describe the facts of what you are presenting (for instance noting that spectra are normalized and offset is ok - in fact you can just say "spectra normalized to 1 mm thickness"). I got this philosophy from working with Tom Ahrens who totally hammered on me about this and I came to totally agree with him about it.

Authors' Response: We accept this criticism, and have cut the figure captions down in size, following the suggestions made here.

Changed Text of Manuscript: Figure captions have been shortened, deleting information given in the manuscript text and referring to common contouring conventions.

25. figure 3b: I may fundamentally misunderstand this. Is this spectrum the result of subtracting away the OH in quartz or is it actually a spectrum of pure mica? If the former, then it just demonstrates how my complaint about your subtraction is valid, because the right side of the band looks abruptly cut, in other words there should be a tail of absorption on the low wavenumber side but it abruptly dives to zero. I think this is an artifact of your subtraction method but I am not sure.

Authors' Response: Sorry. We need to revise our caption to this figure. None of these spectra have been subject to subtraction. Fig. 3a is the spectrum of a quartz grain in the MT with clear signs of mica inclusions through the 3600  $\text{cm}^{-1}$  OH band (even though we do not detect micas by optical microscopy). Figs. 3b, 3c, and 3d are all spectra (without any subtraction) of mica grains, where we could find nice large ones to measure. Fig. 3b is a muscovite grain in a MT sample. While we recognize that this spectrum shows some measurement trouble, we prefer this one to others measured in thicker IR plates where the OH absorption is too large and is clipped abruptly. This OH band of muscovite is amazingly strong and is measurable in a very thin sample! It happens that we were able to measure this grain without the heavy fringing seen for similar thickness quartz grains. Figs 3c and d are spectra (not subtracted) of muscovite and biotite grains in the MCT.

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Changed Text of Manuscript: We have rewritten this figure caption to describe the spectra more clearly.

26. Figure captions for figures 6-8: I think a lot of the detail is repeated (such as the scale for the contours, the units for OH, the "cool" versus "warm" coloring, etc) and can be said just in the first figure caption, which is referred back to.

Authors' Response: We accept this criticism, and have cut the figure captions down in size.

Changed Text of Manuscript: We have cut the length and detail of these figure captions, retaining format details in the caption to Fig. 6 and referring back to these conventions in captions to Figs. 7-9.

#### Comments/Responses to Review & Comments of Anonymous Referee #2

This manuscript presents a very detailed and comprehensive study aiming at relating the OH content in quartz mylonites to the microstructure. The most attractive part of the paper is clearly the OH maps superimposed to the microstructure. Also, the authors have pushed the limits of analysis to detect quite low water contents with a spatial resolution of about 10  $\mu\text{m}$ .

This is mostly an analytical work and the authors clearly have the will to do it well and to convince the reader of their care. I have the greatest respect for that, since the quality of the data is of primary importance in science. However, the drawback is that the paper is very long. The abstract itself is nearly two printed pages long, containing very general statements ("synchrotron IR radiation is brighter. . ." ) which should not be placed here. Besides lengthy description of technical details, the paper contains many repetitions. I have no objection for the paper being published as it is since this is not wrong, but I really think that the exposure of the work would be far better if the paper would be strongly re-written to be much shorter in order to get readily to the point (with possibly all technical details gathered in a "Methods" supplement).

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Authors' Response: We note that both referees agree on this point, and we have shortened the manuscript, particularly the abstract, introduction and methods, as described above. We have dropped some of the excessive detail and eliminated redundant text.

As for removing the methods section and placing it in supplementary materials, we prefer not to do this, as mapping OH contents in mylonites by synchrotron radiation coupled to FTIR is new, and we would like highlight methods as well as results. Moreover, we intend to refer to the methods section of this contribution in future publications, relying on it being easy to find.

It may be a small detail also, but I am not sure that the choice of cool colours for high water content is the most appropriate since one is used to look for “hot spots” in warm colours. The colour scale chosen is however clearly indicated and the reader has all information needed to correctly interpret the figures.

Authors' Response: Originally, we had produced plots with the reserve sense of colors and found it intuitively confusing. We literally had to keep telling ourselves that red meant lots of water and blue meant very little. Clearly topographic elevation, bathymetric and other common color contouring conventions show larger values (high altitudes of the Himalayas) as warm colors and smaller values (deep oceanic trenches) as cool colors. However, there are important exceptions, such as seismic tomography and imaging, where low velocities usually appear as warm colors (where the logical inference is that low velocities mean high temperatures) and high velocities as cool colors (implying high velocities mean low temperatures). Clearly, one can go wrong with inferences of physical properties and red and blue may mean other things (lithology, melt, etc). However, the color code we have chosen is less risky, as we are not making inferences of physical properties and a parameter such as temperature. The color code simply represents dry (low OH) quartz by an “arid” red color and wet (high OH) quartz by “watery” blue color.

Now, what is the point of this paper? High resolution maps of water content in relation

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to microstructure is found to be complex and heterogeneous. The link between ductility and water content does not appear simple and systematic. However, the authors recognize, that evidences for water contents are always larger than the solubility, and they highlight the usual impression that the history of water uptake through microcracking is pervasive. This confirms that water weakening must be a dynamic process and that looking, far from equilibrium, for a simple relationship between plastic strain (which is not a state variable) and water content (the evaluation of which might be biased by continuous changes) might be a concept to abandon.

Authors' Response: Compared with the study of deformation microstructures, mapping OH in deformed rocks and attempts to explain concentration patterns on the basis of water influx, loss, and water weakening is at an early stage. Thus, the complexities we find and the challenges to our understanding may not be surprising. Taking microstructures as an example, few structural petrologists expect microstructures to represent a state of equilibrium. They are the end effects of dynamic processes as well, with defect populations, grain boundaries, etc responding to driving forces. When we're lucky, deformation microstructures might represent a simple steady state deformation of constant strain rate, stress, temperature, pressure, fugacity of water, etc followed by a quick "quench" when microstructures are frozen in by a sudden plunge in temperature. However, we also know that many rocks record complex microstructures that are the overprinted results of multiple deformations at different geologic conditions.

We continue to be motivated by the experimental fact that quartz has never been deformed by dislocation mechanisms in the lab without water present. And while IR spectroscopy was initially employed in lab studies of quartz deformation to identify the equilibrium hydrogen or water defect(s) responsible for its weakening, most studies have only succeeded in measuring the sources of water (fluid inclusions, etc) that are not the equilibrium defects themselves, but may supply the needed hydrogen defects. In this sense, our FTIR of naturally deformed quartz yields results very similar to laboratory studies of experimentally deformed quartz. Moreover, it appears that some

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rocks (such as the MT) fit our concepts of a readily deformable mineral when lots of molecular water is present. The fact that some rocks appear to be dry and processes of recrystallization lead to water loss from shear zones shows that we have much to learn, but we're not discouraged. We submit that it's early to give up understanding the role of water in naturally deformed rocks and how it might change during deformation, microstructural evolution, and metamorphic reaction.

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Interactive comment on Solid Earth Discuss., doi:10.5194/se-2017-23, 2017.

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