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Discussion

Reply to: Comment by Gray, Gregory and Miller on "Structural evolution, metamorphism and restoration of the Arabian continental margin, Saih Hatat region, Oman Mountains"

We welcome discussion of the complex structural interpretation of the HP zone of the Saih Hatat region, Oman. There are two very different models to explain the structures and timing of high-pressure metamorphism in NE Oman. Most geologists working in Oman favour a single, continuous NE-directed subduction to explain the origin and emplacement of the ophiolite and late-stage subduction of the continental margin to form high-pressure eclogite facies metamorphism (full references in Searle et al. (1994, 2003, 2004) and Searle and Cox (2002)). The second model favoured by Gregory et al. (1998), Gray et al. (2000, 2004) and Gray and Gregory (2000, 2003) involves an early (130–95 Ma) nascent SW-dipping subduction zone dipping beneath the passive margin, followed by a flip to NE-directed subduction during ophiolite emplacement. The crux of the arguments centre around the structures in NE Saih Hatat and older (pre-95 Ma) $^{40}Ar/^{39}Ar$ and Sm/Nd ages from the As Sifah eclogites.

At the outset, it is clearly wrong for Gray et al. to claim first recognition of the Saih Hatat fold-nappe. These structures were known for at least 15 years prior to 1998, by numerous geologists working in Oman, and were actually first mapped out at 1:100,000 scale by the BRGM group (map sheets Masqat, Quriat, Fanjah; Le Métour et al., 1986, 1990). Searle et al. (1994, fig. 5) published photos of some of these structures, although our mapping at that stage was far from complete. The mapping by Miller et al. (2002), while similar to earlier mappings, represents a significant improvement. Although we do disagree in some respects with the mapping of Miller et al. (2002), the mapping was *not* the point of our disagreement with Gray and his colleagues. We do disagree strongly over two main issues: (1) the interpretation of the structures with respect to early nascent SW-dipping subduction beneath the Oman passive margin, and (2) the timing of HP metamorphism.

1. Interpretation of mapped structures

The major structural controversy is outlined in the arguments of Gregory et al. (1998) and Gray et al. (2000) for SW-dipping nascent subduction prior to ophiolite formation and emplacement. Gray et al. (2000, p. 514) wrote: "New detailed structural mapping requires at least two major SW-dipping crustal scale shear zones that potentially root into the Moho. The presence of these shear zones necessitates convergence involving underthrusting directed toward the Arabian continental shield for at least part of the geological evolution of the Oman Mountains". We reiterate that the exposed geology of NE Oman does *not* require the shear zone to extend more than 30 km down to the Moho. The Upper–Lower plate discontinuity can only be traced down to the top of the Late Proterozoic Hatat schists as depicted on our restored cross-section (Searle et al., 2004, figs. 4 and 5), and in any case the sense of motion on this structure is more logically explained by exhumation-related structures.

We never suggested that Gray and colleagues mapped '*only*' one major shear zone. The word '*only*' did not appear in our paper, and is a misrepresentation. It is obvious that the NE Oman high-pressure region is characterized by widespread evidence of shear strain with several major and many minor, shear zones having been mapped. Miller et al. (2002) mapped a '*disrupted zone*' within their Lower plate, between the Hulw unit (P–T conditions 7–8 kbar; 380–420 °C; Goffé et al., 1988; Searle et al., 1994; El-Shazly et al., 2001) and the As Sifah blueschist (12–15 kbar; 450–550 °C) and eclogite (15–20 kbar; 540 °C; Searle et al., 1994) units beneath. This zone, now apparently rediscovered by Gray et al., as their 'As Sheik shear zone', is the same shear zone as depicted in Searle and Cox (1999, fig. 12) and mapped out in Searle et al. (1994, 2004).

Gray et al. have misrepresented our structural data in several ways. Firstly, we did recognize NNE-trending folds throughout the HP region. These NNE-facing folds we reconcile with extrusion of HP rocks along the footwall, resulting in apparent extensional fabrics (see Searle et al., 2004, figs. 8 and 9). Gray et al. choose to interpret NE-facing folds as resulting from SW-directed subduction, but the structures are equally, if not more logically interpreted as resulting from extrusion of HP rocks towards the continent. Secondly, we showed the shear zone between

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the Hulw and As Sifah units as being beneath the Upper–Lower plate discontinuity (Searle et al., 2004, fig. 6). It is the P–T data of Goffé et al. (1988) and Searle et al. (1994) that demonstrate a major difference in P–T conditions (6–8 kbar) across this zone, and this can only be the result of major attenuation of the sequence during shearing to juxtapose these rocks; it is more than just a '*disrupted zone*'. Thirdly, we have never discounted the Upper–Lower plate discontinuity as a major detachment. We did propose that the restoration and P–T conditions from rocks above and below show that movement along this detachment must have been largely horizontal, not down to the base of the crust, as proposed by Gray et al. Fourthly, the accusation that we failed to recognize upside down stratigraphy as part of major recumbent folds is bizarre. We show photographs and cross-sections of these recumbent folds (Searle et al., 1994, figs. 5 and 7; Searle et al., 2004, figs. 4, 8, 10 and 11).

2. Timing of HP metamorphism

The age of peak metamorphism of the As Sifah eclogites has long been controversial. The model preferred by Gray et al. has been strongly influenced by $^{40}Ar/^{39}Ar$ phengite ages in spite of the fact that in many other high-pressure terranes of the world, excess argon is a major problem and is not always resolved by interpretation of step heating analysis. It is well known that the closure temperature for Ar diffusion in white micas is ~350 °C, 200 °C lower than peak temperatures in the As Sifah eclogites (Searle et al., 1994). Furthermore, there is no doubt that U–Pb zircon ages (closure temperature >850–900 °C; Lee et al., 1997; Cherniak and Watson, 2000) reflect crystallization not cooling, and that Rb–Sr ages (with a closure temperature of ~500 °C) should also therefore be older than $^{40}Ar/^{39}Ar$ phengite cooling ages for the same rocks. The U–Pb and Rb–Sr ages of the As Sifah eclogites are in all cases younger than the $^{40}Ar/^{39}Ar$ ages published for the same or nearby rocks. The As Sifah eclogite $^{40}Ar/^{39}Ar$ age spectra have older apparent ages, not younger; and any reasonable interpretation of all of this data must discount the argon dates due to the problem of excess argon. Gray et al. have failed to explain how zircons contained within high-pressure phases can yield younger ages than phengite argon ~350 °C cooling ages from the same eclogite bodies.

Gray et al. (2004) published Sm–Nd apparent ages of garnet–garnet leachate–whole rock with calculated ages (based on at most three points) of 110±9 and 109±13 Ma. However, there are other explanations of this limited dataset, including younger garnet growth, arising from potential influence of sequestration of older Sm–Nd signatures in phases contained within garnet, or Sm–Nd disequilibrium amongst the phases in respect of the 'whole rock'. The fundamental problem not addressed by Gray et al. (2004) is that garnet contains zircon inclusions dated far more precisely by U–Pb TIMS at 79.1±0.3 Ma (Warren et al., 2003), an age comparable with the U–Pb SHRIMP age of 82±1 Ma subsequently published by Gray et al. (2004). There is no petrological or geochronological evidence for more than one episode of zircon growth, and zircon and rutile are part of the stable HP assemblage (Warren et al., 2003). These U–Pb dates corroborate a Rb–Sr age of 78±2 Ma (El-Shazly et al., 2001) and form a very self-consistent dataset, once the scattered and variably older $^{40}Ar/^{39}Ar$ ages are discounted.

Therefore, we reiterate that peak high-pressure metamorphism of the Oman eclogites occurred at ~79.1±0.3 Ma (Warren et al., 2003), not at 109±13 Ma (Gray et al., 2004). This is after ophiolite formation (zircon ages from plagiogranites ~95 Ma; Tilton et al., 1981) and after the start of ophiolite emplacement ($^{40}Ar/^{39}$ amphibole ages from the metamorphic sole rocks ~94 Ma; Hacker, 1994; Searle and Cox, 2002). Models of continental subduction of the Oman margin therefore do *not* need to involve a tectonic event prior to, and separate from, ophiolite formation and emplacement.

3. Interpretation of early SW-dipping subduction

Having discounted the structural 'necessity' of a SW-dipping Upper–Lower plate discontinuity all the way down to the Moho, and the old, pre-95 Ma $^{40}Ar/^{39}Ar$ phengite ages and the Sm–Nd garnet ages (with errors spanning 26 m.y. from 96 to 122 Ma), it only remains to counter the tectonic arguments for the nascent SW-directed subduction proposed by Gregory et al. (1998) and Gray et al. (2000, 2004). Their model shows the HP As Sifah unit subducting SW beneath the Arabian margin during the early Cretaceous. This model is testable and requires (1) that the Oman margin be an active margin and (2) that a suture zone must exist along the top of the As Sifah unit downgoing plate. All other models involve a variation on the theme of a single NE-dipping subduction zone, evolving with time.

The sedimentary evolution of the early and middle Cretaceous rocks in Oman record a completely stable, passive carbonate margin during that time (e.g. Scott, 1990), an observation inconsistent with the Gray et al. model of an active subduction zone beneath the margin. If the Gray et al. model was correct, then the 'microplate' descending to ~80 km depth beneath the Oman margin would now be represented by the As Sifah unit which is only of the present-day proportions 10 km NE–SW, by <5 km wide. The lithological units comprising the Hulw and As Sifah units (which according to the Gray et al. model must be separated by a suture) are largely the same, without any lithological evidence for exotic rocks at their mutual boundary (Searle et al., 2004, fig. 5). Accordingly, there is no evidence of a suture between the As Sifah unit and overlying structural units. Also, there is no geological or geophysical evidence of hidden continental crust outboard of the As Sifah region, making the SW-dipping subduction model untenable. We refer readers to our recent paper (Searle et al., 2003), which outlines in detail the geological and geochronological arguments against the Gray–Gregory tectonic model.

Gray et al. continue to cling to their model, which contains considerable inconsistencies and cannot be reconciled with over 30 years of detailed sedimentological, structural, metamorphic and geochronological data, gathered by many other researchers from the petroleum and academic sectors. The greatest pity is that, despite some thorough and detailed mapping presented in Miller et al. (2002), this work has suffered from the attachment of an untenable tectonic model. On the basis of the brief summary presented here and the more detailed arguments presented by Searle et al. (2003, 2004) and Warren et al. (2003), we can only view continent-ward subduction and high-pressure metamorphism prior to 95 Ma as an incorrect interpretation of geological and geochronological data. We hope that the plethora of recent papers published by the Gray–Gregory group does not mislead public opinion towards a model that has no geological base.

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