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eEarth Discussions

Interactive comment on "Palaeomagnetic investigations of sediments cores from Axios zone (N. Greece): implications of low inclinations in the Aegean" *by* E. Aidona et al.

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Comments on the paper "Palaeomagnetic investigations of sediments cores from Axios zone (N. Greece): implications of low inclinations in the Aegean" by Aidona et al.

This paper is interesting and deserves publications. I have no fundamental criticisms about the main topic of the paper (low inclinations), but a lot of points need strong improvements. Authors have in particular to be much more rigorous and precise in their presentation, selection and interpretation of the results. I recommend therefore a moderate revision of the paper.

The main problem in the paper is that a lot of informations are missing, incomplete or

not clearly presented. Some possible significant factors have even been forgotten.

The first remark concerns the origin of the remanent magnetization. Is the latter a primary magnetization or a remagnetization? In some parts of the text, allusion to remagnetization for other rocks or for Epanomi samples are made, but in other part, authors compare obtained inclination of part of their results with inclination, previously obtained on outcrops sites and interpreted as a primary magnetization. A surprising result here is however that all the data present only normal polarity, suggesting rather a widespread remagnetization. That will not change significantly the discussion about the low inclination problem, but has to be clearly discussed.

A forgotten fact is that deep boreholes are rarely perfectly vertical. Deviations of some degrees are very frequent (authors can ask the people of the borehole for that). That, added with the lack of dip value for most of the samples, could have non-negligible implications for the inclination values. Another neglected factor is that boreholes were not drilled with amagnetic tools, and that parasitic magnetization can have been acquired during drilling. Directions with high inclinations shown on figure 8 could have another origin than a simple viscous overprint...

Another lack concerns the uncertainty related to measurement and determination for part of the data. Nothing is given about the uncertainty associated with the viscous component and with the AMS data (if AMS has been measured with KLY3 - used equipment is not indicated -, α95 values have been obtained from measurement, and that should be moreover a good criterion of data selection - more suitable than that about Kmin inclination, used by the authors, which is not far from a circular reasoning).

Results about magnetization analysis are very badly presented. Figure 3 is of very low quality and insufficient (see minor comments). According to figure 3b, there is effectively a high coercivity component, but it remains unknown (no actual demagnetization for field higher than about 60 mT). Is it the component indicated p 42 l 10? Authors are also writing about a low temperature component, but up to 450°C. Is it that they call

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later the viscous component (but 450°C is much too high for a viscous component)? If not, how was isolated the viscous component? Results of the demagnetization have to be much more clearly presented (which components have been isolated and for which temperature or field spectra) and illustrated.

Concerning the viscous component, I am not sure that the results from the second storage center are better than from the first one. Inclinations are far from the horizontal, but also not so close to that of the present field... Without complementary information (see above), it is difficult to have an indisputable interpretation. For example, owing to their high inclination, some retained directions of the figure 8 could have a totally indeterminate declination if their $\alpha 95$ is not very small. Even if we assume that all components of the figure 8 (assuming also that they are only of viscous origin) and used AMS data are perfectly determined, the scattering on figure 10b is high enough to have confidence zone including a northward direction. Conclusion about the rotation of this area is therefore a little bit optimistic and weakens the paper. In addition, authors themselves kept this mean direction for the declination, but rejected it for inclination.

Large uncertainty is for another hand not always here a used criterion of rejection, since 3 of the accepted final directions (table 2) correspond to uncertainties on inclination of 39.8°, 40.4° and 46.8°!

Minor Comments

- Angles are in degrees (unit often missing in text and table).

- P 38 L 6: demagnetization here does not reveal here the presence of magnetite. Results are compatible with the presence of magnetite. This presence is shown by the rock-magnetism data (it should be then more logical to put the rock-magnetism parts before the paleomagnetic sections in the text).

- P 39 L 3: during the Cenozoic? For the Cenozoic formations?

- P 41 L 3 and table 1: Massic χ or volumic susceptibility K? Same question for the

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scale of the figure 5 - if volumic, how measured (powder)?

- P 41 L 7: negative susceptibilities are not due to paramagnetics, but to diamagnetics!

- P 41 L 21: Kappabridge alone does not do that. In which atmosphere (Air or Argon)?

- P 43 L 1-2: Thermomagnetic analyses with Kappabridge and CS attachment do not indicate maximum blocking temperature, but susceptibility variation!

- P 43 L 10-11: “Presence of pyrite attributed to low temperature...” incomplete sentence.

- P 44 L 1: VRM is not destructed. A VRM still exists but it is not the same as before drilling.

- P 45 § 1: corresponding number of samples?

- P 46 L 2-3: the paper of Westphal et al., 1986 is based on old data, often not obtained with the present paleomagnetic criteria, and is a little bit outdated. To be deleted.

- P 47 L 26: this old alternative pole was never confirmed. Probably, it should be better to forget it.

- Two references of the references list are not called in the text.

- Table 2: “Cone” of confidence for only inclination data? Decimals are not significant for k values. Give the corresponding ages (cf P 45 § 1).

- Table 3: incomplete caption.

- Figure 1: add at least the main geological limits.

- Figure 2: bad choice of the filling. As presented here, that implies complicated geological evolution with erosions and discordances. Filling pattern has to follow the bedding.

- Figure 3: hardly readable and incomplete. It has to be redrawn with higher size and

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readable text and numbers. Samples names have to be clearly indicated. It should be good to have 2 more examples corresponding to twin-specimens analyzed by thermal and AF methods. On the Zijderveld plot, numbers corresponding to treatment are often hidden by lines (I suggest to delete these numbers, to put directly some marker values on the figure and to delete the text below). Curves are indicated in stratigraphic coordinates, but dip is mainly unknown here...? For the demagnetization curves, scale has to be given with values of field or temperature, and not with proportion of the maximum value. Add also on this diagram the susceptibility variation (K/Kmax) for the heated samples.

- Figure 5: we can see a small peak on the heating curve around 450-500°C. Is it Hopkinson peak or beginning of transformation? This can be determined with a new curve for maximum temperature of 500°C. Caption: transformation does not occur (at least mainly) during the cooling.

- Figure 7: specify in the caption that geographic coordinates correspond only to the present field direction (other data are in specimen coordinates).

- Figure 8: again specimens coordinates (delete “N”). Projection type?

- Figure 9: projection in which hemisphere? Which coordinates?

- Figure 10: projection type? Specify the coordinates? Figure b seems to be in stratigraphic coordinates and “N” is then missing.

- Figure 11: orientation of the cross-section? Presentation of the a95 is strange and misleading (it is not around the mean value).

Interactive comment on eEarth Discuss., 2, 37, 2007.