Perspective

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The making of slab breakoff

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Abstract: In the early 1990s we met as postdocs in Cambridge just after finishing our PhDs in geophysics and geochemistry. We engaged in an informal discussion on lithospheric mantle melting in convergent orogens that was guided by two questions: (1) how can plutons have a mantle source signature in cold convergence zones; and (2) be located in the downgoing plate? The 'slab breakoff' hypothesis emerged. Here we review from our personal perspective how the idea was originally developed. We then provide our view looking back from our role as spectators, given that we have left the topic after the design of the hypothesis.

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We have been invited to reminisce about how the 'slab breakoff' concept came about. We use this opportunity to reflect how this hypothesis arose from pure coincidence when we met as young postdocs at Cambridge University. At that time, we would have never anticipated the overwhelming application of this hypothesis. Today, when we type 'slab breakoff' into Google Scholar, we receive 20 000 hits. How did all of this happen?

Starting point

Where were we before slab breakoff appeared in our life? Huw Davies (hereafter HD), being a geophysicist, had recently completed his PhD thesis in late 1989, at California Institute of Technology where he worked on constraining mantle seismic structure from travel time of P and S waves. He then developed a dynamic model of the source region of subduction-zone magmatism. This work involved developing a numerical thermal model of the slab and mantle wedge temperature using the finite-element numerical method. The velocity of the slab was prescribed and the velocity in the mantle wedge was solved for. The work considered phase diagrams for melting, dehydration of basaltic and peridotitic rock compositions, and how water might be a major player in melt generation. It also considered how the melt might migrate towards the surface; possibly through magma-driven fractures. This work was advanced later with a model of the magma source region. Through his models HD was sensitized to the difficulty of heating shallow depths in a convergence zone to the high temperatures required for lithospheric melting.

Friedhelm von Blanckenburg (hereafter FvB), a geochemist, wrote his doctorate thesis at ETH Zürich in 1990 on isotopic ages in a Tertiary Alpine pluton, the Bergell Intrusion, and on radiogenic isotopes (Nd, Sr) to derive the sources of their melts. A confusing observation emerged: the Bergell pluton, which intruded 30 Myr ago, contained a lithospheric mantle source. In an orogenic setting this was thought to indicate subduction magmatism. Yet the melts intruded into the presumably downgoing slab composed of Penninic nappes of the Alps (rather than into the rocks of the Adriatic microplate beneath which the Penninic nappes were thought to have subducted southwards). Thus, the intrusion was located on the wrong side of the past Alpine suture, and there were several other small plutons aligned over *c.* 1000 km along the 'Periadriatic' fault.

Meeting in Cambridge

In 1991 HD and FVB met as first year postdoctoral researchers at Cambridge University in the Department of Earth Sciences, working in entirely different units. HD was based in the Institute of Theoretical Geophysics and FvB in Isotope Geochemistry developing new isotope systems on a fancy new and very big secondary ion mass spectrometer.

Why does a theoretical geophysicist talk to a geochemist?

One of the traits of Cambridge Earth Sciences at that time was that scientists adored attending talks in disciplines entirely different from their own. In keeping with this spirit HD attended a seminar FvB gave on his research about the Tertiary Alpine plutons. FvB showed from his isotopic data that the plutons had a definite contribution from the lithospheric mantle. This immediately spurred a question from HD. From his previous subduction-related research, it was not obvious how one would be able to produce the thermal conditions to melt the mantle beneath an orogen, built by thickening of the lithosphere. The mantle in the lithosphere is cold, and following collision is likely to be at even greater depth and hence higher lithostatic pressure, and therefore (without any other changes in conditions) thermodynamically even less likely to melt. This observation led us to define the questions: (1) how can plutons have a mantle source signature in cold convergence zones and (2) be located in the downgoing plate?

FvB and HD decided to meet and consider these two questions. They evaluated potential existing explanations for the Alps phenomena. One was wholesale delamination of the lithospheric mantle (Bird 1979). We discounted this model as it would lead to more widespread magmatism than the small string of intrusions encountered in the Alps. The same objection held for the case for convective instability of the thermal lithospheric boundary layer (Houseman *et al.* 1981), designed as a hypothesis for uplift of the Tibetan Plateau. Wortel and Spakman (1992) were the furthest in developing an idea that they called 'slab detachment'. Yet their hypothesis was not applicable to the Alps, as they did not consider this process to be driven by continental collision and to be associated with the uplift of high-pressure rocks, nor did they invoke the consequences in terms of magmatism. Essentially none of these intriguing previous hypotheses have taken into account lithospheric discontinuities such

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as those generated by the subduction of oceanic crust followed by subduction of continental crust upon basin ocean closure.

Generation of the slab breakoff hypothesis

In one of these meetings in 1991 the hypothesis was outlined. Slab breakoff, in the sense of Davies and von Blanckenburg, would take place if an ocean basin closes and continental lithosphere is subducted. Breakoff would take place at the interface between dense oceanic and lighter subducted continental lithosphere. The idea was seen to have potential to be an answer to our initial questions. Basically, a shallow enough slab breakoff would take the cold lithosphere away from the orogen, potentially exposing the deep parts of the orogen to hotter underlying asthenospheric mantle, which could lead to melting of the lithospheric mantle previously enriched in solids of lower solidus. We also saw that the geometry of the melting could also fit the observations, namely intrusion of melts into the downgoing plate upon continental collision. We were encouraged to develop the hypothesis further.

Developing the hypothesis

Thermomechanical model

An obvious encouragement for the hypothesis was that we could see a driver for slab breakoff from the difference in density of oceanic v. continental lithosphere. But was it enough? Therefore, we needed to estimate the extensional force that would arise from the change in buoyancy from subducting oceanic to subducting continental lithosphere and compare this with the strength of the lithosphere. To do this HD developed a thermomechanical model.

A simple model was made for the buoyancy, largely driven by the difference in crustal thickness. The model was inspired by the thesis work of J. van den Beukel (van den Beukel 1992), whose supervisor was R. Wortel. A thermal model motivated by the analytical model of Royden (1993) was implemented and brought together with rheology estimates to give an upper bound on the strength. We used two considerations to simplify the calculation. The first one was to assume that a critical strain rate would lead to localization during ductile extension. The second assumption was to just calculate the change in force arising from the difference in compositional buoyancy between subduction of continental and oceanic lithosphere, and not try to calculate absolute forces. This change in force was compared with the integrated strength (assuming the critical strain-rate). These showed that slab breakoff was quite plausible, as the change in force could exceed the integrated strength.

We then went on to model the thermal consequences of slab breakoff on the overlying lithosphere, which allowed us to estimate timescales for the whole process, and hence to compare with orogens.

Composition of involved mantle components

One prediction was the rise of hot asthenospheric mantle into the zone vacated by the broken-off slab. As this would lead to juxtaposition of hot asthenospheric mantle onto the thick lithospheric mantle that was previously metasomatized by fluids, partial melting would take place. If degrees of melting are small, we predicted ultrapotassic melts; if they were large, more calc-alkaline magmas would be produced. An important prediction was that melts would follow the zone of breakoff, if the tear propagated laterally.

Testing the hypothesis in the Tertiary Alps

At the same time, FvB reviewed the geochemical composition of involved mantle components as derived from small mafic dykes along the Periadriatic fault and the mafic precursors of granitic plutons (von Blanckenburg and Davies 1995). Indeed, their major and trace element patterns and Nd–Sr isotopes showed a melt origin in a lithospheric mantle segment highly enriched with incompatible elements that could partially melt if heated to *c.* 1000°C. Combined with the thermomechanical model this analysis showed that melting occurred around 80–100 km depth, which then also indicated the minimum depth of slab breakoff.

A more speculative consequence predicted from slab breakoff was the buoyant rise of crustal segments from the subducted continental slab. Indeed, dated minerals in all Tertiary high-pressure rocks yielded ages of 40–30 Ma, fitting with the presumed breakoff date. The hypothesis was ready. A schematic illustration of this slab breakoff hypothesis is presented in Figure 1, a figure used in conferences preceding the appearance of a similar figure in the paper by Davies and von Blanckenburg (1995).

Would anybody be interested in this hypothesis?

The first paper was originally submitted to *Earth and Planetary Science Letters* in November 1992, accepted in November 1994 and published in 1995 (Davies and von Blanckenburg 1995), after spinning the Alps part into *Tectonics* (von Blanckenburg and Davies 1995). In the time between submission and publication, we explored the reception of our idea at conferences. At that time we were not over-optimistic, as we felt we had produced more open questions

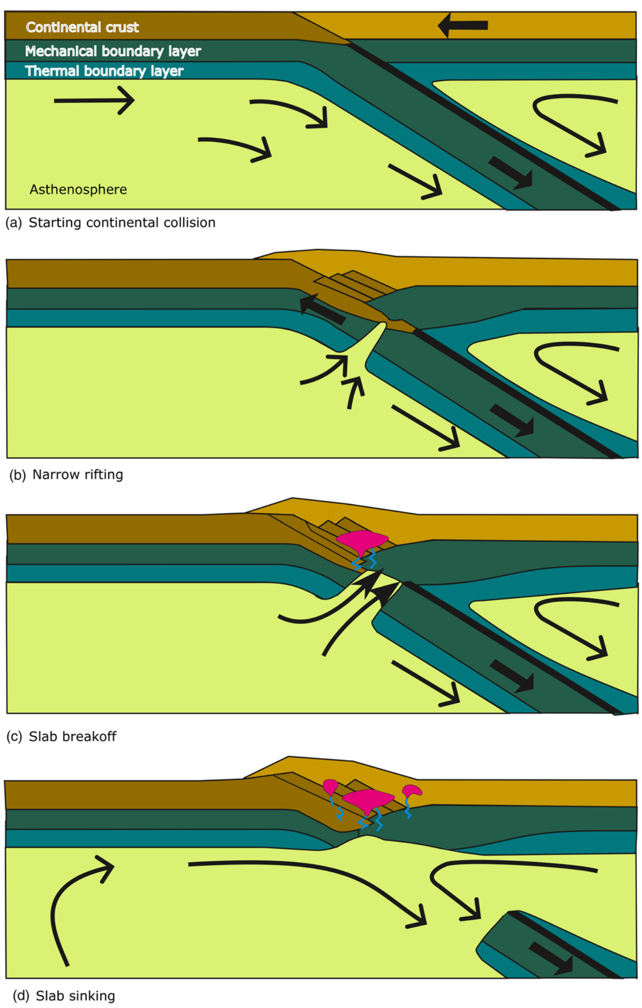

Fig. 1. A schematic illustration of slab breakoff used at conference presentations that preceded the first publication by Davies and von Blanckenburg (1995).

than answers. FvB gave the first talk 1992 at Fall AGU in San Francisco. Indeed, our lack of optimism was confirmed. The talk was moved into one of those 'any other business' sessions, in an evening slot after a long day, and a disappointingly low number of 15 scientists attended. HD presented a talk at the 1994 AGU Fall Meeting; again, a session in the last Friday slot with limited interaction. Most memorable to HD was being approached after the talk by another Huw Davies, who was wondering whether the idea could be applied to rocks that he was looking at in Papua New Guinea. Again, the attendance was not exactly an encouragement.

Three years later in 1995, then at EUG, after our two papers had appeared something had changed. A large lecture hall was jammed and all seats were occupied. Apparently, we had put something forward that the community was waiting for. In the years to come our hypothesis was embraced in many applications on almost all mountain belts in the world. This was a success we had never expected.

Life after slab breakoff

In the mean time, we, however, had moved on into other scientific territories. HD was doing studies of global mantle circulation and subduction. He has not tried to return to the topic of slab breakoff. Today slab breakoff is a topic in the geoscience curriculum at his current Cardiff University, but he does not even teach it himself. FvB moved first into ocean isotope chemistry, then into the cycle of elements and their isotopes between rocks and plants (and even to stable isotopes in human blood!), and eventually into erosion rates and rock weathering using new isotope geochemical methods throughout. FvB's slab breakoff past was so totally obscured that it led to sometimes bizarre encounters. In the year 2006 at a Dahlem conference in Berlin on 'Tectonics Faults' he was approached by the tectonophysicist Peter O. Koons from the University of Maine, USA, who expressed his disappointment over meeting an FvB who does 'all this weathering stuff' whereas he was hoping to meet 'the guy who did the much more exciting Slab Breakoff', before he realized he was talking to one and the same person.

Even though we have moved on scientifically we provide a small perspective on how we see the development of the field. HD has found that slab detachment/breakoff occurs in many of his more recent dynamic models (an advancement on the semi-analytic models used in the original paper). Others have simulated the process explicitly also in more sophisticated models and show that it is plausible dynamically, and what conditions are required. In the Alps, a much more nuanced picture has emerged today in the many studies that have appeared since 1995. Some deal with the propagation of the tear into the Carpathians, others suggest multiple breakoffs and some invoke alternative mechanisms such as slab steepening; too much to review here.

Twenty-five years after our original publication the sedimentologist Eduardo Garzanti and colleagues from the University of Milano–Bicocca, Italy, published a review 'Slab breakoff: a critical appraisal of a geological theory as applied in space and time' (Garzanti *et al.* 2018). They presented a compilation of 160 applications of the hypothesis in different geological settings. In an epistemological analysis Garzanti *et al.* used slab breakoff as an example for most geological theories that are hardly testable. Indeed, the theory cannot be falsified, 'and even if slab breakoff could be falsified in a number of well studied specific cases, the theory may always survive as an existential statement, because it might be documented in a further case, or it might occur in the future'.

Apparently, we have become parts of the history of science. That the simple questions of why granites are on the wrong side of the subduction zone, and why do they evidence lithosphere melting, would trigger all of this is not what we imagined at the time, nor the overwhelming application of something that was indeed merely intended to present a set of hypothetical mechanisms for a phenomenon that was previously not explainable.

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