Reply by the authors to 'Comments on "Palaeomagnetism of the Lisbon Volcanics"' by R. Van der Voo

N. D. Watkins and A. Richardson

We do not accept Van der Voo's criticism of our conclusion that palaeomagnetic data from the Lisbon Volcanics can be explained by a rotation of the Iberian peninsula continuing after the Eocene, although as stressed in our paper we most certainly acknowledge that the conclusion must be tested further.

Van der Voo makes the following four points, which we shall reply to in corresponding order:

1. Contrary to Watkins & Richardson's statement, the Lisbon Volcanics are not flat-lying and undisturbed. Comparison of results shows that by ignoring the slight eastward or southeastward dip of lavas, an error of up to 7° is induced into the resulting palaeomagnetic declination, providing misleading evidence of rotation when compared with the stable European Eocene palaeomagnetic pole position.

Reply. Although we re-state our observation that the lavas are undisturbed (since there is no definite evidence of significant faulting or folding) we acknowledge the fact that the Lisbon Volcanics are not flat, if 'flat' is understood to mean 0° dip.

We have used the term 'flat-lying' to be indicative of a ±5° dip which is not creating a geological precedent. We in fact detected a 5° southeastward dip on the enclosing sediments exposed in roadcuts of the area north of Lisbon, and corrected for this in the presentation of our data. In our opinion no other local tectonic dips close to the sampled bodies were definitely proven to exist, except perhaps near lava number 11 (Fig. 2 of our paper) where a partially columnar body dipped 45° westwards: this was not sampled at this locality because of uncertainties about the origin of its outcrop configuration.

We would point out that Van der Voo is being selective in citing the area's Jurassic to Miocene (west to east) outcrop sequence as proof of uniform eastward regional dip, since he could as well describe the Miocene to Cretaceous (west to east) outcrop sequence near Lisbon, or the Miocene to Jurassic (west to east) sequence west of Alverca.

In any case, even if our mean palaeomagnetic direction for the Lisbon volcanics is further rotated by an amount corresponding to an additional hypothetical 10° eastward dip of the lavas, the palaeomagnetic declination is still west of geographic north, west of the stable European Eocene palaeomagnetic pole position, and therefore not in conflict with post-Eocene rotation of the Iberian peninsula. Van der Voo essentially echoes this possibility by stating that such rotation may have been 'nearly completed' by Eocene times.

2. To support his criticism, Van der Voo presents data (his Table I) from six lavas of the Lisbon volcanics, four of which were originally sampled by Watkins & Richardson. He reports a similarity in magnetic stability but more significantly shows a resulting palaeomagnetic declination (Fig. 5) which is closer to geographic north than that reported by Watkins & Richardson.
Reply. Because of the critical requirement for data accuracy in this particular work, we feel obliged to criticize Van der Voo's experimental and analytical methods.

Since Van der Voo has corrected all data for the present local magnetic declination of 9.5°W, he has clearly employed magnetic orientation methods in the field. Inspection of the field notes involved in the geographic orientation of our collection shows, however, that the local magnetic declination at the sampled lavas are (not surprisingly) large, varying from 1° to 17°W, with one observation of 10°E. Even the within-lava declination varies by up to 18°. This illustrates the danger of magnetic orientation methods, which can obviously contribute to misleading results, particularly with limited collections as used by Van der Voo. The use of a magnetic field orientation method makes the resulting rather high Fisher precision parameter ($k$) of 55 obtained by Van der Voo difficult to visualize.

In the data processing, Van der Voo has rejected results from five specimens out of his total of 22. No details are given concerning the method of selection of the final data points. More significant, however, is the method Van der Voo employs to compute the obviously critical mean direction of remanent magnetism shown in his Fig. 5. There is clearly no point in comparing results when different computational methods are used. We shall argue that Van der Voo's computational procedure is wrong. In using unit vector per specimen for the 17 retained specimens to produce the result indicated in Fig. 5, Van der Voo violates the clear fact that only each lava can represent a single point (or unit vector) in time. In fact, he actually computes the average direction of remanent magnetism in each lava, as presented for comparison with our results in Table 1. We have analysed Van der Voo's data, applying unit vector to each of his six lavas with the following results (figures in brackets are our results, obtained by applying unit vector to each of our twelve sampled lavas):

$$D=351.8° (346.7°); I=+40.5° (+37.2°); x_{95}=8.3 (11.2).$$

Considering the fact that we sampled twice as many lavas, the agreement is good, being statistically indistinguishable. As will be discussed below, however, if the comparison is restricted to only those actual lavas sampled by both groups, the agreement in results is even more pronounced.

Although a practice not unknown in the literature, the treatment of each specimen as a point in time is, by any meaningful model, quite invalid. Recomputation of our data applying unit vector to each of our 39 specimens results in palaeodeclinations varying from 339° to 347°, depending on the demagnetizing field used to treat all specimens. Van der Voo's high data precision is also correspondingly misleading, resulting as it does from 17 rather than 6 unit vectors, although the role of rotation of lavas to suspected original attitudes has contributed to the apparent data precision.

To conclude replying to this particular point, we must say that we are quite puzzled by Van der Voo's purpose when examining his Table 1 and Fig. 4: in the four lavas which by chance were sampled by both groups, the declination differences between the two sets of results are +2°, -3°, +6°, and -5°, meaning that on average for each group the mean palaeomagnetic declinations for each lava (not specimen) are identical! Such agreement by two independent groups is in itself quite noteworthy. At this stage we feel it reasonable to question what Van der Voo is arguing about: this most unusual agreement in results must surely convince him that we also corrected for tectonic dip. Why does he compute mean directions using each lava in one case, but use each specimen for calculating the mean direction for his entire collection in the other case?

3. Additional evidence is presented by Van der Voo for a lack of post-Eocene rotation of the Iberian peninsula, in the form of palaeomagnetic results from the
Sintra intrusive complex near Lisbon, and the Southern Portugal monchique intrusive complex, both of which provide virtually zero declination.

**Reply.** We await published field and computational details of those surveys with interest, but would point out that locally restricted intrusive complexes may only rarely provide meaningful palaeomagnetic results when palaeomagnetic pole positions are involved, because of the frequently very limited time range of the bodies, which leads to a lack of confidence in the amount of cancellation of non-dipole effects. If the intrusive materials are coarsely crystalline, definition of the remanent magnetism directions may have presented difficulties because of magnetic instability, which is not uncommon in intrusive rocks. Palaeomagnetic data representing only a limited time span cannot generally be considered relevant to the type of tectonic problem under discussion. We would also mention that Van der Voo's result from the Lisbon Volcanics, which involves only six separate bodies (perhaps less, depending on the distribution to the five rejected specimens), cannot be considered to be ideally suited to cancellation of non-axial dipole components.

4. Van der Voo states that the Cretaceous occurrences in the Bay of Biscay observed by Jones & Funnell, as mentioned by us in a note added to the proof of our paper, are additional evidence of any Iberian peninsula rotation being completed by Eocene times.

**Reply.** As clearly explained in the note added in proof to our paper, Cretaceous occurrences in the Bay of Biscay, as described by Jones & Funnell, do not provide additional evidence of a lack of post-Eocene rotation of the Iberian peninsula, any more than the occurrence of Cretaceous sediments in the Atlantic would provide evidence against post-Eocene crustal-spreading in the Atlantic. The occurrences may provide limits to possible post-Cretaceous rotation, but the distribution of such sediments must be defined prior to any more definitive statement being made. If any part of the Bay of Biscay does not have Cretaceous or earlier sedimentary cover then a post-Cretaceous opening cannot be excluded.

In summary, although acknowledging our neglect of the description (if not the computational consideration) of local geologic dips up to 5° we do not accept Van der Voo's criticism of our interpretation of our data, because of the fact that his supporting arguments are largely based on palaeomagnetic data which are too limited in volume, and which are based on field and computational methods totally unacceptable to us. Again, this does not mean that we absolutely rule out the possibility that post-Eocene rotation has not occurred, since the method as applied is close to the limits of its resolving power. We believe that our data, being based on a more extensive collection and a much more meaningful computational method than described by Van der Voo, do not rule out post-Eocene rotation of the Iberian peninsula. The final answer to this problem lies most probably in the marine geology and geophysics of the Bay of Biscay.

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