

## Geology

### Carbon recycled into deep Earth: Evidence from dolomite dissociation in subduction-zone rocks: Comment and Reply

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#### Notes



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## REPLY

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I appreciate the interest that Hermann has shown in our study of carbon recycled into deep Earth (Zhu and Ogasawara, 2002a). The work reported in that paper was accomplished in Ogasawara's laboratory at Waseda University; all samples were collected by Ogasawara, and the first author investigated these samples independently. The description and conclusion reached resulted from his own observation and ideas, hence the first author has the sole responsibility.

First of all, the rock I studied was dolomite marble, not dolomitic marble as Hermann commented. These two kinds of rocks are rather different (Ogasawara et al., 2000). For example, all clinopyroxene show exsolution texture in dolomite marble, whereas clinopyroxene in dolomitic marble is homogeneous.

Dolomite is the main matrix mineral as well as the main inclusion phase in garnet from dolomite marble. Based on this, Hermann's statement that dolomite remained stable during subduction is obviously wrong—aragonite + magnesite can easily form dolomite with pressure decrease or temperature increase (Luth, 2001). Only high-pressure phases trapped in garnet can be preserved in low-pressure environments due to overpressure of garnet just like coesite in garnet or in zircon. This is the reason that dolomite decomposition texture is preserved in garnet only. The suggestion by Hermann of a reaction between pyrope and dolomite to form clinocllore is not supported by petrographic observations. If that reaction had really happened, a reaction zone consisting of clinocllore + calcite on the boundary between garnet and matrix dolomite should be observed everywhere. This, however, is not the case in Kokchetav dolomite marble.

Aragonite characterized with remarkably high intensity on back-scattered electron (BSE) images was explained in the original paper

by its higher density. An additional factor could be the contents of some heavy trace elements. We believe the Si content of 3.02 for analyzed garnet with the range of 3.01–3.04 is not from the analytical uncertainty and normalization process. All garnet analyses constantly have  $\text{Si} > 3.0$  in dolomite marble, whereas garnet in the reaction zone always contains  $\text{Si} < 3.0$  (Fig. 2A in Zhu and Ogasawara, 2002a). The analytical method and conditions as well as the normalization process for these two kinds of garnet are exactly the same. Hermann might have been confused by the field B defined in our paper, which only represents the earliest stage of metamorphism in the graphite stability field. Further exhumation obviously led the slab into the calcite stability field. The minimum pressure-temperature conditions for the exhumation path were estimated to be 800 °C and 2.5 GPa (Ogasawara et al., 2000).

All published temperature estimates for rocks I studied were based on a pyroxene-garnet thermobarometer, which yielded  $>950$  °C for clinopyroxene-garnet assemblage (up to 1200 °C; Okamoto et al., 2002). These values, however, could not represent the equilibrium temperatures for garnet and clinopyroxene, as clinopyroxene in dolomite marble developed exsolution textures. Another factor we must consider is that all pyroxene-garnet thermobarometers were designed based on experimental studies, which were carried out under pressures  $<4$  GPa (mostly  $<3$  GPa). Could we use such pyroxene-garnet thermobarometers to estimate temperatures of mineral pairs equilibrated at pressure  $>6$  GPa? The answer is probably "no." Nevertheless, the occurrence of postcollisional lamproitic rocks in Kokchetav ultrahigh-pressure massif (Zhu et al., 2002) implies high-temperature environments in the upper mantle.

The experimental results of Luth (2001) produced a steep slope for the phase boundary of dolomite decomposition compared to the result of Sato and Katsura (2001). The difference in the reaction boundary reflects the different kinetics between decomposition and synthesis reactions (Shirasaka et al., 2002). Besides their large difference, however, these two experiments produced very similar results at high pressures. For example, dolomite decomposition occurred at 8 GPa (1100 °C) and 9 GPa (1200 °C) based on Luth (2001) and at 8.1 GPa (1000 °C) and 8.5 GPa (1100 °C) based on Sato and Katsura (2001). The high pressure estimate is consistent with phlogopite exsolution texture in super-silicic clinopyroxene found in dolomite marble (Zhu and Ogasawara, 2002b). Phlogopite exsolved from super-silicic clinopyroxene implies that the precursor of super-silicic clinopyroxene must be stable at pressures higher than 8 GPa.

Finally, I want to emphasize again that dolomite decomposition was defined by texture observations at first order in our paper. In order to explain the lack of magnesite and the formation of clinocllore and the compositional change of garnet in the reaction zone between dolomite inclusion and its host garnet, I logically hypothesized a complex reaction (the reaction 2) based on petrographic observations (Fig. 1 in Zhu and Ogasawara, 2002a). This hypothetical mechanism not only provides a reasonable explanation for phase relations observed in dolomite marble, but could also explain the scarcity of magnesite in Kokchetav ultrahigh-pressure rocks although magnesite occurs in many ultrahigh-pressure terrains (Zhang et al., 2002a, 2002b, and references therein).

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