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Interactive Comment

Interactive comment on "Geometry of the Turkey-Arabia and Africa-Arabia plate boundaries in the latest Miocene to Mid-Pliocene: the role of the Malatya-Ovacık Fault Zone in eastern Turkey" *by* R. Westaway et al.

R. Westaway et al.

Received and published: 3 December 2007

Reply to comments by anonymous referee 1

Paragraph 1

The first criticism is that we only investigate a small part of the boundary zone between the Turkish, African, Arabian and Eurasian plates. In fact, our paper covers a ˜300,000 kmˆ2 area that is mainly located in Syria and eastern Turkey, by no means a small area. This is because we are interested, at this stage, in discussing what the kinematics of this area were in the latest Miocene to Early Pliocene (LMMP), and how they relate to

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the present-day kinematics. As we state in the text, the fact that the kinematics were different in the LMMP from at present in this part of the plate boundary zone means that they were also different elsewhere in the boundary zone. However, at this stage we are mainly interested in establishing the geometry of the plate motions in the LMMP in the part of the region that we examine, rather than looking at the rest of the boundary zone. A broader study, as the reviewer is suggesting, would also be impracticable given the length restrictions imposed on papers in this particular journal.

Paragraph 2

The reviewer seems to think that the basis of any manuscript on the kinematics of the study region should be work on GPS satellite geodesy. He indeed cites the papers by McClusky et al. (2000) and Vernant et al. (2004). The McClusky et al. (2000) paper has been of fundamental importance in constraining the kinematics of the present study region. As is discussed in more detail in the Appendix below, it underpinned the demonstration by Westaway (2003) that on the basis of geological evidence the northern DSFZ is transpressive and also supported the arguments by Westaway (2004) regarding the nature of the linkage between the DSFZ and EAFZ. These papers, based on geologicql evidence, rather than the underlying geodetic evidence, have in turn formed the basis for subsequent work. For instance, the Seyrek et al. (2007a) kinematic model, likewise based on geological evidence, includes a preferred Euler vector for the relative rotation of the Arabian and African plates of 0.434+/-0.012°/Ma about 31.1°N, 26.7°E. This is an order-of-magnitude more tightly constrained than any estimate available from GPS.

McClusky et al. (2000) determined an Euler vector for the relative motion between the Arabian and Eurasian plates of 0.5+/-0.1°/Ma about 25.6+/-2.1°N, 19.7+/-4.1°E. This was an important result, given that the relative motion between this pair of plates cannot be directly constrained using geological evidence, because there is nowhere where their common boundary is a simple fault zone. Vernant et al. (2004) presented a revised Arabia-Eurasia Euler vector, determined mainly using data from Iran, which

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is 0.41+/-0.10°/Ma about 27.9+/-0.5°N, 19.5+/-1.4°E. As Vernant et al. (2004) showed, their pole of rotation lies within the margin of uncertainty of the McClusky et al. (2000) pole, but is more tightly constrained. Unfortunately, it is not possible to use this pole and the associated rotation rate in any analysis of the kinematics of the present study region, because (unlike McClusky et al., 2000) Vernant et al. (2004) did not determine Euler vectors for any of the other plates that share common boundaries in this region (i.e., for the Africa-Arabia, Turkey-Arabia, Turkey-Africa, and Turkey-Eurasia relative motions). Since the Vernant et al. (2004) reference has thus not added anything to knowledge of the present study region there seems to be no point in citing it. Furthermore, although the McClusky et al. (2000) paper has been of fundamental importance to the development of ideas on the study region (see Appendix), we have nothing new to say about it and thus cannot see any value in citing it, either. It anyway relates only to the present-day kinematics and therefore has no bearing on the LMMP kinematics, which is mainly what the present manuscript is about.

Paragraph 3

The reviewer also specifies discussion of details related to the internal deformation of the Arabian plate, providing a list of possible references. The Matar and Mascle (1993) paper is about the kinematics of the DSFZ in the vicinity of the Ghab Basin in NW Syria. This is one of many publications that failed to notice the transpressive geometry of the northern DSFZ (see Appendix); it is thus not a particularly useful reference to cite. Salel and Seguret (1994) discussed Cretaceous and Palaeogene crustal deformation in central Syria, which is not a topic of any particular relevance to the present study. Litak et al. (1997) is one of a number of studies of Late Cenozoic deformation of the northern Arabian Platform that were cited in the recent paper on this topic, by Demir et al. (2007), along with others, including Rigo di Righi and Cortesini (1964), Chaimov et al. (1990), Karig and Kozlu (1990), Brew et al. (1997), and Coskun and Coskun (2000). One could add a number of other publications to the list, such as Alsdorf et al. (1995) (mentioned by the reviewer), McBride et al. (1990) and Chaimov et al.

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(1992). However, as was discussed at some length by Demir et al. (2007), a major difficulty with the existing literature on this topic is the lack of chronological control on the deformation. This difficulty is compounded now that a distinction needs to be made between the LMMP phase and the present phase of crustal deformation. In most localities within the northern Arabian Platform it is currently impossible to say whether rates of deformation in the LMMP phase were the same as, or faster than, or slower than, the rates at present, or indeed whether the two phases of deformation have been in the same sense or have had different senses.

For instance, Demir et al. (2007) discussed one locality in NE Syria (Halabiyeh, between Madan and Tibni in Fig 1) where some control exists on the local rate of deformation of the crust, because terrace deposits of the River Euphrates are warped and can be dated as they are capped by datable basalt. However, an additional major difficulty, also noted by Demir et al. (2007), is that the dating of this basalt has demonstrated that the existing chronology of the Euphrates terraces in Syria is wrong, and that this well-developed fluvial terrace staircase is far older than was previously thought. Furthermore, the previous literature has correlated the Euphrates terraces assuming that they each maintain a constant height above the river. Since this assumption can now also be seen to be wrong, the field evidence has to be examined over again to work out precisely how the terrace deposits do in fact correlate. Work is currently in progress by one of us (R.W.), in collaboration with Syrian coworkers, to do this; a brief summary of the results obtained so far has indeed just been published (Westaway et al., 2007a). Similar recent studies have also revealed gentle local warping of river terraces in SE Turkey, including the terraces of the Euphrates around Birecik (Demir et al., in press) and of the Tigris around Diyarbakir (Westaway et al., 2007b). Some time ago a similar effect was observed in the Euphrates terraces in western Iraq (Tyrácek, 1987). However, no dating evidence was provided, and - for obvious reasons - none of us plan to go there to obtain such evidence any time soon. It will thus be some time before evidence such as this can be used to determine overall rates of internal deformation within the northern Arabian Platform.

In view of the fact that this work is in progress and that the outcome of it will probably not be known for years, and given that the existing literature provides no useful information about rates of deformation within the northern Arabian Platform, either at present or during the LMMP phase, we have deliberately kept discussion of this topic as brief as possible. We have thus cited Demir et al. (2007) as the most recent published update of the literature on this issue, and have also stated in the text. "East of the marked strands of the northern DSFZ, a succession of fold mountains (labelled in red in Fig. 1) extends across central and NE Syria and NW Iraq. These folded structures are thought to be underlain by blind reverse faults that have accommodated a significant component of the northward motion of Arabia. One such structure (the Jebel Bishri) is transected by the River Euphrates; Demir et al. (2007) have shown that the older (? Mid-Pliocene) terraces of this river are significantly warped across this structure, suggesting that such structures accommodated significant crustal shortening during the LMMP" (page 176 (8) line 24 to page 177 (9) line 2). We do not see any way to add usefully to this brief comment on this topic, but have expanded it slightly to include citation of some of the references requested by the reviewer and discussed above.

Paragraph 4

The reviewer concludes that we should expand discussion of deformation within the Arabian Platform beyond what is currently provided. We disagree. We can see no point in diluting a manucript that currently focusses on a well-defined issue by expanding discussion of this other topic. This is for two main reasons: (1) as discussed above, such a discussion would lead to the conclusion that there is insufficient quantitative data to say anything useful on this topic, and so it would therefore be pointless; and (2) length of papers in this journal is limited, so there is no scope for padding any paper with extraneous information.

Paragraph 5

The reviewer has suggested that we expand discussion of what new constraints on

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the kinematics of the "wider region" are provided by our analysis. We are not clear what this comment means. If it means discuss at length other parts of Arabia and the Eastern Mediterranean region (i.e., it is the same point as was made in paragraphs 1 and 4 of the review) then it has already been covered.

If, instead, the comment means "summarise the quantitative conclusions of the paper regarding the localities that have been studied", then these conclusions would seem to be the following: (1) The MOFZ was transtensional, with a slip rate of ˜2-3 mm/a, and was active during the latest Miocene to Early Pliocene (˜7-6 Ma to ˜3.5 Ma); (2) Slip on the MOFZ was associated with the dramatic crustal shortening that occurred at this time in the Kahramanmaras area of SE Turkey; (3) The MOFZ Euler vector is tentatively inferred to have involved relative rotation between the Turkish and Arabian Plates at ˜0.85+/-0.15°/Ma about a pole at ˜37.75+/-0.15°N, ˜38.8+/-0.3°E. (4) predictions from the resulting kinematic model agree with extensive geological evidence; and (5) we do not know what the DSFZ slip rate was, nor what rate of relative motion was accommodated at this time by internal deformation within the Arabian Plate. Conclusions (1), (2) and (4) are clearly stated in the text. Conclusion (3) is not; it has now been added to the body of the text, as well as to the Abstract and Conclusions sections. Conclusions (5) is a negative conclusion, so it is not emphasized.

Paragraph 6

We agree with the reviewer that Figire 1 is not displayed satisfactorily in the Discussion version of this manuscript. This is because it has had to be over-reduced to fit it onto a page in landscape format, this being the format used for the Discussion version. However, the Figure has been designed to display properly on a conventional journal page in portrait format, as is used for the final version of papers in this journal.

The majority of placenames marked on the Figure are mentioned in the text, where they are used to describe locations of active faults or geometries of faulting. The relatively sparse placenames away from the active fault zones are useful for location purposes

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and do not clutter the figure. We do not see any problem with abbreviating the names of some localities and writing the placenames in full in the key.

Paragraph 7

We do not wish to add an extra Figure showing the study region with the slip during the present phase of deformation restored to indicate the geometry of the region at the end of the LMMP phase. This is, first, because the manuscript is currently at the limit of length allowable for this journal, so no significant additions can be incorporated. To be of any use, such a Figure would need to be quite detailed, and so would take up a lot of space. Second, it is not difficult to visualise what the geometry of faulting was during thr LMMP phase by looking at the existing Fig 1 (as the text indeed says).

Paragraph 8

This is a repeat of earlier points. As already noted, some of the references that the reviewer has asked us to cite are relevant, but not critical to any arguments; they have now been included. Regarding the issue of self-citation, it is a matter of fact that the present team of authors (particularly the first author) have achieved more than most people regarding the understanding of the Late Cenozoic crustal deformation in the study region over the past decade or more. As a result, a certain amount of self-citation is inevitable. To demonstrate this, the principal developments in the topic are summarised below in the Appendix.

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We thank this reviewer for his comments, which emphasize the significance of our manuscript submission.

General comments

All three "general comments" request substantial additions to the text. However, like reviewer 1, reviewer 2 does not seem to have talen on board that this journal has a rigid limit on the length of any article. The present manuscript is right on this limit; thus

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it is not possible to add to its length. In fact, as the journal editorial staff know already, a version that covered the subject matter at greater length was submitted earlier in 2007, but was bounced by the journal for being too long. As a result, the manuscript was rewritten so that the main text emphasized the regional kinematics and the online supplement covered local details pertaining to the Malatya-Ovacık Fault Zone. The editor clearly prefers this format, so we will stick with it. In fact, since the journal does not have a print version and publishes both text and online supplementary material in an equivalent manner for open access, it is immaterial whether a given point is made in the text or in the supplement. The situation would be different if we had submitted to a subscription-based journal, such as Geology or G-cubed, for which supplementary material would only be accessible to subscribers. This was a major factor that influenced us to submit to eEarth in the first place.

(1) The reviewer criticizes the amount of self-citation. 29 references have been cited, of which 10 are self-citations, thus, indeed, 24%, or very slightly more than a third, of the references are self-citations. However, the purpose of much of this citation is to direct readers to recent publications that establish a kinematic model for the present phase of deformation in the region, involving the EAFZ. Most of these publications are in journals or books that allow much longer articles, and either cover the historical development of ideas at length, including discussion of alternative points of view, or direct readers to earlier papers that include such discussion, which has not been repeated because there has been nothing new to say about it. The development of ideas on this topic over the past decade or more is summarised in the Appendix, below. It can thus be seen that the present authors (particularly the first author) have been responsible for many developments in this field; thus some self-citation is inevitable. In response to reviewer 1, we have added 4 more references, none of which are self-citations, thus reducing the percentage of self-citations to ˜30%. Clearly, any number more of additional references could also be added, including many more self-citations (see Appendix). The criteria in choosing references is not who wrote them but whether they say something relevant about the study region. Percentages of self-citation references of this order are not

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uncommon. For instance, each publication on the Arabian Platform by the Cornell group (whose work we were asked to cite more extensively by reviewer 1) cites many other papers by the same group. As an example, the Litak et al. (1997) paper has 15 self-citation references out of 57, or ˜26%.

(2) The reviewer has asked for the discussion of the development of ideas about the MOFZ to be placed in the main text and not in the online supplement. However, as also noted above, that would make the main text too long. It is anyway immaterial where this discussion is located as both the main text and the supplement end up as files hosted on the Web.

(3) Like reviewer 1, this reviewer has suggested providing an extra Figure to summarize the changes in geometry of the plate motions. We do not wish to do this, first, because the manuscript is currently at the limit of length allowable for this journal, so no significant additions can be incorporated. To be of any use, such a Figure would need to be quite detailed, and so would take up a lot of space. Second, it is not difficult to visualise what the geometry of faulting was during thr LMMP phase by looking at the existing Fig 1 (as the text indeed says).

Minor points

(1) The reviewer has asked for the meaning of the abbreviation DSFZ to me stated again on p. 172 (4), line 11. This has already been defined twice (in the abstract and in the caption to Fig. 1) but stating it again does not cause any major problems.

(2) On p. 180 (12), lines 3 and 6 the reviewer asks that brackets be used in text that is already within brackets. Multiple nesting of brackets, as has been requested, is not generally considered good style in text.

(3) On page 182 (14), line 6, the reviewer objects to Figure being abbreviated to Fig. We can see no problem with this.

(4) The reviewer objects to the use of colour in Figs 1 and 2; he suggests using less

strong colours for some of the information so other information would stand out better. We cannot see anything wrong with the existing use of colour. On the contrary, the major problem with the Figures, as currently published, is that they have had ro be over-reduced in order to display them on pages in the landscape format with a wide right-hand margin that is used in the online discussion part of this journal. The Figures have instead been designed to display properly in the format used for the final version of papers for this journal, where they are intended to occupy the full height and/or width of a page in normal portrait format.

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Appendix: Key developments in undertsnding the kinematics of the study region

Westaway (1994) presented the first quantitative kinematic model for the interaction between the DSFZ, EAFZ, and NAFZ, based on geological evidence and analysis seismicity. This publication is generally taken as marking the start of the "modern era", as opposed to the "dinosaur era"on this topic. The vast earlier literature, made redundant by this publication, was either qualitative, and thus untestable, or made wild claims regarding rates of motion (e.g., slip on the NAFZ at ˜60 mm/a; extension in the Aegean region at ˜100 mm/a) for which there was never any proper evidence.

Westaway and Arger (1996) presented the first detailed quantitative analysis of the kinematics of the SW part of the EAFZ in the Gölbasi area. This paper established that the SW EAFZ has slipped by 37 km, a much greater distance than hitherto thought possible, and demonstrated that the modern left-lateral faulting post-dates the Late Cenozoic anticlinal folding in this area. This is indeed one of the issues discussed in the present manuscript. Many previous studies had argued that the EAFZ in this area is synkinematic with the crustal shortening, a view that continues to be repeated in the literature despite the clear evidence to the contrary.

McClusky et al. (2000) presented the first quantitative kinematic model for the Eastern Mediterranean region based on GPS satellite geodesy. This was quite similar to the Westaway (1994) solution, notwithstanding the totally independent information used, the principal differences being: (i) the NAFZ slip rate was revised to ˜22 mm/a compared with Westaway's (1994) ˜17 mm/a; (ii) the EAFZ slip rate was revised to ˜9 mm/a compared with Westaway's (1994) ˜13 mm/a; and (iii) the relative motion vectors derived from the GPS data indicate significant transpression across the northern DSFZ in NW Syria and southern Turkey.

Arger et al. (2000) presented K-Ar dates for many of the Late Cenozoic basalt flows in eastern Turkey. In particular, they showed that the basaltic andesite volcanism in the area SE of Kahramanmaras is Middle Miocene (a result subsequently confirmed by Tatar et al., 2004) and not Pleistocene as had previously been thought (on the basis of intuition, given the fresh appearance of the lava flows in the field, rather than any actual dating evidence). Both before and since the publication of these dating results the existence of this volcanism, and its assumed Pleistocene age, have formed the basis of published arguments that the EAFZ became active in the Pleistocene, rather than in the Pliocene as - for instance - Westaway and Arger (1996) suggested. Being Middle Miocene, this volcanism has absolutely nothing to do with the EAFZ and any argument that attempts to link the two is spurious.

Westaway and Arger (2001) presented the first quantitative solution for the kinematics of the MOFZ, and thus established the idea that this fault zone represents left-lateral faulting that was conjugate to the right-lateral slip on the NAFZ in the latest Miocene and Early Pliocene, before the regional kinematics changed when the modern EAFZ became active. The present manuscript updates this solution in the light of newer data (e.g., that presented by Kaymakçi et al., 2006).

Yurtmen et al. (2002) presented the first high-precision isotopic dating of basalt flows that are offset left-laterally by the Amanos Fault at the W margin of the Karasu Valley in the extreme south of Turkey. They thus established a significant slip rate on this fault, which was previously thought to have a very low slip rate given earlier dating. It turned out that this misinterpretation resulted from earlier dating being duff and previous results being poorly documented, in some cases leaving it unclear which of the many basalt flows in the region had actually been dated. Yurtmen et al. (2002) thus first established that the Amanos Fault is an important active fault within the modern plate-boundary configuration. Many people had of course suggested this previously, but the idea was not considered sound due to the dating evidence that then existed.

Westaway (2003) showed for the first time that the idea that the northern DSFZ is transpressive, suggested by the earlier GPS results (McClusky et al., 2000), is consistent with the geological evidence from NW Syria and S Turkey. This idea has since been confirmed by others (e.g., Gomez et al., 2006). Prior to the Westaway (2003) study, the northern DSFZ was unequivocally regarded as a transform fault zone - except of course by those who had tried to claim that it is not an active fault zone at all.

Westaway (2004) showed how the distributed deformation associated with the transpression on the northern DSFZ enables the strands of the DSFZ and EAFZ to link up and form a throughgoing zone of left-lateral faulting in the area of SE Turkey between Gaziantep and Kahramanmaras. This study also presented evidence that the eruption

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of the Homs basalt in NW Syria in the latest Miocene post-dated the start of slip on the northern DSFZ. This means that one cannot use the edges of this basalt as piercing points to restore the total slip on this fault zone, as many people had done previously and others - notwithstanding the evidence to the contrary - have done since.

Robertson et al. (2004) presented an authoritive synthesis of the evidence from the region to the north of Iskenderun Gulf, where the modern boundary between the Turkish and African plates comes ashore. Their synthesis resolved some major inconsistencies between earlier schemes for this region and thereby made clear which aspects of its crustal deformation post-date the latest Miocene start of strike-slip faulting in this region and which are older, and thus unrelated to this strike-slip faulting

Seyrek et al. (2007a) have established from new high-precision Ar-Ar dating that the slip rate on the Amanos Fault is ˜3 mm/a, making it even higher than the rate deduced by Yurtmen et al. (2002). This upward revision in rate means that, for the first time, the observational evidence of active faulting in this region can match the rate required by modern regional kinematic models, such as that by Westaway (2004).

Seyrek et al. (2007b) have shown, on the basis of new dating evidence supported by numerical modelling, that the present topography of the Amanos Mountains has developed since the Mid Pliocene, as a consequence of the transpression on the modern configuration of the northern DSFZ. Seyrek et al. (2007b) also addressed the idea, first suggested by McKenzie (1976) and tentatively adopted by Westaway and Arger (1996), amongst others, that the EAFZ continues SW, crossing the Amanos Mountains, and links into the so-called "Karatas-Osmaniye Fault Zone" near Iskenderun Gulf. It is now clear that this is a mistaken concept; the lineation of the "Karatas-Osmaniye Fault Zone" that has been mistaken as an active left-lateral fault is a relic of the Mid-Cenozoic reverse-faulting that preceded the development of strike-slip faulting in the latest Miocene, as some people (e.g., Kelling et al., 1987) had thought all along.

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