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Numerical tests and qualitative approach to study of lavas and associated carbonate-rich pyroclastic rocks from the Intra-Apennine volcanoes. A reply to comments by D.K. Bailey

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ABSTRACT. — Bailey (2005) reports on major element mass balance calculations for San Venanzo volcano to exclude that carbonate-rich rocks were generated by simple two end-member mixing of kamafugitic lava and sedimentary carbonates. I believe that the comments by Bailey (2005) are invalid because: 1) simple two-end-member mixing is a simplistic assumption, which has never been proposed in my previous papers (Peccerillo, 1998, 2004); 2) Quantitative comparison between average lavas and a single sample of bulk pyroclastic rock is methodologically incorrect, since the latter has much more variable composition than lava, because of diversity of juvenile fraction, post-depositional alteration and probable modification during transport of pyroclastic material. Therefore, Bailey's discussion does not help to shed light on the very nature of carbonate-rich rocks from internal zones of Apennines, and leaves the key-questions I raised in my previous papers (Peccerillo, 1998, 2004), still unanswered.

RIASSUNTO. — Bailey (2005) applica dei semplici test di bilancio di massa, per discutere le relazioni genetiche tra le piroclastiti ricche in carbonati e le lave ad esse associate nel vulcano di San Venanzo. I modelli di Bailey (2005) consistono nel sottrarre calcite pura dalla composizione di un campione di piroclastite ricco in carbonato per verificare che le abbondanze degli elementi maggiori così calcolate sono dissimili da quelle della media delle lave kamafugitiche dello stesso centro. Tale test e un altro analogo vengono interpretati come prove contrarie all'ipotesi che le rocce ricche in carbonato di San Venanzo derivino da mescolamento tra magmi kamafugitici e sedimenti carbonatici. La mia risposta puntualiza che: 1) l'interazione tra magmi e rocce incassanti quasi mai è considerabile come un semplice mescolamento, e che comunque tale ipotesi non è stata mai da me avanzata (Peccerillo, 1998, 2004), almeno nei termini semplicistici discussi da Bailey (2005); 2) il bilancio di massa basato sul confronto tra una media delle rocce laviche e un singolo campione di piroclastite, è da considerare come privo di alcun significato poichè le piroclastiti di San Venanzo presentano una variabilità composizionale molto più forte delle lave a esse associate, a causa della diversità dei clasti juvenili, per effetto dei processi di alterazione secondaria che sono più forti nelle piroclastiti fini che nella lava, nonchè per le probabili modificazioni subite dal materiale frammentato durante il trasporto e la messa in posto. Concludo, quindi, che gli argomenti riportati da Bailey (2005) non hanno alcuna rilevanza petrogenetica e, pertanto, lasciano le mie obiezioni sulla natura carbonatitica delle piroclastici intra-Appenniniche ancora senza risposta.

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COMMENTS AND REPLY

In a recent paper on carbonate-rich pyroclastic rocks from the Intra-Apennine volcanic Province (Peccerillo, 2004), I expressed surprise and some degree of disappointment for the lack of reaction to my objections on the carbonatitic nature of these rocks (Peccerillo, 1998). Obviously, my criticism was only addressed to people directly involved in studies on Intra-Apennine Province (IAP). However, the comments by Bailey are welcome and much appreciated, especially for their constructive spirit and for the nice tones, which, in my opinion, are always to be used in scientific discussions.

The comments by Bailey (2005), however, do not help to shed further light on the very nature of IAP carbonate-rich rocks, and do not remove my initial doubts on the origin (i.e., magmatic or external) of carbonates. Bailey (2005) states that I had suggested bulk mixing between carbonate sediments and kamafugitic magmas to explain the composition of IAP carbonate-rich rocks. Successively, he performs quantitative major element mass balance calculation for average lava composition and a carbonate-rich pyroclastic rock from San Venanzo, to show that addition of pure calcite to lavas does not give a composition matching those of carbonate-rich rocks. Alternatively, subtraction of calcite to carbonate-rich rocks does not give the composition of kamafugitic lava. These numerical tests are interpreted to exclude mixing.

Unfortunately, the discussion by Bailey (2005) contains several flaws that invalidate his approach and conclusions. In short:

1. Although I surmised an external origin (i.e., from wall rocks) for carbonates of the IAP rocks, I did not advocate simple bulk-mixing, but rather mentioned, though did not discuss, a number of possible processes (Peccerillo, 1998, 2004). I am well aware of the complexities of interaction between magmas and wall rocks, which in a very few cases can be considered as a simple two-end-member mixing. In my papers, I gave for granted that IAP carbonate-rich pyroclastic rocks are mixtures between silicate magma and carbonates, as clearly stated by Stoppa and Woolley (1997). If this was the case (and I had no reasons to have doubts about it), I concluded that calcite is a diluent of the silicate fraction, since the abundances of all trace elements and most major elements decrease with increasing carbonate contents, from lavas to the associated carbonate rich rocks. In his comments, Bailey (2005) seems to give another version of the carbonate-rich composition, recalling that Polino rock consists of mantle debris surrounded by carbonates. This seem in contradiction with Stoppa and Woolley (1997), and adds some confusion to discussion. In any case, my comparison was only restricted to San Venanzo and Cupaello occurrences, which are the only places where both lavas and carbonate-rich pyroclastic rocks have been found. The reason of this choice is fully explained in my reply to Woolley et al. (2005), and will not be reiterated here.

2. Comparison between lavas and pyroclastic rocks is always problematic, and is particularly so in the case of San Venanzo. Stoppa and Woolley (1997) report that carbonate-rich lapilli tuff have a composition for the silicate fraction that ranges from melilitite to phonolite. This is much more variable and quite different from the composition of kamafigitic lava. Note that, K-feldspar has been found in the pyroclastic rocks of San Venanzo (Stoppa and Cundari, 1998), which is not the case for kamaugitic lavas. This is not surprising, since compositional variations of the juvenile fraction of pyroclastic rocks and from these to late erupted lavas are very common for volcanic sequences. Additional compositional variation for pyroclastic rocks can result, among others, from clast selection during transport as well as from post depositional alteration of fine material. Note that values of LOI are much higher in the pyroclastic rocks than in the lava at San Venanzo (e.g., Stoppa and Woolley, 1997). For these reasons, I believe that numerical tests that are based on compositions of average lava and of a single sample of bulk carbonate-rich pyroclastic rock, such as those performed by Bailey (2005), represent simple mathematical exercises that cannot be of any help to prove or disprove mixing or any other process. Because of all these problems, I used a broadly qualitative approach in comparing lavas and associated carbonate-rich pyroclastic rocks (Peccerillo, 1998, 2004), and on this basis the decrease of element abundance with increasing carbonate contents was detected. Such a method is relatively save and much more appropriate than numerical tests, since it is not affected to a significant degree by compositional

variations of juvenile component (phonolites are enriched in incompatible elements, anyway), and also by secondary modifications, unless these are so severe to completely offset pristine compositions, also for immobile trace elements. This does not seem the case for San Venanzo rocks, judging from data reported by Stoppa and Woolley (1997).

3. As fully discussed in my reply to Woolley *at al.* (2005), I fear that the geodynamic implications of the Intra-Apennine Province have been overemphasised. Whatever the nature of IAP rocks (either carbonatitic or carbonated rocks), they have the same trace element and radiogenic isotope signatures as the mafic rocks of the nearby Roman Province. This strongly suggests that the Roman and IAP magmas come from similar sources that have suffered the same type of metasomatic modification. Therefore, a single geodynamic setting is probable for the two provinces. Since the IAP rocks can be suspected to have interacted significantly with wall rocks, I do not see any reason to rely upon IAP rock rather than on the Roman volcanics to constrain petrogenesis and geodynamic significance of central Italy magmatism. The occurrence of carbonatite-kamafugite in central Africa does dot tell much on source evolution and geodynamic setting of central Italy occurrences. The African rocks have very different trace element and radiogenic isotope signatures than central Italy magmas, which suggests different types of source metasomatic modifications and distinct geodynamic settings. Magmas with similar major element composition can occur in different tectonic settings. This is true for most magma types, from tholeiitic basalts to lamproites and, I believe, kamafugite-carbonatite. In my opinion, trace elements and radiogenic isotopes are much better tools to constrain evolutionary histories of mantle magma sources and, therefore, to distinguish among various geodynamic settings.

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