

before we can come to any definite conclusion on these and other controversial points.

M. B. RAMACHANDRA RAO.

Mysore Geological Department,  
Bangalore,  
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<sup>1</sup> C. S. Pichamuthu : *Proc. Indian Acad. Sci.*, 1935, 2, (3), 254-277; *Curr. Sci.*, 1935, 3, 606-608. *Half-Year. Jour. Mys. Uni.*, 1935, 8, No. 1.

<sup>2</sup> R. B. Foote, *Mem. Mys. Geol. Dept.*, I, pp. 29-31.

<sup>3</sup> E. W. Wetherell, *Rec. Mys. Geol. Dept.*, IV, 92-98.

<sup>4</sup> W. F. Smeeth, *Rep. Chief. Ins. of Mins in Mysore*, 1901, p. 18.

<sup>5</sup> H. K. Slater, *Rec. Mys. Geol. Dept.*, VII, Pt. 2, p. 4.

<sup>6</sup> W. F. Smeeth, *Rec. Mys. Geol. Dept.*, XIV, Pt. 1, pp. 25-26.

<sup>7</sup> P. Sampat Iyengar, *Rec. Mys. Geol. Dept.*, XV, Pt. 2, pp. 106-116.

<sup>8</sup> C. S. Pichamuthu, *op. cit.*, pp. 265, 273-274.

<sup>9</sup> P. Sampat Iyengar, *op. cit.*, pp. 107, 115.

<sup>10</sup> *Ibid.*, p. 111.

<sup>11</sup> C. S. Pichamuthu, *op. cit.*, pp. 262-63. Figs. 2 and 3.

<sup>12</sup> C. S. Pichamuthu, *op. cit.*, pp. 24, 37.

<sup>13</sup> A. Holmes, *Petrographic Methods and Calculations*, p. 437; also F. W. Clarke "Data of Geochemistry," *U.S.G.S. Bull.* 695, pp. 27-28.

<sup>14</sup> C. S. Pichamuthu, *op. cit.*, pp. 6, 17.

<sup>15</sup> *Ibid.*, p. 31.

<sup>16</sup> *Ibid.*, p. 9.

<sup>17</sup> *Ibid.*, p. 16.

<sup>18</sup> B. Jayaram, *Rec. Mys. Geol. Dept.*, XX, Pt. 2, p. 40.

<sup>19</sup> C. S. Pichamuthu and M. R. Srinivasa Rao, *Curr. Sci.*, 1933, 1, 276-277.

<sup>20</sup> C. S. Pichamuthu, *op. cit.*, p. 24; also *Curr. Sci.*, 1935, 3, 606-608.

<sup>21</sup> *Ibid.*, p. 11.

I HAVE read through the remarks offered by Mr. M. B. Ramachandra Rao on the results of my work on some of the rocks of the Shimoga Schist Belt, and I am afraid that many of these remarks are based on a rather superficial reading of my papers. It is after more than five years of field and laboratory investigation that I have come to the conclusion that the conglomerates, quartzites and banded ferruginous quartzites of this area are possibly sedimentary in origin, and I do not think that I can reasonably be accused of either basing my observations on "insufficient examination" or being "hasty" in my conclusions.

I am perfectly aware that "many of the features such as the shape and rounding of the pebbles, sharp boundary between pebble and matrix, varied assemblage of pebbles, the difference between pebbles and matrix, and alternation of pebbly and non-pebbly bands, etc., are not entirely wanting in conglomerates which could be shown to be of autoclastic origin." But it is the cumulative effect of all these and other characters found

in one particular bed, that I consider more important. I am not as sure as Rao, however, that a varied assemblage of pebbles can be present in an autoclastic conglomerate; certainly not, at any rate, the great variety that I have noticed and described in the Kaldurga conglomerates.<sup>1</sup>

Further, he states "Pichamuthu's argument that the granite pebbles do not contain orthoclase or microcline... appears to be based on insufficient examination." Rao is obviously not aware that by the courtesy of the Director of the Mysore Geological Department, I had an opportunity of examining not only the particular slides he refers to (Z<sub>4</sub>/869, 866) but also several others, and I am sure I have found no microcline or orthoclase. There is, no doubt, a little muscovite in the granite pebbles, but not enough to suggest that all the original potash-felspars were converted into this mineral.

I am particularly pleased to read from Rao's remarks that evidences of sedimentary origin of some of the rocks of the Shimoga Schist Belt have recently been obtained by B. Rama Rao. These discoveries strengthen my contention that some of these rocks have had a sedimentary origin. I would like to point out, however, that it is not quite fair to characterise my views arrived at after a detailed study of the various aspects of these rocks as "hasty" or "unwarranted" while according to Rao, ripple marks, current bedding and even "rain prints" recorded by Rama Rao in these metamorphosed rocks are "undoubted evidences of sedimentation"!

With reference to the tectonics of the area, the V-shaped outcrops of beds surrounding Nandi village,<sup>2</sup> indicate a pitching fold. Rao criticises me for "assuming" an overfold here, but himself proceeds to *assume* that the structure might have been caused by the intrusion of the granite.

Regarding the banded ferruginous quartzites, the great dominance of magnesia over lime has been considered both in the Lake Superior Region<sup>3</sup> and in Singhbhum<sup>4</sup> as evidence of sedimentation and in support of my conclusions, I have only followed this line of argument and have drawn attention to the similar relationships existing in the Bababudans.

In regard to the occurrence of spilitic rocks, it will be seen from a paper of mine to be shortly published elsewhere, that there is in this area, a soda-rich series of rocks

comprising albite granites, albite syenites, keratophyres, soda felsites, oligoclase dolerites, albite basalts, albitites and albite schists, and that the distribution of these rocks is not quite so "local" as Rao seems to think. Is it far-fetched to suggest that sediments derived from such a terraine would be rich in soda?

Rao would have it that metamorphism has been the cause of banding exhibited by the ferruginous quartzites. In support of this he quotes me and says "Pichamuthu himself notes as one of the effects of igneous intrusion 'the banding of the rocks have been emphasised and the ironstones in the neighbourhood recrystallised'." Rao has missed the significance of the word "emphasised" used by me; it obviously implies that the bandings were there before they were metamorphosed and that they were subsequently enhanced by metamorphism.

To any one who has not read my papers, Rao's statement, "that bababudanite is of secondary origin and not an original constituent of the amphibolites was pointed out by Jayaram as far back as 1923," might convey the impression that I was not aware of this. I have referred in one of my papers<sup>5</sup> to Jayaram's views who considered "that the occurrence of this mineral is precisely comparable to that of tourmaline in the altered acidic rocks of the Champion gneiss series". The other officers of the Mysore Geological Department did not, however, share with Jayaram in this view. My colleague, M. R. Srinivasa Rao, and I were the first to point out that the mineral was developed as a result of thermal metamorphism.<sup>6</sup> I am frankly surprised at Rao suggesting that I have now changed my views regarding the origin of this mineral in the schists. I have done no such thing. That I still adhere to the view that bababudanite has always originated due to contact metamorphism, becomes clear on page 30 of my paper on "The Iron Formations and Associated Rocks of the Eastern Bababudans" where I say "they (the bababudanite-magnetite schists) are formed by the thermal metamorphism of certain of the original layers in the ironstones"; also on page 37, where I have said, "like the mineral bababudanite, these amphibole magnetite schists have also originated as the result of contact metamorphism".

With reference to the age relationship between the Tarikere and the Bababudan Series, I have nowhere suggested that the

Kaldurga conglomerate is a basal formation. When I say that "The Tarikere Series is younger than the Bababudan Series, not because of its intrusive character, but considering the stratigraphical sequence," I do not imply a stratigraphic break between the two Series. All that I mean is that the rocks belonging to the so-called Tarikere Series (mainly the conglomerates) overlie those which are supposed to belong to the Bababudan Series (the iron formation, etc.).

CHARLES S. PICHAMUTHU.

Central College,  
Bangalore,  
December 13, 1935.

<sup>1</sup> C. S. Pichamuthu, *Proc. Ind. Acad. Sci.*, 1935, 2B, 265-268.

<sup>2</sup> *Ibid.*, p. 262.

<sup>3</sup> Van Hise and Leith, *U. S. Geol. Surv. Mon.*, 1911, 52, 506.

<sup>4</sup> Percival, *Trans. Min. Geol. Inst. India*, 1931, 26, 200.

<sup>5</sup> C. S. Pichamuthu, *Curr. Sci.*, 1935, 3, 608.

<sup>6</sup> C. S. Pichamuthu and M. R. Srinivasa Rao, *Curr. Sci.*, 1933, 1, 276-77.

#### Pungency in Chillies (*Capsicum annum*): A Mendelian Character.

IN a letter to *Current Science*<sup>1</sup> under the above title, K. Ramiah and M. R. Pillai state as follows:—

"The inheritance of a large number of characters in chillies has been studied by Deshpande (1933) but we have not come across any reference to the inheritance of pungency. It has therefore been considered that information given below with regard to this character will be of interest."

The inheritance of pungency has been studied by me and the results were reported in the *Scientific Reports*, Imperial Institute of Agricultural Research, Pusa, 1930-31, p. 37 and published in the *Indian Journal of Agricultural Science*, Vol. V, No. IV, Aug. 1935, 513-16. Both the references have evidently been overlooked by the authors. Their results, however, agree with mine in so far as the F<sub>1</sub> and F<sub>2</sub> generations are concerned.

R. B. DESHPANDE.

Botanical Section,  
Imperial Institute of Agricultural  
Research, Pusa,  
November 25, 1935.

<sup>1</sup> *Curr. Sci.*, 1935, 4, 236.