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Discussion

Reply to "A comment on early Jurassic palaeomagnetic study of lower Jurassic marine strata from the Neuquén basin, Argentina: A new Jurassic apparent polar wander path for South America"

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We acknowledge Taylor and Roperch for their interest in our paper and welcome their comment which provides us the opportunity to further clarify some aspects related to methodology and interpretation.

The scope of the paper by Iglesia Llanos and Riccardi (2000) was the construction of the first Jurassic magnetostratigraphic scale in South America and there demagnetisation paths were largely used to recognise magnetic polarities. Conversely, in the new study (Iglesia Llanos et al., 2006) remagnetisation circles were discarded for calculation of directions and the most favourable lithologies resampled to improve the overall quality of the data.

Nevertheless, the main point of disagreement with Taylor and Roperch is that they strongly reject the possibility that significant continental motion may have taken place in 15 My, which leads them to interpret our results in terms of vertical axis rotation at the Hettangian–Sinemurian localities. In contrast, we are convinced that the discrepancy between Hettangian–Sinemurian and Pliensbachian–Toarcian paleomagnetic directions in our study indicates continental drift. The departure of our Hettangian–Sinemurian palaeopole by up to 40° from other proposed paths (Besse and Courtillot, 2002; Schettino and Scotese, 2005) is most likely due primarily to the lack of reliable well-dated coeval poles. However, Taylor and Roperch seemed to have missed the fact that the Jurassic APW path for Eurasia that was constructed using poles determined by modern palaeomagnetic standards and having updated ages, supports our interpretation. The consistency with the South American data is such that as expected, show very similar latitudinal and rotational movements (Fig. 1, see also Iglesia Llanos et al., 2006).

The structure of the reply follows that of the comment.

1. Geological setting

Taylor and Roperch express concern about the description of the structural information in Iglesia Llanos et al. (2006). We consider the level of detail adequate for the purpose of our study. Indeed, it appeared to be sufficient for a thorough analysis by Taylor and Roperch. Moreover, the level of detail does not substantially differ

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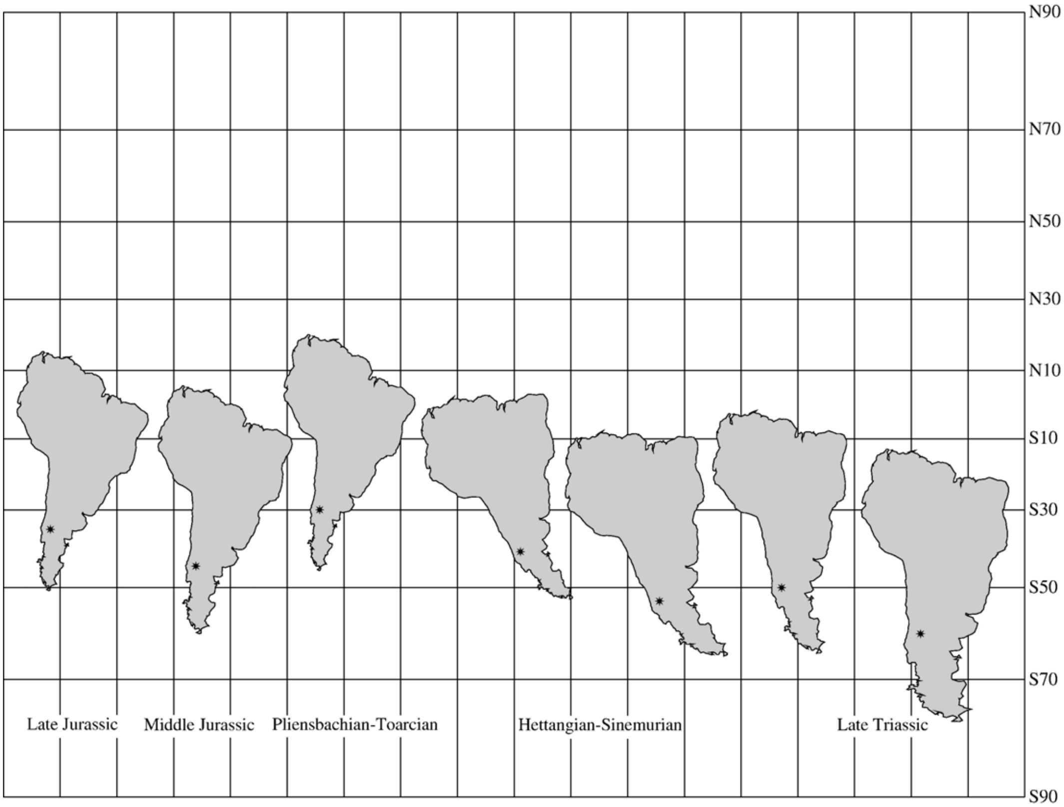

Fig. 1. Palaeolatitudes of South America recalculated using the palaeomagnetic poles of Eurasia from Iglesia Llanos et al. (2006). Rotational and palaeolatitudinal movements of the continent are fully consistent with those determined with the South American palaeopoles (in Fig. 14).

from that presented in other studies that use palaeomagnetism in relation with deformation (e.g. Roperch and Carlier, 1992; Roperch et al., 2000).

The difference of more than 50° in declination between AM-LC (Hettangian–Sinemurian) and PA (Pliensbachian) that Taylor and Roperch interpret as evidence for relative rotations in the area is, in our opinion, due to the different rock ages which we then interpret in terms of apparent polar wander. The palaeomagnetic pole involving data from the PA locality agrees with others of the same age obtained farther to the south in Rajapalo–Chacay Melehue (250 km) and in Pampa de Agnia Basin (730 km, pole 4, Fig. 12 in Iglesia Llanos et al., 2006). The PA (Pliensbachian–Toarcian) pole partly contains data from a former paleopole that was regarded by Taylor as cratonic (Randall et al., 2001). Given that the PA locality lies on top of AM-LC, the stated rotation of the older rocks would have occurred between the Sinemurian and the Pliensbachian. To our knowledge, there is no geological record of such tectonic event in the study area.

Taylor and Roperch suggest that the region where we have carried out the field work may have been affected by magmatism, metallogenesis and low grade metamorphism, and that, therefore, remagnetisation is likely to have occurred. We see, however, no reason to assume a priori that this has been the case. For instance, Roperch et al. (1997) have shown that Andean Upper Jurassic rocks in southern Chile located farther south from our localities, carry primary magnetisations and are not rotated, in spite of the fact that these Jurassic rocks crop out in a deformed zone that was in turn, intruded by the huge Patagonian Batholith during the Cretaceous.

2. Sampling and polarity

We would like to explain that the described separation of 10 m represents ALL sampling sites, including those that had been ruled out from our study for showing inconsistent palaeomagnetic behaviours and were thus not shown in Figs. 3–4/ Tables 1–2 of the original study. The multiple sites quoted by Taylor and Roperch actually correspond to a total of three distributed over two sills in Las Chilcas (Table 1) separated c. 20 cm from each other. It is possible that they do not represent independent measures of the palaeofield, yet when we average the three sites into a single VGP, the Hettangian–Sinemurian PP does not significantly change. The stated relationship between polarity bias and lithology is inconsistent since we clearly showed (e.g. Fig. 3 in Iglesia Llanos et al., 2006) that both volcanic (see AM10 and AM21) and sedimentary rocks carried normal and reverse polarities. In addition, we demonstrated through a contact test performed between a normal polarity sill and the reverse country rock (Fig. 8 in the original study) that the sills have only produced a partial remagnetisation.

3. Data analysis

To respond Taylor and Roperch on this particular issue, we performed a goodness-of-fit test of the Fisher distribution (Fisher et al., 1987) for individual localities. Accordingly, the population of directions from Puesto Araya do fit a fisherian distribution while those from Rajapalo–Chacay Melehue define a slightly elliptical distribution that does not change the calculated mean direction.

4. Interpretation

Unfortunately it was no possible to date the Hettangian–Sinemurian sills, yet we were able to assign a chronostratigraphic age. Thus, based on the fact that they are restricted to the synrift phases and are not found in the overlying sag phases in PA as well as on petrographical observations, we interpret that injection of these sills occurred soon after deposition of the section.

Taylor and Roperch are insistent on the fact that the fold tests we performed are not valid and that they need to be done differently. Although there is no reference of the fold test they used to combine palaeomagnetic data of different age (Hettangian–Sinemurian from the AM-LC localities and Pliensbachian from PA locality), their Fig. 2 clearly depicts the improvement of statistical parameters within each population upon bedding correction and the significant change of direction of Component B due to the difference in age. We would like to explain that Enkin's fold test (Enkin, 2003) was not used in our study as Taylor and Roperch suggest, because it is recommended as a rule of thumb, only when having sampled sufficient sites to obtain an α_{95} less than 1/6 of the bedding attitude difference (Enkin, 2003). This requirement does not apply to our data (see Tables 1-2 in the original study). Instead, we used the MacFadden's fold test (McFadden, 1990) whose only requirement is that magnetic directions were drawn from a population with a uniform azimuthal distribution about the mean. In addition, MacFadden's test gives equal weight to all sites when testing the correlation between the site mean directions and the bedding attitudes and is fit for bedding changes of less than 30°. Directions of Component B without decompaction correction do pass the fold test (minimum test value SCOS was at 100% of complete unfolding, with SCOS = 0.013 for 95% critical value = 5.7 and 99% critical value = 8.0). However, compaction in the Hettangian–Sinemurian section is so conspicuous from flat ammonites and sedimentary structures, that we thought it was highly unlikely to get reliable palaeofield directions without applying the inclination shallowing correction. Therefore, all Hettangian–Sinemurian site means from sedimentary rocks were corrected to perform all calculations, in agreement with other authors who have used the inclination shallowing corrections to improve their APW paths (e.g. Kent and Tauxe, 2005; Tan et al., 2007). For the conglomerate test, even though we did not describe single clast lithologies, we did follow the basic premise of sampling an intraformational conglomerate (Llambias et al., 2007) whose clasts have been derived from the underlying sampled rocks.

We explained in the original study that in reverse polarity sites such as the basaltic flows in Rajapalo at c. 60 m from the base (in Fig. 4) we got the best palaeomagnetic behaviours (see Fig. 9) and used their directions as reference for Component B. Nevertheless, if we leave out the reverse polarity sites as Taylor and Roperch recommend the pole positions do not change (Fig. 2). This also supports that Component B represents the original Jurassic palaeofield while the A component is a remagnetisation acquired most likely during recent times.

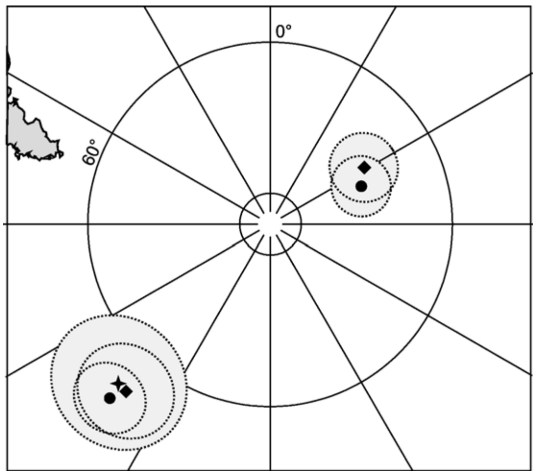

Fig. 2. Detail of the Hettangian–Sinemurian (223°E, 51°S) and Pliensbachian–Toarcian (67°E, 74°S) palaeomagnetic poles calculated using all sites (circle), only normal polarity sites (diamond) and only the sills (star).

5. Discussion

Taylor and Roperch do not accept rapid continental drift in c. 15 My and attribute the change in polar positions to rotation of the sampled localities. Although we are aware that rotation is a possibility, we are convinced that the different palaeomagnetic directions we obtained reflect different age of acquisition and therefore we interpret our data in terms of polar wander — either apparent or true.

The rotation and latitudinal shifts that South America would have undergone during the lowermost Jurassic are also observed using the Eurasian poles (Fig. 1), which are certainly far more reliable than those from Africa. Yet, one additional indication of such latitudinal shifts in our opinion, is provided by independent biogeographical data. Although maximum velocities for a continent is matter of debate (see e.g. Meert and Tamrat, 2004), there are reports of continental minimum velocities as high as 24 cm y^{-1} for Laurentia and Gondwana (Meert and Van der Voo, 1997) and 26 cm y^{-1} for the Brasilian Craton (Schettino and Scotese, 2005) which are higher than the 20 cm y^{-1} we calculated for the Sinemurian–Pliensbachian.

Finally, we know Taylor and Roperch have carried out and intensive palaeomagnetic work in the Andes. Precisely thanks to this sort of studies, it is well known that there are large areas of the Central Andes that were NOT affected by block rotations (e.g. Irwin et al., 1987; Randall et al., 1996; Roperch et al., 1997; Arriagada et al., 2003), as could have occurred in our sampled localities.

Taylor and Roperch gave us with their comment the chance to further explain very important issues of our study, among which the most significant is the departure of the Hettangian–Sinemurian pole from the usually assumed path. As already stated, our data indicate that the cause is apparent or true polar wander, although however we do not deny that new and improved data could provide an alternative explanation.

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