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Palaeomagnetic constraints for Variscan mobilism of the Upper Silesian and Małopolska Massifs, southern Poland — reply

GENERAL REMARKS

In his earlier papers, J. Nawrocki (1993*a, b*) has argued that Upper Silesian Block (USB, southern Poland) was an integral part of the Old Red Continent (ORC[1]) in the Middle Devonian, based on the position of palaeomagnetic pole *D* obtained from the Eifelian/Givetian dolomites of the USB. I have shown (M. Lewandowski, 1994) that this concept is doubtful, since it leads to a confusing ORC Middle Devonian palaeogeography, in which carbonate oolite of the Laurentian craton had to be deposited at too high palaeolatitudes.

Accounting for the ca. 70 Ma time error associated with the palaeopole *D* and aiming to solve overmentioned palaeogeographic paradox, I have reinterpreted palaeomagnetic data by J. Nawrocki (1993*a, b*) accordingly. A newly created general seenario of the Variscan mobilism of USB, involving dextral strike-slip block translations and/or intra-block vertical-axis rotations is an original approach (not just a comment, as suggested by J. Nawrocki) that does not require elimination of the existing palaeomagnetic data and it is not handicaped by the drawbacks of the former interpretations (J. Nawrocki, 1992*a, b*, see M. Lewandowski, 1993, p. 56).

In the comment, kindly put forward by J. Nawrocki (1995, p. 272–282), he rejects my interpretation and concludes: "None of arguments presented by M. Lewandowski (1994) imply the necessity of mobilistic interpretation...". As a matter of fact, I did not imply necessity but just possibility, which makes some difference. Since, however, no arguments of mine have been regarded important by J. Nawrocki, there is nothing to do but leave his comment without reply, as it looks that we practice different palaeomagnetology, indeed. This paper is therefore intended as a necessary supplement to J. Nawrocki's comment, consisting the merits unspoken by my opponent. Since not all remarks by J. Nawrocki require broader discussion, I will refer to some of them only briefly. The more important points I will discuss in the later sections.

1. **Time error problem**. In his comment J. Nawrocki supported my estimation in this respect. One should just to note that, although the source data are still the same, the age of the remanence *D* is gradually getting older in the successive papers by J. Nawrocki (from Givetian/Frasnian through Givetian down to Eifelian — cf. J. Nawrocki, 1993*a, b* and this volume, respectively).

[1] Understood as Baltica and Laurentia in the fit of Bullard and his colleagues.

2. **Precision of the palaeomagnetic record.** With respect to the precision parameter *k*, I would like to make it clear that I was interpreting the *k* value on the sample level (ca. 77) — quite adequate to the problem, as it averages the laboratory errors. With respect to the spatial variability of mineralization, I am a bit surprised by J. Nawrocki's reluctance to acknowledge such a possibility. In my experience, composition and distribution of magnetic carriers can vary within one sample, not to mention over a distance of several hundred metres.

3. **Reliability of the direction *C***, obtained from the Namurian/Westphalian clastics of the Upper Silesian Coal Basin (USCB, an integral part of the USB). Given a negative polarity test for direction *C* (information that appeared later than my paper of 1994), it's likely that the sediments hosting this remanence may be handicapped by an unresolved component, so they should be reinvestigated. It should be noted that the problem of the reality of the *C* direction is relevant to the mobilistic scenario presented by J. Nawrocki (1992*a, b*) for USB but has no meaning for the mid-Palaeozoic rotations of the Małopolska Block (M. Lewandowski, 1991, 1993), since the latter was an earlier event.

4. **The Late Carboniferous palaeogeography**, depicted in Figure 3 can hardly be eommented on for it is a specific mixture of different ideas on the consecutive stages of the Variscan orogeny. With respect to Armorican unit, it is portrayal of the one of two of scenarios presented by J. B. Edel and M. Lewandowski (1993). Suggested by J. Nawrocki independent northward drift of Armorican bloeks in the Stephanian-Early Permian time, allegedly postulated by J. B. Edel and M. Lewandowski (1993), is a myth resulted from the misunderstanding of the text, since we have underlined that the youngest palaeomagnetic overprint (*A*), observed along the Variscan belt of Europe, is compatible with the late Carboniferous-Early Permian poles of Baltica, indicating common northward movement of the blocks under discussion.

THE ORIGIN OF NRM

Judging from the comment it seems that J. Nawrocki does not perceive that in my paper I have not excluded the possibility that pole *D* from Siewierz is of Eifelian/Givetian age. Nonetheless, J. Nawrocki questions my suggestion on diagenetic, secondary origin of magnetite in Siewierz dolostones and maintains, without evidence, that the remanence is primary.

To support his view J. Nawrocki cites Z. Bełka (1993) to indicate that the dolostones from Siewierz area were never been heated in the geological past. First of all, dolostones from the Mała Wioska and Podleśna quarries are not on the list of exposures investigated by Z. Bełka (1993). From the map by Z. Bełka (1993, his Fig. 8) it is obvious that the dolostones from Siewierz are situated in the area where the conodont alteration index (CAI) varies from 1 to 5. The closest to the formation in question are the Givetian rocks encountered in borehole S-43, which has yielded CAI = 2 (see Z. Bełka, 1993, his Table in the Appendix). Fluid inclusion studies of ore-stage minerals reveal that ores of Mississippi Valley-type were commonly formed in the 70–140°C range (D. T. A. Symons, D. F. Sangster, 1994, although temperatures 200°C were also encountered, see H. Pan *et al.*, 1993)

that corresponds to CAI values of 1–2 (max. 3, respectively). Hence, a thermochemical overprint in the Siewierz dolostones cannot be excluded.

Remagnetization due to migration of fluids during orogeny is not a single, time and space restricted event as suggested by J. Nawrocki. It has been proven that orogeny-induced remagnetization is a general process that has affected many Phanerozoic foreland sediments on the North American craton. It was repeated several times, from the Acadian orogeny (e.g. Newfoundland zinc deposits, H. Pan, D. T. A. Symons, 1993) through Ellesmerian (D. T. A. Symons, D. F. Sangster, 1992), then post-Middle Mississippian/pre-Late Pennsylvanian (H. Pan *et al.*, 1993), subsequent stages of Alleghenian (D. Suk *et al.*, 1993*a, b*; D. T. A. Symons, D. F. Sangster, 1994), up to Laramide orogeny (e.g. Pine Points Zn-Pb deposits, D. T. A. Symons *et al.*, 1993). Palaeozoic formations of the USB were in a similar geotectonic position relative to the Variscan front as their North American counterparts were with respect to the Alleghenian orogenic belt, hence they might have experienced remagnetization associated with orogeny-driven fluids. Spatial variability of mineralization and associated magnetization is quite expectable, bearing in mind the multiplicity of factors that control these processes. Formations that experienced several events of remagnetization may carry a composite or simple magnetization, the latter being an effect of a total overprint. Remanence *D* could be, therefore, obliterated by a younger overprint on the areas farther south of the Siewierz platform.

THE AGE OF MAGNETIZATION

J. Nawrocki argues for primary origin of characteristic natural remanent magnetization (NRM) in dolostones of the Siewierz area, relying on the similarity of his pole *D* with the one Middle Devonian pole from Eifelian ophiolites of the southern Mugodzhar region (South Urals). Note that in the paper to which I have most referred (J. Nawrocki, 1993*a*), the age of remanence has been determined as Givetian-Frasnian, hence J. Nawrocki's complaint that I have used the poles from the Upper Devonian of the ORC for comparison is not justifiable. By the lowering of the age of remanence *D* down to the Eifelian, J. Nawrocki has not achieved very much since the southern Laurentia was not situated under latitude of 50°S in this time, too (cf. C. R. Scotese, W. S. McKerrow, 1990; P. A. Ziegler, 1989; R. Van der Voo, 1993).

My opponent regards the pole from the Middle Devonian ophiolites of the Mugodzhar region (South Urals) as the key reference pole for Baltica and points out that I have used this pole for constructing my apparent polar wander path (APWP) for this palaeocontinent. (I did it following the M. A. Smethurst and A. N. Khramov (1992) data compilation, but I have not used this particular pole to draw any geotectonic conclusion for they note that the pole is of "...limited reliability and should be interpreted with caution."). On the other hand, J. Nawrocki regards the APWP by T. H. Torsvik *et al.* (1993) as the most reliable one (see my remarks in M. Lewandowski, 1994). However, the most reliable APWP does not include J. Nawrocki's key pole, as T. H. Torsvik *et al.* (1992, 1993) did not include the Middle Devonian ophiolites from the South Urals into Baltica.

Having read(?) the paper by A. N. Didenko and D. M. Pechersky (1989) J. Nawrocki has come to the conclusion that these authors have changed their previous opinion about

tectonic mobilism of the Mugodzhar ophiolite suite. In fact, from the text by A. N. Didenko and D. M. Pechersky (1989) we can easily learn that the pole under discussion represents only the eastern part of the ophiolite suite whose magnetization is similar to the pole *DII*, reported by A. N. Khramov (1982) as characteristic for the Silurian/Devonian (not Eifelian!) of the Russian Platform. By referring to the paper by A. N. Didenko and D. M. Pechersky (1989), therefore, J. Nawrocki indirectly confirms that his pole *D* overlaps exactly the pre-Eifelian poles. It is worth mentioning that magnetization of the western part of the ophiolite is different, although equally well defined as the eastern one, which suggests that some, yet unrecognized, tectonic correction is required to obtain characteristic, overall mean formation magnetization.

A. N. Didenko and D. M. Pechersky (1989) were certainly aware of this problem, since they write as follows: "The palaeomagnetic inclinations are similar except one (Tab. 7) but palaeomagnetic declinations are significantly different, which can be accounted for in the most logical manner by block rotations about a vertical axis. This is in agreement with the geological situation (Fig. 1)." and later: "Prior to the Permian (the time of post-folding remagnetization), the allochton (i.e. ophiolite complex — M. L.) was thrust upon the continent, producing appreciable deformation and block rotations." The same text (in Russian) the reader may find in the paper by K. S. Burakov *et al.* (1984). Why, therefore, J. Nawrocki has understood the view of A. N. Didenko and D. M. Pechersky as opposite to the previously formulated one and why he considered the Mugodzhar Block stable with respect to Baltica, I can not really guess. Is this an example of the "discerning analysis", as J. Nawrocki calls his own research in the Polish summary?

For a more detailed geotectonic story of the Mugodzhar region, comprising the Late Eifelian-Early Givetian formation of oceanic crust, subsequent oblique convergence and collision in Late Devonian-Early Carboniferous time, accomplished eventually by a subsequent large-scale thrusting due to collision of the Kazakhstan Plate with the East-European eastern continental margin, the reader is kindly referred to the paper by L. P. Zonenshain *et al.* (1990). This should dispell J. Nawrocki's reservation to the post-emplacement mobilism of the ophiolite of the South Urals.

J. Nawrocki certainly overestimates the meaning of the high quality index *Q*. R. Van der Voo (1990, 1993) indicated that, for a given time interval, palaeopoles of *Q* higher than value of 2 display the same scatter with respect to the overall mean. The reason for rejection the "highest quality index" approach in a construction of APWP was too few entries, which led to a starkly underdetermined APWP (R. Van der Voo, 1990). Moreover, there is no doubt that some of the highest quality poles represent secondary magnetization. Paradoxically, evidence for this also comes from J. Nawrocki, who states that good quality Devonian poles (namely four he has chosen) from North America are very dispersed! This gives J. Nawrocki reason to call the Devonian part of the North American APWP artificial, leaving without answer the question as to how the most reliable poles could produce an artificial APWP. I cannot understand why the Devonian APWP based on the two highest quality poles for Baltica, is superior to the coeval APWP constructed for 43 palaeopoles averaged by time intervals for the ORC (see R. Van der Voo, 1993). This is the critical question, as the former path introduces discord between ORC facies distribution and palaeoclimatic zonation in the Devonian, while the latter does not.

It is well known that relying only on single poles (whatever quality) may lead to the palaeogeographic nonsense. This has happened to J. Nawrocki, since his Figure 1 implies, a significant overlap of Laurentia onto Baltica (Laurentian poles lie too far to the south-west when compared to the APWP for Baltica in Bullard's reconstruction of the ORC).

CONFIGURATION OF THE OLD RED CONTINENT IN THE MIDDLE DEVONIAN

In the comment on my paper J. Nawrocki argues against my data and interpretations although the proper targets for the critics should be the reconstructions by C. R. Scotese and W. S. McKerrow (1990), P. A. Ziegler (1989), R. Van der Voo (1993) and others, who show Laurentia, incorporated into the ORC, straddling the equator in Middle Devonian time. To weaken my palaeogeographic arguments, J. Nawrocki recalls the idea of large-scale sinistral offset along the Great Glen Fault (R. Van der Voo, C. R. Scotese 1981) — disproved long ago (T. H. Torsvik, 1984, 1985*a*, *b*; J. C. Briden *et al.*, 1984) and perhaps forgotten by the creators.

J. Nawrocki points out (after B. J. Witzke, 1990) that carbonate oolite might be deposited up to 45° of latitude, not mentioning that this concerns one site only. The reconstruction of the ORC according to his pole *D* (M. Lewandowski, 1994, his Fig. 4) requires that the bulk of carbonate oolite of Middle Devonian age of Laurentia has to be situated at a palaeolatitude of 40–50°S, while B. J. Witzke (1990) places them within the range of 5–25°S. C. R. Scotese and S. F. Barrett (1990) calculate that carbonates have maximum likelihood of occurring between 15 and 25° latitude, giving only a 3% chance for development of carbonates between a latitude of 45°and the geographic pole. Consequently, there are the same chances for J. Nawrocki's concept that the final welding of the USB with the ORC had already taken place in the Middle Devonian. With respect to Gondwana, some part of this palaeocontinent could really be at distinctly higher latitudes, but this certainly does not apply to its carbonate shelf; given the size of this megacontinent, Gondwana must have included polar environments at the geographic pole at the same time as carbonate facies in the tropics.

By the way, I am indebted to J. Nawrocki for his remark on the age of rocks from Bukowa Góra — indeed, they are of the uppermost Emsian age, while the remanence is probably Middle-Upper Devonian (M. Lewandowski *et al.*, 1987). However, I do not agree that uppermost Eifelian sandstones do not occur in the Holy Cross Mts.; it is unknown if they occur.

MOBILISM OF THE MAŁOPOLSKA BLOCK AND PALAEOMAGNETIC DATA FROM USB

The fundamental assumption of J. Nawrocki (1993*a*, *b*) for disproving the strike-slip movement of the southern Holy Cross Mts., was structural unity of the USB and MB since the Late Silurian onwards ("It is obvious that the Małopolska Massif (MB — M. L.) was behaving similarly (to USB — M. L.) because of the tectonic framework of these areas." J. Nawrocki, 1993*a*). According to J. Nawrocki (1993*a*), this idea was supported by many

Polish geologists. Following J. Nawrocki (1993*a, b*) I have also extrapolated palaeomagnetic data and resulted geotectonic implications from the USB over MB (M. Lewandowski, 1994). To my surprise, ignoring this time the opinion of many Polish geologists, J. Nawrocki (1995) wrote without explanation: "Possible movement of the Upper Silesia block does not have to indicate the necessity of occurrence of such movement in the Holy Cross Mts. area." This implies that J. Nawrocki, does not consider the USB and MB a coherent structural unit any more. One would, therefore, expect that J. Nawrocki will retract his negation of Variscan mobilism of MB, since it has been formulated under a wrong assumption.

NEW TEST OF PALAEOMAGNETIC STABILITY

J. Nawrocki has presented a new test to determine palaeomagnetic stability of a rock. By now, a higher unblocking temperature (T_{ub}) has been considered a necessary and sufficient condition for palaeomagnetic stability of rocks, since the process of acquisition of viscous remanent magnetization (VRM) is related to the **low** T_{ub} grains. This is because domain wells of such grains can easily cross energetic barriers and record a VRM. The coercivity of the **high** T_{ub} magnetic grains have not been considered important for magnetic stability of rocks, since the T_{ub} is the first-order factor that controls magnetic stability in geological time. That is why we clean a rock specimen by means of alternating field (AF) method at the room temperature first and then we subject it to thermal cleaning, if any remanence is still left.

J. Nawrocki has reversed the standard palaeomagnetic procedure to ascertain that the specimen **kg4d 500** of the Gruchawka Formation displays very low coercivity after heating in 450°C. This observation has led J. Nawrocki to the conclusion that the specimen is unreliable.

From the fact that only 6% of the initial remanence was left after thermal cleaning, I can guess that remanence may reside in Ti-magnetite (typically a low-coercivity, moderate T_{ub} magnetic phase). Bearing in mind that bigger grains display higher T_{ub} but lower coercivity (and *vice versa*), what kind of response to AF cleaning may be expected from this type of carrier after heating in 450°C, when most of the smaller grains have been relaxed in the free-field space and only the bigger ones remained palaeomagnetically oriented? Nothing else than it has been ascertain by the experimentor. Does it mean that we have to reject all palaeopoles defined for rock containing Ti-magnetite rocks that otherwise have been proved to carry as old as a Palaeozoic remanence? Perhaps so, but how the coercivity of the heated specimen translates into terms of palaeomagnetic stability and how magnetic carriers interact in a sedimentary matrix J. Nawrocki does not explain. By application of AF to the specimen from Gruchawka, J. Nawrocki has only introduced spurious magnetization that obscured a well-defined, shallow northwesterly characteristic component (see orthogonal diagram of his Fig. 4), similar to that from the Upper Emsian sandstones farther east of Kielce (cf. M. Lewandowski, 1991)..

Since J. Nawrocki declares that the investigated specimen is representative for the Lower Devonian Gruchawka Formation (see his Fig.4), it therefore confirms rather than denies the reality of significant departure of the direction of the characteristic remanence, residing in

the rocks in question, from a reference, coeval direction of Baltica. Consequently, contrary to J. Nawrocki's opinion, it speaks in favour of post-Early Devonian mobilism of MB.

The test described above has prompted J. Nawrocki to call the pole from the Emsian sandstones (M. Lewandowski, 1991) very suspect. I would very much like to see, therefore, what the coercivity of a Siewierz dolostone would be after heating above 450°C. Taking specimens from Podleśna quarry (J. Nawrocki, 1993*a*, his Fig. 2) as examples, they reveal a lack of any remanence at all, so there is nothing left for later AF treatment. Consequently, the dolostones of Siewierz should be considered less stable than the sandstones of Gruchawka and, with a clear conscience, J. Nawrocki may address suspicion to his own data.

Relying on the experiment, carried on the Siegenian?-Emsian? (J. Malec, 1993) Gruchawka Formation from Kielce, J. Nawrocki infers palaeomagnetic properties of the younger (the Upper Emsian) sandstones, located some 30–40 km eastward and concludes that they have also have low coercivity, since the directions in both formations are similar. Then he extrapolates his conclusion onto various Variscan formations of Central and Western Europe, suggesting that their palaeomagnetic record may also be unreliable for the same reason. In other words J. Nawrocki assumes that similar palaeomagnetic directions, whatever age and region, belong to the same genetic population. Consequently, it is enough to disprove one in order to reject all the rest. Such an induction does not follow generally accepted standards of scientific reasoning and, as such, cannot be approved.

SUMMARY

This paper indicates that the relatively stable model for the post-Early Devonian geotectonic development of the Upper Silesian Block (southern Poland) is based on an illusive superiority of individual, very well-defined palaeopoles, over the majority of sufficiently, well defined palaeomagnetic poles. It has been, therefore, documented that the key pole for this concept has been erroneously used by J. Nawrocki since it represents only one structural unit of the otherwise strongly rotated ophiolite suite of the South Urals. It has also been substantiated that recognition of palaeomagnetic stability from the coercivity of a specimen previously demagnetized by means of temperature has no foundation in the physical background of palaeomagnetism, hence the test is rejected.

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PALEOMAGNETYCZNE OGRANICZENIA MOBILIZMU WARYSCYJSKIEGO MASYWU GÓRNOŚLĄSKIEGO I MAŁOPOLSKIEGO — REPLIKA

Streszczenie

W odpowiedzi na komentarz J. Nawrockiego wskazano, że postulowana przez tego autora poeifelska stabilność bloku górnośląskiego względem Baltiki oparta jest o iluzoryczną wyższość zbioru kilku paleobiegunów o najwyższym w skali siedmiostopniowej wskaźniku wiarygodności Q nad zbiorem wielu paleobiegunów, z których każdy ma wartość $Q > 2$.

Jednocześnie udokumentowano, że kluczowy dla argumentacji J. Nawrockiego paleobiegun z południowego Urału pochodzi z kompleksu ofiolitowego, który podlegał poeifelskiej rotacji, w związku z czym nie może być on reprezentatywny dla pozycji paleogeograficznej Baltiki w eifl u.

Zwrócono uwagę, że test stabilności paleomagnetycznej autorstwa J. Nawroekiego, polegający na badaniu koercji próbki skalnej po jej uprzednim rozmagnesowaniu temperaturą, nie ma uzasadnienia w fizycznych podstawach paleomagnetyzmu i, jako taki, nie może być podstawą wyciągania jakichkolwiek wniosków.

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