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Comment and reply

Reply to comment by S.H. Büttner on: "One kilometre-thick ultramylonite, Sierra de Quilmes, Sierras Pampeanas, NW Argentina"

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We thank S.H. Büttner for the opportunity to clarify our work and the evolution we proposed for the shear zones of Sierra de Quilmes. Büttner et al. (2005) was the first comprehensive study of the Tolombón complex. Their map, photographs, and petrography were helpful and although we cited their work more than any other, we apologise that S.H. Büttner felt the work was under referenced.

We have worked throughout the Tolombón complex (as first named in Toselli et al., 1978) covering much of the area described in Büttner et al. (2005) and far beyond, in numerous transects across the Sierra de Quilmes. This was the first of several papers that we aim to publish on the area and our intent was to present our data on the El Pichao shear zone (PSZ). Our next paper is on the high-grade Tolombón complex and makes full use of the comprehensive geochronology data set in Büttner et al. (2005).

The criticisms in Büttner (2015) can be summarised as: (1) we did not appropriately consider the effect of tilting, (2) we did not provide data to indicate that the PSZ formed at the same time as the other shear zones in the Tolombón complex, and (3) we did not provide evidence that the region he studied also underwent thrusting. We will deal with each of Büttner's (2015) concerns in turn below.

(1) Failure to appropriately consider the effect of tilting

We are fully aware of the possibility of tilting of blocks. We know that there are multiple shear zones and brittle faults regionally and throughout Sierra de Quilmes and they could have tilted individual blocks or the entire metamorphic complex. We have seen first-hand tilted Neogene and Quaternary volcanic layers towards the centre of Sierra de Quilmes and tilted (*ca.* 25°–30°) Cretaceous syn-rift sequences in the north sector of Sierra de Quilmes. We acknowledge the possible effect of tilting in Section 8.2. The important point is not whether the region was tilted because our interpretation that the region was contractional during the Famatinian orogeny is not based on the current geometry of the thrust planes but based on the fact that hot rocks have been placed on top of cooler rocks. The apparent contradiction Büttner (2015) refers to at the end of Section 2 is as follows "Büttner (2009) suggested that shear zones were originally horizontal and extensional, and later rotated by Andean uplift to show apparent thrust shear sense. This interpretation is not consistent with our findings …" It was not our intention to deny that there was any tilting, and we believe the intent behind our sentence is clear: that we find no support for the assertion in Büttner (2009) that shearing occurred during extension and that it is purely as a result of tilting that the shear zones now have an apparent thrust sense.

(2) Failure to provide data to indicate that the PSZ formed at the same time as other shear zones in the Tolombón complex

Büttner (2015) is quite correct that we have not constrained the absolute timing of motion on the PSZ. We have dated monazite from rocks of the PSZ, and obtained a mean age of ~480 Ma, similar to ages of migmatites of the Tolombón Complex, suggesting that monazites may not have been reset during shearing or that shearing was contemporaneous to migmatization. The ultramylonites formed at amphibolite facies (biotite and feldspar are stable) and were later locally retrogressed to greenschist facies with chloritized bands, and subsequently by cataclastic brittle deformation. Timing of retrogression and brittle reactivation remains unconstrained and could have been any time since the Famatinian.

Büttner (2015) suggests that the sheared pegmatites that we documented in the PSZ are type-2 pegmatites from Büttner et al.

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(2005), dated to 440 Ma and that therefore shearing took place after 440 Ma. Pegmatites in anatectic terranes are common and may form throughout the anatectic event. There is no reason to suggest that the pegmatites in the PSZ are the same age as the cross-cutting pegmatites from Büttner et al. (2005). Although we have no way of disproving this assertion it is perhaps notable that Büttner et al. (2005) describes type-2 pegmatites as most common in the garnet-cordierite-sillimanite and biotite-muscovite zones whereas the PSZ pegmatites can be traced into voluminous pegmatites within the less-sheared but high temperature orthopyroxene zone.

What we have been able to ascertain is that there is continuity in the kinematics of deformation that took place during partial melting of the Tolombón complex and deformation accommodated by the PSZ. Both accommodate motion of the hanging wall towards the west and place hot rocks on top of cooler rocks (compare Fig. 6 in Büttner, 2009 with stereonets in Fig. 2 of Finch et al., 2015). Our lack of a precise age of movement on the PSZ should not detract from the message that the Famatinian orogeny recorded in Sierra de Quilmes was a result of crustal shortening rather than crustal extension. We summarise the evidence for this below.

(3) Failure to provide evidence that the Divisadero region discussed in Büttner et al. (2005) and Büttner (2009) underwent thrusting.

In Finch et al. (2015) we contend that the shear zones of the Tolombón complex are part of the same shortening event that formed the PSZ. We came to that conclusion by combining the following observations:

a) W-directed movement in high-grade regions was contemporaneous with partial melting, to judge from the relationships between leucosomes and thrust planes and folds (Fig. 4d,e in Finch et al., 2015 and 4c in Büttner et al., 2005).
b) W-directed movement in high-grade and low-grade rocks alike in the Divisadero region is the same as in the mylonitic rocks of PSZ (compare Fig. 6 in Büttner, 2009 with stereonets in Fig. 2 of Finch et al., 2015). From (a) and (b) we conclude movement continuity through time and space and suggested that movement on the PSZ remained constant as it cooled.
c) Most significantly, we find in Divisadero and at the PSZ, thrust planes that place hotter rocks above colder rocks. We will publish a full description of the major Divisadero shear zone in our next paper.

In summary, we find no support for the interpretation of Büttner et al. (2005) and Büttner (2009) of an extensional terrane. The main argument for extension in Büttner (2009) is evidence for thinning of metamorphic packages in the migmatitic zones so that pressures recorded by the rocks are less than that indicated by the current distance between samples. Although thinned isograds are typical of extensional terranes, they alone should not be used as evidence for extension because the final distribution of isograds depends on the relative timing of paragenesis development and deformation. Furthermore, thinning of layers or metamorphic packages does not require extension. This may also occur during shortening when there is a component of shortening orthogonal or at a high angle to the isograds, bedding or foliation (pure or general shear). Numerous studies (Piñán-Llamas, 2007; Büttner, 2009) have determined that the Tolombón complex underwent general shear, as indicated by symmetric folding with axial surfaces parallel to the shear plane. Therefore, evidence for thinning in the Tolombón complex is not inconsistent with thrusting.

We hope this response provides clarity. Once again, we thank S.H. Büttner for the opportunity to clear up inconsistencies between his work and our own and we acknowledge the rich source of data and inspiration his papers have provided.

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