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Reply to discussion by G.I. Alsop and R.E. Holdsworth of "Sheath fold development with viscosity contrast: analogue experiments in bulk simple shear" by Marques et al., Journal of Structural Geology, 30, 1348–1353.

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Alsop and Holdsworth have done meritable work regarding the geometry of sheath folds. We value their discussion of our work, but we do not agree with their general conclusion that sheath fold geometry can be used to discriminate bulk strain type. We feel that deformation geometry is not unequivocally related to type of flow, and that it is therefore incorrect to make a priori assumptions about flow types in shear zones when investigating deformation geometries. Therefore, the conclusion of Alsop and Holdsworth that our experimental results support their findings is incorrect. What Marques et al., (2008) showed is that viscosity contrast, under bulk simple shear, can change the geometric features typical of passive simple shear, and therefore that geometry is not unequivocally related to type of flow.

In cross-section, typical sheath folds appear like ellipses nested inside one another, defined by successive layers. In simple shear, the ellipses should be typically eccentric, which means that thickness should vary between limbs when measured along z (reference frame of Alsop and Holdsworth, 2006) (e.g. Figs. 2 and 3 of Skjernaa, 1989; Mies, 1993). Only at very large strain should they become similarly thick. Ez (2000) called the attention that thickness difference between limbs could be a diagnostic feature of the strain type, because pure shear or constrictional deformation can produce similar limb thickness along z, in contrast to simple shear. Surprisingly, Alsop and Holdsworth (2006) do not use these thickness variations as potential criterion to distinguish between strain types. Mies (1993, Fig. 12) demonstrated that all three types can be produced by kinematic (passive) amplification of initial deflections in simple shear. All depends on shape and orientation of initial deflection. The way this problem has been addressed is (e.g. Cobbold and Quinquis, 1980; Skjernaa, 1989; Marques and Cobbold, 1995; Rosas et al., 2002): based on field observations and data, take a simple rheology (e.g. linear viscous) and subject a layered passive model (absence of rheological contrast among layers) to a desired flow type, both experimentally and theoretically. This has shown the unequivocal relation between sheath fold geometry and flow type for a given rheology and initial geometry, which has served as benchmark for further investigation. However, we think that Alsop and Holdsworth (2006) addressed this problem by putting the cart before the horse; they collected a large amount of observations and measurements, and then related sheath folds to "known" strain types in ductile shear zones. We think that there is insufficient ground in the literature to consider strain types in these examples to be established. As a result, Alsop and Holdsworth (2006) state that R' of sheath folds formed in simple shear is significantly less than 1 (R' ca. 0.69), while theory and experiment have demonstrated that in passive simple shear R' can be greater, equal to or less than 1.

Reply to specific comments (*in italic*)

1) Let us recall the original definition by Alsop and Holdsworth (2006) of R', in their abstract: "… *aspect ratios from outer to inner rings is defined as* R' (*where* $R' = Ryz/Ry'z'$)". When one says from… to, it means a range, not the extremes; hence they should be more careful when giving a definition to avoid misunderstandings. Besides, they do not even specify if it is relevant to measure ellipticity using the outermost or innermost surface of the ellipses. Note that in nature ellipses are not lines but layers, and layer thickness varies along the ellipses. Moreover, it has been shown theoretically and experimentally that a sheath fold born passively from a spherical cap under simple shear shows $R' = 1$, no matter what ellipses one compares. However, we were careful enough to keep consistency and measure always one or the other. Contrary to Alsop and Holdsworth's opinion, our measurements in Figs. 5 and 7 are absolutely correct: there are only two ellipses to measure, and we did it consistently by always measuring the inner surface to avoid undesirable effects of thickness variation.

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Therefore, the meaning of R' and measurements by Alsop and Holdsworth should be viewed with extreme caution.

2) This comment is no more than conjecture by Alsop and Holdsworth, because they do not have any idea of the actual fold evolution. In contrast, we were careful enough to carry out reference experiments that showed that a very similar pattern can be obtained in passive simple shear (Fig. 6 of Marques et al., 2008), hence without any interference or refolding, aiming to avoid this kind of misinterpretation.
3) "*Correct calculation of elliptical values indicates* $R' = 0.69$" – The value obtained by Marques et al. is 0.64 (Fig. 7) hence saying that the calculations are incorrect makes no sense.

"*The experiment of Marques et al. therefore strongly supports our interpretation that cats-eye-fold patterns* ($R' < 1$) *are indeed generated during simple shear deformation*." – This conclusion is incorrect, because rheological contrast was ignored in Alsop and Holdsworth (2006). It was strain type from geometry, which Marques et al. show it is not an unequivocal relationship. In fact what Marques et al. (2008) showed is the opposite, that one cannot extract strain type from geometry, because many variables influence the process. Marques et al. added a new variable.

4) As shown in point 1, Marques et al.'s measurements are absolutely correct, and consistent with all theoretical and experimental work on passive sheath folding. To say that "… *the typical cats-eye-fold patterns generated during experimental simple shear deformation once again support our original interpretation*" is unreasonable, and not in line with published theory and experimental data.
5) Instead of investigating why supposed simple shear does not produce $R' = 1$, one could also argue that flow was probably NOT simple shear.
6) The comment by Alsop and Holdsworth is in contrast to Alsop and Holdsworth (2006), where they conclude in their abstract that "… *empirical relationships* … *allow sheath folds to act as both effective* (>*95% consistent*) *and robust discriminators of bulk strain type*". Because many factors have a major influence on sheath fold geometry, their conclusion is not applicable.
7) Alsop and Holdsworth's comment is misleading, because viscosity contrast is rarely obvious and often difficult to prove. In Fig. 2 of Marques et al. (2008) the viscosity contrast is certainly not obvious; in fact, it seems the opposite.

"*In summary, natural sheath folds clearly do not display alternating Ryz values to coincide with the alternating multilayers*!" – It is not clear what the authors meant here: we did not test alternating layers of different viscosity (multilayers with more than two layers of contrasting viscosity), we tested two layers with contrasting viscosity to simplify a typically complex natural system and isolate the effects of one variable. This is a basic principle of physical experimentation.

The statement of the authors that "… *Marques et al.'s results are entirely consistent with our analysis of more than 1800 elliptical patterns from natural sheath folds*." is incorrect. On the contrary, we showed that flow type cannot be deduced from sheath fold geometry, because it depends on many factors, including viscosity contrast. What Marques et al. (2008) showed experimentally is that, if there is viscosity contrast (active folding), R' will not be equal to one, despite the applied bulk simple shear and the spherical shape of the precursor deflection (for which $R' = 1$ if passive folding). Hence there is no unequivocal relation between R' and flow type if there is viscosity contrast.

Alsop and Holdsworth (2006) and present discussion have expressed the idea that constriction can generate Bull's eye pattern, and that flattening across simple shear (their general shear) "… *increases ellipticity of eye-folds* (R_{yz}) *with greater component of pure shear*". Regarding flattening across simple shear, Jiang and Williams (1999) showed that in thinning shear zones sheath folds might not even form. Regarding constriction, Alsop and Holdsworth (2006) and present discussion have not explained why constriction should affect differentially the outer and the inner ellipses. Why is the inner ellipse more affected by constriction than the outer ellipse, if both are subjected to the same bulk strain type? This problem should be solved before jumping to conclusions about the effects of constriction on sheath fold geometry.

The theoretical works of Skjernaa (1989) and Mies (1993), and experimental of Cobbold and Quinquis (1980), Marques and Cobbold (1995) and Rosas et al. (2002) have demonstrated that the original shape and orientation of the layer deflection, by itself, can produce all the empirical relationships presented by Alsop and Holdsworth (2006) regarding ellipticities. Therefore, it is misleading to say that one can unequivocally determine strain type from these patterns, and that "*empirical relationships* … *allow sheath folds to act as both effective* (>*95% consistent*) *and robust discriminators of bulk strain type*". Alsop and Holdsworth could not show that there is an unequivocal relation between strain type and sheath fold geometry, therefore the meaning of R' and measurements by Alsop and Holdsworth should be viewed with extreme caution.

Finally, we would like to leave the following question: why do sheath folds observed in ductile shear zones show such a diversity of geometrical features? The search for the many possible answers to this question could take us much further in the understanding of the process of sheath fold formation.

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