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Comment on "Eye and sheath folds in turbidite convolute lamination: Aberystwyth Grits Group, Wales", by H.L.O. McClelland, N.H. Woodcock, C. Gladstone, Journal of Structural Geology 33 (2011) 1140–1147

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1. Discussion

Sheath folds are very important structures found in ductile shear zones and slumps, and therefore have deserved continued attention from structural geologists (e.g. Agar, 1988; Berthé and Brun, 1980; Brun and Merle, 1988; Carreras et al., 1977; Cobbold and Quinquis, 1980; Coward and Potts, 1983; Exner and Dabrowski, 2010; Ez, 2000; Gaudemer and Tapponnier, 1987; Henderson, 1981; Hibbard and Karig, 1987; Jiang and Williams, 1999; Malavieille, 1987; Mandal et al., 2009; Marcoux et al., 1987; Marques, 2009; Marques and Cobbold, 1995; Marques et al., 2008; Mies, 1993; Minnigh, 1979; Passchier et al., 2011; Rosas et al., 2001, 2002; Skjernaa, 1989; Talbot, 1979), to mention but a few as example of the great amount of work published on sheath folds. The paper by McClelland et al. (2011) raises a number of points that are already mentioned in these key publications, and hence need discussion.

2. Definition of sheath fold

The definition used by McClelland et al. ("*folds in which hinge lines curve more than 90° within their axial planes*", first sentence of the Introduction section) is attributed to Ramsay and Huber (1987). However, these authors define sheath fold as "*A fold with tight or isoclinal profile and which shows variations in its hinge line of more than 90°*." (p. 638, under the title "*Keywords and definitions*"). There is no reference to the fold axial plane in this definition. Because variations in hinge line of more than 90° can occur outside the axial surface (e.g. Type 2 interference pattern of Ramsay and Huber (1987)), Marques et al. in Journal of Structural Geology (2008, p. 1348, l. 13–16) added that the angle should be measured on a unique axial surface (see also Skjernaa (1989)), and completed the definition of sheath fold: "*A sheath fold can be unambiguously defined as a fold whose hinge is curved more than 90° within the axial surface*." The definition of sheath fold is thus purely geometric, without any genetic connotation (see also Skjernaa (1989)). The quotation therefore should be to Marques et al. (2008) rather than Ramsay and Huber (1987).

3. Dip of axial surfaces

Fig. 5b of McClelland et al. shows opposite dips of the axial surface of measured sheath folds. How is this compatible with the assumed downstream shear? In a simple shear dominated regime, how is it possible to have folds with opposite facing in the section normal to the vorticity axis (cf. Fig. 4), and a significant percentage of folds with axial surfaces close to vertical (Fig. 5b)?

4. Strain regime

In the Introduction section, McClelland et al. say that "*Two-dimensional sections through sheath folds show closed loops called eye*

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folds, the detailed geometry of which can be used to discriminate between simple-shear and general-shear strain regimes." McClelland et al. measured the mentioned geometrical details, in 2D sections, and plotted them in the graphs of their Fig. 10; surprisingly, however, they do not use it to discriminate the strain regime. Based on field evidence, on the works of Mies (1993) and Skjernaa (1989), and on the experimental work of Marques et al. (2008), Marques (2009) showed, beyond reasonable doubt, that the detailed geometry cannot be used to discriminate the strain regime.

5. Amount of shear strain

McClelland et al. say, in the first paragraph of the Introduction section, that: "*Since the experimental work of Cobbold and Quinquis (1980), sheath folds have mostly been interpreted as the product of high shear strain*." However, Cobbold and Quinquis (1980, p. 120, c. 2, l. 16–17) say exactly the opposite: "*In our experience with models of this kind, the formation of sheath folds is hard to avoid*.", which means that they form very easily from any non-cylindrical deflection on passive layers, at low values of shear strain (see also Fig. 7a of Marques et al. (2008); where small sheath folds, marked by arrows, form from small bubbles in the pink silicone layer). Authors prior to Cobbold and Quinquis (1980) could have thought that sheath folds are the product of high shear strain, not authors after that paper was published in 1980. McClelland et al. use this quotation to support their conclusion: "*These structures flag a warning to the universal interpretation of high shear strain from eye and sheath folds in sedimentary and metasedimentary rocks*." What do the authors mean by "*universal interpretation*"? Who said that sheath folds only form by high shear strain? However, Ramsay and Huber (1987, p. 619, l. 12–16) already warned that "… the structures of this geometry can be found in environments outside the shear zone terrain. Do not therefore regard the presence of sheath folds as automatically indicative of the proximity of a shear zone".

McClelland et al. say that "*The average shear strains involved are less than 1.0, lower than generally necessary to generate sheath and eye folds*." Such a statement implies knowledge/data that is not shown in the article, thus raising several critical questions: (1) In order to estimate shear strain, the authors measured the dip of axial planes in cross-section. Where were these sections located in the "sheath" fold? Because the studied sheath folds die out to the sides (normal to the sheath axis), it is critical to know where in the fold the dip of the axial surface was measured. A good example to illustrate this could be the two consecutive sheaths in Fig. 6: why is the axial surface of the sheath to the right of the image much steeper than the one to the left? (2) Are the observed folds actually sheath folds? No measurements are presented regarding the hinge angles necessary to characterize a sheath fold (or even if it is actually a sheath). Eye-like sections certainly do not tell us that the hinge variation angle is greater than 90°. The authors themselves recognize that it is an interpretation, not a measurement: "*Plan views of eroded convolutions support the interpretation that convolution crests and troughs (and their associated subjacent ripples) are highly non-cylindrical*." Quoting Skjernaa (1989) "*In spite of all the attention that has been given to sheath folds and tubular folds in recent years, the term sheath fold in particular has been used rather loosely, sometimes to cover almost everything that can give rise to a closed outcrop pattern*." When a definition is given with an angle (Ramsay and Huber, 1987), then the angle must be measured to ensure that the eye-like patterns correspond to actual sheath folds. (3) What is the meaning of "(value) *generally necessary to generate sheath and eye folds*"? What is this value? Who defined it?

McClelland et al. repeatedly use the following statement throughout the text (e.g. p. 1147, Conclusions section): "*The sheath geometries result instead from nucleation of convolution hinges on already sinuous or linguoid current ripples in the underlying part of the turbidite bed. Caution is needed in diagnosing high shear strains from eye folds*." However, measurements of hinge angle are not presented. Several workers have shown that the sheath fold shape depends on the geometry of the precursor deflection. The influence of the shape and orientation of the initial non-cylindrical deflection was investigated, and reported in Journal of Structural Geology, by e.g. Skjernaa (1989), Mies (1993) and Marques and Cobbold (1995), who have shown that those two variables could account for most of the observed geometric features of sheath folds. The work of Marques et al. (2008) re-enforces this idea, and further shows that rheological contrast can also account for some of the geometrical features observed in sheath folds. The geometrical model of Mies (1993) clearly shows that sheath folds can form before $\gamma = 1$ (cf. its Fig. 10). The experimental results of Marques and Cobbold (1995), in particular those in their Fig. 5, show that a sheath fold can be tubular after $\gamma = 3$ if the initial precursor is prolate and aligned with the greatest axis parallel to the shear direction. Therefore, a sheath fold can form well below $\gamma = 3$.

McClelland et al. further say that "*This value ($\gamma < 1$) is much lower than the values of 10 or more normally ascribed to sheath folds (e.g. Cobbold and Quinquis, 1980)*." This is a misquotation of Cobbold and Quinquis' work, because they do not say that $\gamma = 10$ is needed to form sheath folds. What they do is to show results at $\gamma = 10$, no more than that. Careful inspection of Fig. 4 of Cobbold and Quinquis (1980) shows that $\gamma = 10$ is enough to form highly stretched sheaths (actually tubular folds), therefore much less is needed to form a sheath fold. Many authors say that sheath folds are found in high strain shear zones, but this does not mean that high shear strain is absolutely needed to form sheath folds.

Based on the above, it seems that many of the findings presented in this paper have already been published in earlier work mentioned above. It would be helpful if the authors could point out how their findings differ from those of the authors mentioned here, which were not quoted in their paper, and to make clear why such quotation was apparently not necessary.

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