Reply to the reviews by Andreas Kronenberg and Subhajit Ghosh

We thank both reviewers for their thorough and insightful reviews of our manuscript titled "Dissolution-precipitation creep in polymineralic granitoid shear zones in experiments II: Rheological parameters." We greatly appreciate the thoughtful comments and constructive feedback, which have provided us with valuable guidance for improving the quality of our work.

We have carefully considered your suggestions and concerns regarding the quantitative nature of the stresses reported and the data correction methods used. In response to your reviews, we would like to provide detailed responses to the most important points you raised and address the other comments in detail during the revision of the manuscript.

Briefly summarized, the major points that were raised in both reviews concerned over the quantitative nature of the stresses reported. In particular, both reviewers have sought a clarification of the process of data correction and evaluation in order to arrive at sound implications for the observed mechanical behavior.

Among many very helpful comments, which we will improve in a reviewed version of our manuscript, we would like to respond here to the following most important suggestions for improvement:

1. Additional discussion on the deformation mechanisms and their influence on the ultramylonite samples based on microstructural data.
2. Further clarification and justification of the reported stress values.
3. Addressing the limitations and uncertainties in the current experimental framework.

1. **Microstructural data**

To address this point, we would like to highlight here that a thorough microstructural analysis is shown in a companion paper (Dissolution-precipitation creep in polymineralic granitoid shear zones in experiments I: strain localization mechanisms). The microstructural manuscript is the first of the two communications. The main reason for the sequence of the companion manuscripts is that the microstructures in conjunction with the mechanical data form the principal basis for our interpretations. The second manuscript (the one concerned here) is an attempt to derive more quantitative data for potential applications for extrapolations in tectonic modeling, mainly in a semi-quantitative way. We realize that one of the problems with companion manuscripts is that reviewers typically do not get to see both manuscripts for their review. Consequently, some of the reviewers' criticism of a shortcoming of microstructural documentation is due to the fact that the microstructures and their interpretation presented here are only a brief summary of the material presented in manuscript no. 1. As it has been mentioned in our manuscript (possibly not clearly enough), the mechanical data presented here serves as a discussion of rheological parameters for polymineralic rocks as an extension of manuscript no. 1, which focusses on observations and interpretation of deformation mechanisms. The deformation mechanisms and implications for natural rocks form the main substance of the first part of this study.

2. Clarification and justification of the reported stress values

We are grateful to be able to discuss the correction of our data and the implications in the following, as we kept the discussion on this part shorter in the manuscript for keeping the length of the text short.

2.1. Application of calibrations/corrections:

Due to time constraints we did not perform our own calibration for this study. However, we evaluated the different corrections presented in the literature. For the corrections that can be applied, we would like to present a comparison of the values for stresses obtained with different corrections and the consequent calculations of resulting n-values.
In the following table 1, we show the strain rates and stresses we have calculated. The maximum range for the stress exponent n covers roughly n=1.4 to 1.8. This is an important observation, as the outcome and interpretations of our study are not affected by the exact values but only deal with the range of values.

Table 1: Measured and corrected stress measurement of different experiments and the resulting calculated stress exponents n. The stresses given here are steady state stresses at the end of the experiment, which also correspond to the final microstructures. A regression is performed on two datasets – 650 and 725°C. Then the resulting n-values are averaged, weighted by the amount of datapoints in each set. Note that experiment 615NW and 618NW are Type I experiments with a pre-fracture and the other experiments are fine-grained gouge simple shear experiments. Experiment 615NW showed slightly different microstructures and was not displayed in the first version of the manuscript, hence the n-value differs slightly. However, here we still show it to estimate the largest range of uncertainty within our study.

Exp. No	Temp. (°C)	Strain rate (s-1)	equivalent strain rate (s-1)	Shear stress (MPa)	Shear Stress friction corrected (MPa)	Equiv./Diff stress (MPa)	Diff stress-Friction corrected (MPa)	Shearstr. Friction HK, SSC (MPa)	Diffstress friction HK, SSC (MPa)	Shearstr. Friction HK, MSC (MPa)	Diffstress friction HK, MSC (MPa)	Weights
673NN	**650**	4.52E-05	5.22E-05	46.26	30.42	92.52	60.84	-1.79	-3.58	22.21	44.42	
673NN	**650**	2.91E-04	3.36E-04	195.83	186.55	391.65	373.10	112.18	224.37	136.18	272.37	
673NN	**650**	3.07E-03	3.55E-03	447.76	411.49	895.51	822.98	276.39	552.77	300.39	600.77	
673NN	**650**	3.96E-04	4.58E-04	192.38	142.38	384.75	284.76	79.94	159.88	103.94	207.88	
618NW*	**650**	1.07E-03	1.23E-03	334.46	274.14	671.20	544.78	175.97	349.69	200.12	397.69	
615NW*	**650**	9.65E-04	1.11E-03	232.17	225.69	516.90	398.55	137.57	242.94	164.76	290.94	
calculated n-value				**1.78**	**1.54**	**1.78**	**1.54**	**1.82**	**1.75**	**1.54**	**1.54**	**6**
677NN	**725**	4.85E-05	5.59E-05	18.94	11.45	37.88	22.89	-15.65	-31.29	8.35	16.71	
677NN	**725**	4.66E-04	5.38E-04	100.24	91.11	200.48	182.21	42.51	85.01	66.51	133.01	
677NN	**725**	4.78E-03	5.52E-03	275.21	253.19	550.42	506.38	160.83	321.66	184.83	369.66	
677NN	**725**	4.89E-04	5.65E-04	131.05	93.74	262.10	187.48	44.43	88.86	68.43	136.86	
calculated n-value				**1.60**	**1.40**	**1.60**	**1.40**	**1.76**	**1.76**	**1.40**	**1.40**	**4**
weighted avg				**1.71**	**1.48**	**1.712**	**1.48**	**1.80**	**1.76**	**1.48**	**1.48**	

In particular, we would like to discuss the concerns regarding the Holyoke and Kronenberg 2010 (H&K2010) molten (MSC) (Equation 1) and solid salt cell (SSC) (Equation 2) assembly corrections:

$$\sigma_{gas} = 0.73 \times \sigma_{GriggsMSC} \quad (+/-30\ \text{MPa}) \qquad \text{Eq. (1)}$$

$$\sigma_{gas} = 0.73 \times \sigma_{GriggsSSC} - 48\ \text{MPa} \quad (+/-30\ \text{MPa}) \qquad \text{Eq. (2)}$$

We understand the criticism of our application of the molten salt cell correction to our solid salt cell assembly experiments. At first sight, it appears to be an application of the wrong correction. However, we would like to point out that the slope of the two corrections is identical, and the only difference is the shift of values along the y-axis. If the solid salt correction is applied, it will result in negative differential stress values for the slowest strain rate steps (i.e. low sample strengths). This situation has arisen in our lab and in other labs before, and this is the main reason, why this correction commonly is not applied. As the authors of H&K2010 discuss in their paper, some potential problems of the calibration lie in the mating of pistons in the Griggs apparatus, which is not designed to be operated at the low confining pressures required for the calibration implementing a gas apparatus. For the reason to avoid negative stresses (these would imply an extension experiment, but we have performed only shortening experiments), we have tried to apply the correction of the slope of the calibration curve without the shift along the y-curve. This correction is the molten salt calibration (again: the slope is identical to that of the solid salt correction). Our highest n-value results from calculating the stress exponent without taking the slowest strain rate step, as it appears negative after applying the H&K2010-SSC correction. This questions the validity of the lowest stress. However, since we measured this value in a strain rate stepping experiment (the advantage of such experiments is that all stress values are measured with respect to the same reference value of the hit point, i.e. avoiding different friction terms in different experiments), we believe that the datapoint should be used as an indicator for the stress sensitivity. Hence, we used the correction for a MSC, as the SSC correction does not affect the slope and only a vertical offset with respect to the MSC. Also, as in Table 1, the influence of this correction on n, due to the very low stresses we measured, lies beyond the decimals shown/relevant.

Furthermore, the experiment at the higher temperature of 725°C gives us the smallest stress exponent of n=1.4. It could mean that the deformation mechanisms at higher temperature change, however, as shown by microstructures, the deformation mechanism remains the same. This observation is our reasoning for averaging the calculated stress exponents between the 650°C and 725°C experiments (Table 1). The averaging is performed by weighting the n-values by the number of datapoints through which the regression line was fitted. Overall, this procedure was made to take more data into account. But, as mentioned above, even considering a range of n values of 1.4 to 1.8 is not significant for our interpretations and discussion in the manuscript.

2.2. Baseline force – correction of load by piston friction calculations:

The other uncertainty being addressed is the calculation of the hit point at different strain rates. It is inferred that the force during the run-in stage of the experiment before the hit point stays constant at a given displacement rate (often termed "friction") but could change depending on the vertical displacement rate (Proctor et al. 2016). This topic still is a source of debate in the scientific community applying solid-confining-medium-apparatus, as has been shown in a workshop at Orleans in 2020, where most of such apparatus users were present. For most typical displacement rates, the effect on the measured force has been found negligible by M. Pec 2014 in their tests in the same apparatus that we used in this study. Also, Proctor, 2016 have found that the change of force for different vertical velocities at slow rates is very low, being only 3-5MPa for one order of magnitude change. Based on these assumptions, we did not perform additional run-in steps, also because it was very important for this study to have pristine microstructures. However, from the experimental series that we performed, we could investigate the run-in curves of different experiments at different strain rates. In Figure 1 we show the plots for displacement – force curves of complete experiments.

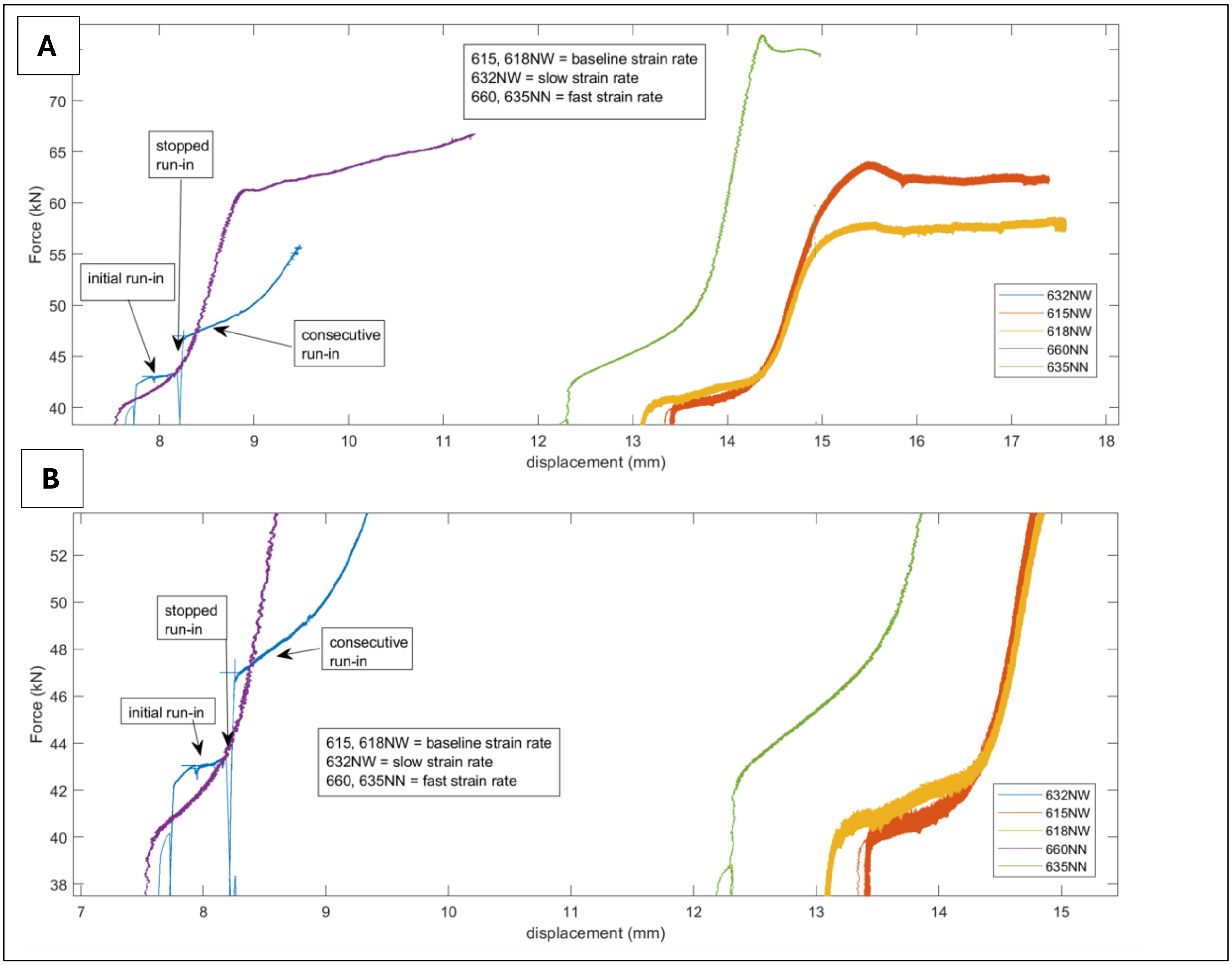

Figure 1: Comparison of the measured forces during experiments at different strain rates. The vertical piston displacement rates are defined as follows: for the baseline strain rate ≈10^{-8}m/s, slow strain rate≈10^{-9}m/s and fast strain rate≈10^{-7}m/s. 1A is the overview and 1B is zoomed into the run-in curves of the experiments.

Figure 1A is the overview, while Figure 1B is a zoom-in of the run-in curves. Three different vertical displacement rates were used here. The experiments 618NW and 615NW are performed with 10^{-8}m/s, experiments 660NN and 635NN are one order of magnitude faster at 10^{-7}m/s, and 632NN one order of magnitude slower at 10^{-9}m/s. Although at different strain rates, the run-in force of experiment 660NN is comparable to 615NW and 618NW. The run-in force of 635NN, however, is ≈5kN higher than the same strain rate experiment 660NN, and the slower strain rate experiments 615NW and 618NW. At the same time, the slow strain rate experiment 632NN shows a different trend: First, it is performed on another rig and secondly, there are two values for the run-in force. The reason for the two values (see Figure 1B) is that there was a cooling problem during run-in, and the experiment was stopped and restarted. Once experimental conditions were reached again, the slope was different from the initial run-in. Thus, we hence raise the question whether a calibration of different run-in values by hit-point stepping is even reliable within one experiment?

Based on the observations on the larger dataset of M. Pec 2014, we still assume a base-force from the hit-point even for stepping experiments, because the slope of the run-in appears to be unreliable as a calibration tool altogether.

Limitations and uncertainties in the current experimental framework:
The evaluation of different corrections we can apply to the experiments to obtain the stress exponent are a part of uncertainty. In addition, also the calculations of the grain size exponent m and activation energy Q bear a very large uncertainty as well. We probably have not communicated the purpose of this manuscript clearly enough: We do not intend to publish flow law data for real extrapolations to be implemented in, e.g., tectonic models. Our purpose is to show the mechanical consequences of our interpreted deformation mechanisms in terms of rheology in a semi-quantitative way. We need to address this point more clearly and explicitly state the severe restrictions of our approach for the quantification of the mechanical results.

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mechanisms in terms of rheology in a semi-quantitative way. We need to address this point more clearly and explicitly state the severe restrictions of our approach for the quantification of the mechanical results.

We will incorporate a more critical way to describe all uncertainties in a revised version of the manuscript. It is a justified point of the criticism that the calculated values may be "widely accepted within the community". Again, it is not our intention to provide quantitative data for extrapolation - it is beyond the scope of this study to share parameters that should be accepted and used directly. The aim is to give a first order estimate – that there is grain size sensitivity m, which is not =3 but closer to 2 to 1 and the stress exponent n is not =1 or =3 but somewhere between 1 and 2. These values give us the possibility to extrapolate and infer what the values could mean, rather than establishing a flow law. Our study highlights the necessity to perform more experiments to calculate more reliable parameters, while showing the meaning of those in terms of deformation mechanisms. The combination with the microstructures in the companion paper 1 of this study shows that the calculated values represent legitimate assumptions for deformational behavior of fine-grained polymineralic rocks supporting the interpretations of microstructures.

Further important comments we would like to address here:

- Approach to correction of localized strain data appears confusing:
  As in the localized strain experiments (Type I) it is uncertain, what is the area on which the force acts to be corrected for to calculate stresses. It is reasonable to assume that shearing reduces the overlap area, but at the same time the stress on the shear zone is most likely not reducing as fast as some barreling component seems to occur or/and resistance of the confining media. The comparison with the most reliable gouge shear experiments (Type III) with defined geometry prove that not applying any geometrical correction to localized samples is a good approach. We can make this comparison, because we compared the microstructures.
- Strength of our experimental framework:
  The disadvantage of relatively poor stress resolution of the solid medium apparatus can be compensated with the advantage of the solid medium apparatus that we are able to deform at strain rates that differ by orders of magnitude. Thereby we can define the slope in a stress vs strain rate diagram over 3 to 4 orders of magnitude different strain rates. The slope of several points is based on several rates within one experiment. This minimizes the error on the machine, as we can use the measurements relative to each

other, even though the absolute values overall might have an error of σ=/- 30MPa, as calculated by H&K2010.

- Citations and references:
  We highly appreciate the feedback on some of the references we cited, especially the factual mistakes and will change these in the revised manuscript. We will also incorporate a more detailed comparison with the very interesting study of Ghosh et al. 2022.
- Why do we not use the equivalent stress in coaxial geometry:
  The angle of the shear zone matters in terms of the stresses acting on the shear zone. If we calculate the stresses only as coaxial experiments, this geometry will be neglected as well as the shear strains.

Thank you once again for your insightful and constructive comments on our manuscript. We deeply appreciate the time and expertise you have invested in reviewing our work.

In light of your feedback, we will make several key revisions to enhance the clarity and robustness of our manuscript, particularly in addressing the quantitative nature of the stresses, and the use of stress correction methods.

We would like to emphasize that the fundamental outcomes of our study used in a semi-quantitative way remain valid and robust. The mechanical data can be demonstrated to be consistent with interpretations of microstructural features. While we recognize the importance of the issues the reviewers have raised, we believe that our primary results are valid but need to be expressed in a more qualified way expressing the limitations of the work better.

We are committed to refining our manuscript further and will address additional comments and suggestions you have provided. The review feedback has been invaluable in helping us improve the quality of our work.

Yours sincerely, Natalia Nevskaya (on behalf of all co-authors)