

Schuling leaves 'the way open to other suggestions'. May I suggest that the last term to which he refers, 'calc-silicate *granofels*', proposed in 1959 by R. Goldsmith (*Granofels*, a new metamorphic rock-name. *Jour. Geol.* 67: 109–110), is a very good general descriptive term and is well applied to the features Schuling sees in the 'residual cipolinos'.

Therefore, a new name is not necessary, and a genetical connotation should be avoided. If this point of view is admitted, the only way to include the genetical aspect in the descriptive term will be to use an adjective. In the present case, 'residual calc-silicate *granofels*' seems to be a good expression. Reading this term, all petrologists will have a clear picture of the rock and at the same time will understand that Schuling has good reasons to believe in its residual character.

### **Residual cipolino: End-product of calcareous rocks in regional metamorphism. A reply**

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In this issue of this journal, MICHOT (1966) takes exception to the author's introduction of the term 'residual cipolino', as well as to the author's contention that it is permissible to use two kinds of names and definitions for one object: the one linking it to direct observations (descriptive definition), and the other to its inferred mode of origin (genetic definition). This, even in petrology, is not a new notion. We might take, for instance, MEHNERT's (1959) two definitions of kinzigites, the one being a petrographic, descriptive definition: garnet-cordierite-plagioclase rocks  $\pm$  biotite-graphite-quartz; the other being a genetic one: rocks with excess Mg-Al-Fe, from which alkalis, SiO<sub>2</sub>, and H<sub>2</sub>O were driven out.

We might even hold that this two-fold system of classification and names is inherent to petrology, as it deals on the one hand with

petrography (descriptive), and on the other hand with petrogenesis (genetic).

I am pleased to see that Michot agrees with one of my suggestions for the descriptive part (calc-silicate granofels) which I proposed for the rocks under consideration. If other petrologists can agree, I think this settles the problem of the first, descriptive kind of name-definition. With regard to the second, genetic kind of definition, Michot points out rightly that it can only be used by people who hold similar views on the mode of origin. I agree with this point. People who disagree with the proposed mode of origin of the residual cipolinos might well use the following genetic name-definitions of these rocks:

- a) Metamorphosed ferruginous dolomitic marl, if one sees them as the product of straightforward metamorphism, accompanied only by loss of  $\text{CO}_2$  and  $\text{H}_2\text{O}$ , or
- b) Regional-metamorphic skarns, if one believes them to be the product of influx of Si, Al, Fe and Mg into limestone bands.

With regard to possibility b) I would like to take this opportunity to clarify a point in the paper under discussion. As I was not able to correct the proofs myself, a passage was omitted in which I indicated the difficulty in explaining these residual cipolinos as regional-metamorphic skarns due to the influx of Si, Al, Fe and Mg into a limestone band. As residual cipolinos contain on the average 10% of CaO, equivalent to roughly 18% of  $\text{CaCO}_3$ , and as they have thicknesses which commonly range from 5 to 30 cm, this means that the original limestone before the process of skarn-formation (involving addition of material) must have had thicknesses of 1 to 6 cm. It seems highly unlikely that such thin bands would ever have occurred intercalated between normal sediments and still retain their distinct characteristics along the strike over several km. It seems therefore more probable that they developed by a process of subtractive metasomatism into residues of normal limestones which were, on the average, 5 to 10 times thicker than what we now find. This means that they must have had thicknesses ranging from 25 to 300 cm and over. Geologically speaking, this seems a much more acceptable scale.

Returning to Michot's discussion on classification, it seems appropriate to cite what I wrote on this subject some years ago (SCHÜLING 1960, p. 17): 'A purely descriptive classification cannot and does not exist. When we give a description we only describe what we consider

significant, thereby making a choice which is mainly determined by one's own theories on the origin of what we are describing.' And, I may add, if we don't make this choice but try to classify according to some genetically irrelevant criterion, we only bring chaos into order instead of vice versa. Let everyone decide for himself of what use would be a classification of rocks according to color (magnetite ore, basalt, bituminous limestone falling in the class of black rocks) or according to mineral content only (sandstone, rhyolite, granite, quartz veins, gneiss falling in the class of the quartz-bearing rocks).

We deceive ourselves when we read too much into names and classifications. We should not pay too much attention to them; they are no more than handles, a convenient shorthand for lengthy descriptions of salient features (descriptive names) or of modes of origin (genetic names), as the case may be.

#### LITERATURE

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