

Short communication

## Discussion: The Betic Cordilleras (SE Spain). Anatomy of a dualistic collision-type orogenic belt. Reply by the Author

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The concept of extensional collapse of orogenic belts is without doubt a major development in recent research concerning the evolution of such belts. The model is simple and elegant and presents a logical explanation for post-collisional extensional features encountered in many collisional orogenic belts.

Criticism put forward by Vissers (this issue) on my paper concerns among others the argument that: 'collapse of the orogenic wedge should end when the extended domain has sunk topographically lower than the zone of thrusting'. According to him, it is crucial to the argument that the working hypothesis of Platt & Vissers (1989) concerns the extensional collapse of continental *lithosphere*, not crust. He presents the results of a simple numerical experiment investigating the effect on surface elevation and change in potential energy of a standard lithosphere (125 km) with initial crust thicknesses of 25 and 30 km. In the experiment the lithosphere is thickened by a factor 2 (250 km) and subsequently the lower part of this thickened lithosphere is detached (according to Visser's figure 1) or removed by convection (according to the Figure caption) and the lithosphere returns to its initial normal thickness. The calculations show that the concept of orogenic collapse does not break down on the argument in my paper;

One may wonder, however, whether the numerical experiment covers the geological reality in the Betics. Thickening (or doubling) of the continental lithosphere in the collision zone in the Betic Cordilleras was caused by underthrusting of the African lithosphere underneath Iberia, so we may assume the presence of a lithospheric root at the moment subduction ceased. The crustal segment represented by the Mulhacen Complex at that moment had been subducted to a depth of approximately 35 km, which estimate is based on the HP metamorphic parageneses (Bakker et al. 1989). Subsequent heating produced medium-grade parage-

neses and from then on exhumation of the continental crust took place and the crustal segment of the Mulhacen Complex cooled down. At the onset of extension, the metamorphic parageneses indicate lower greenschist facies conditions (Bakker et al. 1989; Biermann 1995: Figure 3). According to Van Wees et al. (1992), who discussed the dynamics of extension and inversion in the Betic Zone on the basis of numerical modelling of the P-T-t path, temperatures for the Mulhacen Complex were reduced at the onset of extension at 28 Ma to approximately 360 °C, leading to a depth estimate of 14 km based on slightly higher temperature values than those corresponding to the steady-state geotherm. This indicates an uplift of the crustal segment since subduction of 21 km. The evidence indicates that at 28 Ma complete recovery of the thermal and rheological structure of the lithosphere had taken place, and thus that at the onset of the Late Oligocene–Early Miocene extensional phase a new steady-state thermal and lithospheric configuration was established (Van Wees et al. 1992). In other words, there does not seem to be much reason to assume that at the onset of the Late Oligocene–Early Miocene extensional phase much lithospheric root was still present at the site of the former collision zone, to trigger the sudden uplift and extension.

A second point of criticism by Vissers is that I confuse different interpretations in my discussion of the Platt & Vissers (1989) hypothesis. He writes: 'the working hypothesis of Platt & Vissers (1989) involved the *convective removal of a gravitationally unstable lithospheric root*, not the detachment of a gravitationally unstable subducting lithosphere slab. . . Biermann (1995) somewhat confuses these two interpretations in his discussion of the Platt & Vissers (1989) hypothesis. . .'

I have the feeling that Vissers misread my paper here. I have not written that the Platt & Vissers (1989)

model involves the detachment of a gravitationally unstable subducting lithosphere slab. I wrote: 'Model 2 involves extensional collapse of previously thickened continental crust (Dewey 1988; Platt & Vissers 1989). According to this model, detachment or convective removal of the lithospheric root of the orogen causes rapid uplift of the collision zone'. This sentence, in my opinion describes reasonably well what is shown in Figure 6 of Platt & Vissers (1989), and is compatible with their own explanation of this figure when they write: 'Initially, the collisional ridge in Figure 6A would have been underlain by a thick root of cold lithospheric mantle. Such a root is gravitationally unstable and it may become detached and descend into the deeper mantle. This process has been described as delamination by Bird (1978) and was discussed in terms of convection by Houseman et al. (1981). . .'

My conclusion from Platt & Vissers (1989) and the above 'Discussion' is that extensional collapse of an orogenic belt is a process on the scale of the orogenic belt itself, as it is controlled by the removal of the lithospheric root of the orogenic belt. Application of the extensional collapse model for the Betic Cordilleras can only explain the extensional features in the orogenic belt itself.

I have shown in my paper (following Van der Beek & Cloetingh 1992) that extension particularly affected the eastern part of the Betic Cordilleras, on the basis of post-extensional crustal structure, heat flow, crustal thickness and differences in post-extensional deformation behaviour. It is difficult to explain such a variation by assuming extensional collapse of the orogenic belt unless the process operates on a scale even smaller than the orogenic belt itself.

I prefer a different interpretation that emphasizes the fact that during the Oligocene–Early Miocene tensional stresses dominated the entire western Mediterranean area from the Gulf of Lions down to the Algerian basin, with different expressions in different areas, and that extension in the Eastern Betics is related both in time and space to this western Mediterranean extensional crustal domain.

The final criticism of Vissers is that Biermann (1995) confuses kinematic and dynamic arguments. '... In so far as Biermann (1995) does not elucidate the dynamics of extension in the adjacent areas, the kinematics of the western Mediterranean system as a whole does not provide any argument against the hypothesis that extension in the Betic Cordilleras was indeed fundamentally and genetically linked to the development of an intracontinental collision zone'. One may turn the

argument around: why doesn't Vissers indicate how the extensional collapse of the Betic Cordilleras is related to the overall extensional regime in the entire western Mediterranean? I find it difficult to speculate on the dynamics of extension in the western Mediterranean. However, it seems obvious that the local process of convective removal of the lithospheric root of the Betic Cordilleras cannot explain extension in the entire western Mediterranean area. Following Blanco & Spakman (1993), detachment of the down-going slab may play an important role in extension in the entire western Mediterranean; but that is a process on a much larger scale than convective removal of the lithospheric root of the Betic Cordilleras.

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