DISCUSSION

NUMERICAL SIMULATION OF GROUNDWATER FLOW IN A WEATHERED HARD ROCK AQUIFER: A CASE STUDY by M. Thangarajan. Jour. Geol. Soc. India, v.53(5), pp.561-570

Pradeep Raj, 1-1-298/5 Ashoknagar, Hyderabad - 500 020 comments:

I have read with interest the article and feel that the following points need to be clarified:

1. I believe that in case of pumping tests conducted on large dimension dug wells, Papadopulos and Cooper (1967) method is the right procedure to arrive at the aquifer parameters (Karanth, 1987; Kruseman and de Ridder, 1990). However, in the paper it is mentioned that recovery phase of the aquifer test was used for interpretation of the test data (p.564). Can the author give some standard references where the recovery data from a large dug well was used to arrive at Transmissivity (T) and Storativity (S) of the aquifer? It is pertinent here to note that Rushton (1989, p.12) in a talk at National Geophysical Research Institute, Hyderabad stressed that if the tests are made on large diameter open wells, Theis Recovery cannot be used for calculating T and S. However, it is not clear exactly which method was used to arrive at aquifer parameters given in Table 1 - Theis Recovery or something else which the author forgot to quote.
2. On page 564, the author mentions that the aquifer is semi-confined (from S values). Shallow weathered systems are unconfined as the author himself seems to think, but feels differently a little later on page 567. This surely confuses the reader. Can he clarify?
3. Along Bukaleru river, T values are not given in the model (Fig.5 on p.566). How groundwater is expected to behave along this river? How can this strip of alluvium be ignored totally in the model? Does the river represent a boundary? In my opinion alluvium can be treated as weathered rock, albeit with higher T and S, and hence should not be ignored.
4. An inventory of innumerable dug wells across the district would reveal that in general the weathered phreatic aquifer extends down to 10 or 12 metres and not 30 metres below the surface. Fractured rock aquifer becomes more important below 20 metres. Therefore the author's assumption (p.569) that phreatic aquifer is 30 metres thick is not valid. The author himself seems to have some doubt about it. So he qualifies his statement and writes "if there is any fractured system within 30 metres, then simulation has to be carried out for weathered - fractured coupled media". Can he enlighten us on this?
5. Figure 10 on p.568 shows upward migration of contours (across more than half of the basin) for the post monsoon period, in spite of the normal recharge (assumed in the model) during the year 1994. How can water level decline from pre-monsoon to post-monsoon period in a year when recharge was assumed to be normal? Many of the observation well records (CGWB/SWGD) show a partial rise in water level during post-monsoon period even in a bad year. A post-monsoon decline in water level across half or more than half of the basin can only be due to faulty model calibration.
6. The author concludes that artificial recharge should be adopted to augment groundwater resources (p.569), a conclusion that has no bearing on the model simulated by the author. Just by noting that the water levels in the basin are showing downward trend, any reasonable person would suggest augmenting the recharge to these aquifers. So, what is the contribution of the model with regard to artificial recharge?

Before suggesting artificial recharge, the following questions should have been answered by the author:

(i) How much additional recharge can occur if the existing tanks are de-silted?
(ii) What is the likely quantum of rejected re-charge in excess rainfall years?
(iii) Can the aquifer recoup if there is excess rainfall? What is the frequency of such excess rainfall years?
(iv) In a drier year, can the aquifer remain depleted even with artificial recharge structures in place? How much additional recharge can be expected with such measures?
(v) Where should the artificial recharge structures be located in the basin? (The author casually suggests upland areas).
(vi) What should be their storage capacity?
(vii) What should be the water spread area?
(viii) How important is water spread area vis-a-vis storage capacity of the structure?

7. The author has predicted a situation for May/Oct. 1994 and May 1995. Did he verify the field situation as might have prevailed during these months? He could have done it to note which of the two schemes given by him reflect the field conditions? This question arises because the paper was submitted in May 1998.
8. What is the most important achievement of the model? *That about 10% of the rainfall* infiltrates into subsurface strata and percolates to become part of groundwater body. This fact has been known since long time. Keeping all these deficiencies in view, I strongly feel that the model hardly reflects the prevailing field conditions and hence does not have any predictive value or deductions that could help understand these aquifers in a better or new way.
9. And lastly there are some irritants in the paper:

(i) Geological map of the basin (PROGRESS, 1990) shows large area covered by grey granite. It is commonly understood that there are outcrop level changes in colour, texture, mineralogy and chemistry of granitic rocks. Will it not be better to use the terms like Older Gneissic Complex and Younger Gneissic Complex as suggested by Radhakrishna and Vaidyanadhan (1997) and leave other details to petrologists and stratigraphers.
(ii) The author has used phrases like, "poor soil, heavy run-off etc." What he means by these? Poor soil implies a soil that is only half a metre thick? Or a soil that is clayey with very low vertical permeability? Or the soil that is sandy with high permeability but does not support vegetation? By contrast, what is a rich soil?
(iii) What is the significance of the number 684 mm (p.561)? How did the author arrive at this magic number? Does 683 mm of rainfall produce severe drought and 684 and 685 mm prevent it! "Severe drought conditions prevail ....below of 684 mm" in my opinion is not the right expression.
(iv) PROGRESS (1990) is a private document. When any report is a private document and not easily available, a brief summary of the methods or the data used in the report should have been given, especially as the paper depends on this report for all its field inputs.

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M. Thangarajan, National Geophysical Research Institute, Hyderabad - 500 007 replies:

At the outset, I would like to emphasise here again that the data used for the present model study is provided by PROGRESS, a well-known voluntary organisation carrying out groundwater studies in Hyderabad. Any clarification regarding the collection of data, methodology used to interpret data, description of geological or hydrogeological conditions of the area can be obtained from the technical report of Progress (1990) submitted to Department of Science and Technology (DST). However, the simulation aspect and conclusion arrived at are answerable by the author. The following are the point-wise reply to the comments of Pradeep Raj.

1. PROGRESS, Hyderabad had carried out large diameter dug well pumping tests and interpreted the data using Theis Recovery (90%) data. One standard reference is by Herbert and Kitching (1981), which deals with the determination of aquifer parameters from large diameter well pumping tests using Theis Recovery data interpretation. Others opine that Theis Recovery data (190%) can be applied, if discharge rate is constant; otherwise, one has to use both pumping and recovery phase for large diameter wells. Singh and Gupta (1986) deal with the case of estimating aquifer parameters if the discharge is not constant. The point that I would like to emphasize here is that, the single point estimates of T values are initially interpolated and assigned to finite difference grid as the initial input. During model calibration, transmissivity values are appropriately modified to get a better match of computed water level and observed water level. One can notice from Fig.5 that regional transmissivity values obtained from model calibration are different from the point estimate of pumping test values. This has brought out clearly that even if some unrealistic transmissivity values were obtained by using different interpretation techniques, they will be corrected during model calibration.
2. On page 564, it is stated that "The storage coefficient values of dug well pumping test **indicate** that the aquifer is a semi-confined one". This is a statement based on the field value as shown in Table 1. The storage coefficient value used in the model is different from the pumping test value and it is considered as a phreatic aquifer with storage coefficient 0.01 arrived through model calibration. The statement shows that the pumping test values only **indicate** that the aquifer is semi-confined in nature, but in the model it is considered as unconfined (phreatic) aquifer. Pradeep Raj has also arrived at the specific yield value of 2 to 3% in Kongal basin adjacent to the Bukaleru basin in Nalagunda district (Narasimha Reddy and Pradeep Raj, 1997).
3. All along river Bukaleru, the alluvium and weathered part of aquifer are treated as a single phreatic aquifer. Transmissivity values vary from 50 m^2/day to 120 m^2/day (west to east). The reader may appreciate that as a variable grid size (Fig.3) was used for the present model, the numerical value could not be printed in all grids. The river is treated as a part of aquifer system with high transmissivity values. The transmissivity values in the range of 50 m^2/day to 90 m^2/day were obtained in the river valley parallel to the river course.
4. Before commenting the weathered thickness of the basin, the reader should have read his own paper (Narasimha Reddy and Pradeep Raj, 1997) wherein Table 1 shows the well parameters

of the exploratory borewells in Kongal basin adjacent to Bukaleru basin in Nalgonda district. The column 4 gives a total depth drilled and the column 6 gives the water striking point. The water striking point for selected exploratory wells varies from 7.6 m to 37 m. Wells at Puttapaka and Ghanapuram had water-striking point at 37 m depth. I would like to know from the reader why water was struck at 37 m? The thickness of weathered part is variable in hard rock region and one should not conclude that the aquifer thickness is only up to 20m.

Moreover the two dimensional groundwater flow equation (2) on p.563 describing groundwater flow takes into account of transmissivity values, hydraulic gradient in space and time, storage coefficient value S and input/output stresses. Transmissivity value is a function of both saturated thickness and saturated hydraulic conductivity values. In numerical simulation, only T values are used and therefore, the individual effect of either saturated thickness or hydraulic conductivity is not considered. The assumption of phreatic aquifer thickness as 30 m does not have any bearing in this model. The well inventory made by PROGRESS shows that the maximum depth of dug well is 19 m tapping the weathered zone. The thickness varies from few metres in the highland to as much as 30 m (including the alluvium) in the central part of the river valley. Neither data required for two-layer aquifer simulation nor the computer software available at that point of time did allow the simulation of a weathered-coupled fractured media. If the requisite data for weathered-coupled media are available, then a two-layer model can be prepared.

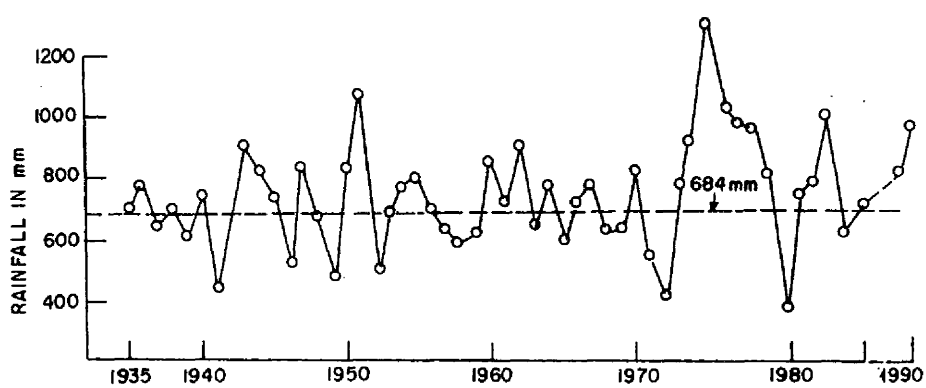
5. In figure 10, there is an index error. The dashed line should have been May 1994 and the continuous line as Oct. 1994.
6. In the conclusion part, it was suggested that "groundwater resources be augmented through artificial recharge". In the present study, no where artificial recharge methods were described and the objective of the model was to simulate the groundwater flow regime by making use of available data. It is explicitly brought out in prediction scenarios that regional groundwater level will decline during drought years. It is customary to give suggestions in the concluding part of the paper. Since, the artificial recharge is not the objective of the paper, the questions raised in this section become irrelevant. However, the reader may refer to Muralidharan and Athavale (1998), which will help him to get satisfactory answers for the questions raised by him.
7. Hydrogeological investigations were carried out by PROGRESS during 1988-1990. The modeling study was carried out with the available data from PROGRESS. Normally, model calibration needs **at least 5 years monthly water level data**, but we could use only 2 years' available data for this purpose. The preliminary model study was completed in 1993, but we waited for some more years to refine the model with additional field data. This could not happen and so the preliminary model study is published now. I could not get any additional data after 1990 and so there is no possibility of validating the model for realistic prediction. A model is a tool to study the aquifer response for probabilistic input/output stresses. A validated model can be used as a predictive tool. As far as this study is concerned, this is only a preliminary model based on limited database.
8. Narasimha Reddy et al. (1994) have arrived recharge estimate in a granitic terrain (Dulapally watershed 30 km north of Hyderabad city) about 10.4% of mean annual rainfall. Athavale (1985) had summarised the injected tracer results as 8 to 10% of annual rainfall. Narasimha Reddy et al. (1992) also reported recharge rate of 15% of mean annual rainfall in Parkal watershed, a granitic terrain in Warangal district. Gupta et al. (1985) also had arrived recharge rate of 8.5% in Shadnagar basin (Mahbubnagar and Hyderabad districts). It is a fact that divergence of recharge rate is significant in hard rock region and I do not know how Pradeep

Raj has concluded that "about 10% of rainfall infiltrates into subsurface strata and percolates to become part of groundwater body. This fact has been known since a long time".

A number of assumptions were used while coceptualising groundwater flow regime. It was repeated at many places that this model has to be refined with additional field investigation for its use as a reliable predictive tool. Nevertheless, the model has achieved the following:

(i) Regional transmissivity pattern was evolved by making use of only 6 transimissivity values,
(ii) Model calibration (steady and transient) has helped to arrive the regional water level configuration at the non-measuring point,
(iii) Though the model could not be validated, the future behaviour will indicate as to how the aquifer will respond to probabilistic input/output stresses and evolve the dynamic reserve as 28 million cubic metre (mcm) for mean annual rainfall of 700 mm/year.

9. Finally regarding comment 9:
   (i) Geological terms used in groundwater hydrology are not uniform. The description varies from person to person. One can notice in Fig.2 on p.562, the colour of granite is given as pink and grey. Narasimha Reddy and Pradeep Raj (1997, p.63) also described the granite in Nalagonda district as "leucocratic within the shades of grey or pink". I fail to understand how the change in terminology will affect the simulation of groundwater flow regime.
   (ii) Poor soil and heavy runoff are used here to give more emphasis on the poor groundwater potential of the basin. For more details regarding soil condition and geomorphology, one may refer to PROGRESS (1990).
   (iii) The mean annual rainfall (50 years average, 1935-1985) of Bhongir rain gauge station is 684 mm. The mean annual rainfall fluctuation is shown in the following figure which suggest that there was a recurring drought condition. The reader may interpret in his own way about the mean annual rainfall of 684 m and its terminology if the rainfall falls below 684 mm.



Mean and annual rainfall fluctuation at Bhongir in Bukaleru basin, 1935-1985.

Over all the comments do not appear to be pertinent to the simulation aspect, which is the main objective of the paper. If Pradeep Raj needs more clarification than what is provided in the reply, he may contact the following persons: K.A.S. Mani of PROGRESS on hydrogeology of Bukaleru basin, M. Muralidharan of NGRI on artificial recharge studies, V.S. Singh of NGRI on pumping test analysis and the author on modeling of aquifer system.

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(2)

K.C. Subhash Chandra, No.78, 5th 'A' Main, 4th Cross, R.P.C. Layout, Vijayanagar 2nd stage, Bangalore - 560 040 comments:

While complimenting the author for his analysis of groundwater flow though numerical simulation, I find it necessary for him to clarify on the following points:

(1) Flow to wells under unconfined conditions remains normally complex, mainly due to desaturation of aquifers and interconnection with source of recharge (Karanth, 1987). Such being the case, in the present context of study made, the author may explain the method that he has adopted to estimate the Transmissivity (T) and Storage Coefficient (Sy) values from the recovery data, particularly of large diameter dug well tests conducted.

(2) While the author states, "that the storage coefficient values of dug well pumping test data indicate that the aquifer is a semi-confined one" (p.564), he assumes in his studies the weathered part of the aquifer as homogenous and porous (p.565). The assumption and the ground reality hence differ.

(3) Comparing the observed water levels of two year period with computed water levels may not depict a correct picture. What is essentially needed in groundwater studies of unconfined aquifer conditions is the long term analysis of water table level/rainfall.

(4) The geological characteristics of the host rock, the topographic slope and the trend of the dykes, together play a role on the occurrence of groundwater and its movement. Merely reducing 50% of the transimissivity values and that too across the dykes, appears contrary to reasoning.

(5) Too many assumptions will ultimately give only assumed results.

M. Thangarajan, National Geophysical Research Institute, Hyderabad - 500 007 replies:

I would like to thank K.C. Subhash Chandra for his valuable comments on my paper. At the outset, let me clarify that the hydrogeological data used for this model study were provided by a voluntary organisation called PROGRESS at Hyderabad. This was also stated in the 'Introduction'.

1. The pumping test on dug well and interpretation was carried out by PROGRESS (1990). Theis Recovery (90%) data was used for the interpretation. The validity of this method was questioned by Dr. Pradeep Raj of Hyderabad and reply to this point is given on p.199 of this number (JGSI).
2. Though pumping test values of storage coefficient indicate that the aquifer is semi-confined in nature (Table 1), the model calibration has shown S value as 0.01 belonging to unconfined aquifer system. The single point estimate of T and S values was initially interpolated and assigned to each finite difference grid point and these parameters were adjusted during model calibration. In general, assumptions are used wherever data are not available. The highly weathered part of the aquifer is normally assumed as a porous one which then solves the partial differential equations governing groundwater flow.
3. Normally ten or more years monthly water level data are preferred for calibrating the transient model. Even five years monthly data may be just sufficient to calibrate the model. In this case, only two years water level data were available which was used in the model calibration. This aspect had been brought out explicitly while discussing the uncertainties in the model (p.568).
4. Correct reasoning can be given only based on field information. The role of dykes on the occurrence and movement of water is not established. The reduction of transmissivity value across the major dyke was arrived through model calibration. It may or may not represent the realistic field condition. This can be ascertained only after carrying out geophysical survey to delineate the extent of dyke penetrating the aquifer (length, width and depth). This can be incorporated in a future model if any study is taken up by some agency.
5. A mathematical model will be a useful tool to characterise the groundwater flow regime and to study the aquifer response for a probabilistic input/output stresses. A number of assumptions was made in the absence of required data. It is therefore, necessary to carry out additional investigations for the refinement and validation of the present model for use as a realistic predictive model.

PRECISE ^{40}Ar-^{39}Ar AGE DETERMINATIONS OF THE KOTAKONDA KIMBERLITE AND CHELIMA LAMPROITE, INDIA: IMPLICATIONS TO THE TIMING OF MAFIC DYKE SWARM EMPLACEMENT IN THE EASTERN DHARWAR CRATON by N.V. Chalapathi Rao, J.A. Miller, S.A. Gibson, D.M. Pyle and V. Madhavan. Jour. Geol. Soc. India, 1999, v.53, pp.425-432.

K. Gopalan, Anil Kumar and Y.J. Bhaskar Rao, National Geophysical Research Institute, Uppal Road, Hyderabad - 500 007 comment:

1. In an earlier paper Chalapathi Rao et al. (1996) have reported conventional K-Ar ages for just one sample each of two kimberlites (Kotakonda and Wajrakarur Pipe-5) and three lamproites (Chelima, Zangamarajupalle and Ramannapeta) within and around the Cuddapah basin. Despite the limitations of the K-Ar method and the risk of relying on just one sample from each suite, they contended that the south Indian kimberlites and lamproites belong to at least two generations (1400 and 1100 Ma), disputing our view (Anil Kumar et al. 1993) in favour of just one generation (1100 Ma). Since ^{40}Ar-^{39}Ar ages are more reliable than conventional K-Ar ages, their present report on Ar-Ar ages of these rocks is welcome. But

surprisingly, they have dated just two rocks (Kotakonda kimberlite and Chelima lamproite) and not all the five, as would be expected. If for some reason they could take up only two samples, the obvious choice should have been the Kotakonda and Wajrakarur Pipe-5 kimberlites to confirm if their Ar-Ar ages are also as different as their K-Ar ages reported earlier.

2. Stepwise heating/degassing commonly followed in Ar-Ar dating serves 'not to avoid the fundamental assumption in conventional K-Ar dating that all ^{36}Ar in a sample is of atmospheric origin' but to distinguish less Ar retentive (low temperature) phases/crystal sites from more retentive (high temperature) ones. Any ^{36}Ar in a sample after correction for interferences from fast neutron irradiation is in fact used to make a minimum correction to measured ^{40}Ar assuming the atmospheric ratio of 296 for ^{40}Ar/^{36}Ar. The correction will be much more and often less precise if the extraneous argon in a sample has a much higher ^{40}Ar/^{36}Ar ratio, as in the case of excess/inherited argon (McDougall and Harrison, 1988).
3. Any Ar-Ar study should necessarily provide information on any special sample treatment prior to neutron irradiation, monitor standard used and its assumed age, neutron fluence, interferences from neutron reaction on K and Ca (that may be entrained in kimberlite micas), ^{39}Ar recoil effects on fine grained samples, temperature steps, and most importantly, analytical errors in measurement of the different Ar isotopic ratios. But none is given for others to assess the validity of the ages reported and exceptionally low errors assigned to them, although Table 1, according to the authors, is said to present such errors. Column 3 in Table 1 should be ^{40}Ar/^{36}Ar and not ^{40}Ar/^{39}Ar ratios. Also Figs.1A and 2A should show isochron (regression?) and not plateau ages. The terms 'total rock age', 'regression age' and 'average plateau apparent age' are confusing, and should have been 'integrated age', 'isochron age' and 'plateau age', respectively. It is not clear how the errors given for these ages were calculated.
4. Neither 'concordance of plateaus' nor 'correlation of isochron plots' (whatever they mean) for the two rocks 'clearly suggest lack of evidence for significant argon loss or the presence of extraneous argon'. Table 1 shows ^{40}Ar loss from phases/crystal sites that released the first about 20% of ^{39}Ar at low temperatures. As for the absence of extraneous argon of non-atmospheric composition, the authors should have calculated the y-intercept of the isotope correlation lines (Figs. 1A and 2A) and shown that value close to 296 in each case, and not much higher as would be expected for excess/inherited argon. But this has not been done. Simple least squares linear regression of the isotopic data for only the plateau portion of the age spectra gives this ratio as +236 for Chelima and –26 for Kotakonda. While the Chelima ratio is distinctly less than the atmospheric ratio (+236), the Kotakonda ratio is clearly anomalous, which do not certainly bear 'testimony of confidence in results'. Since apparently very good plateaus in age spectra of some kimberlite phlogopites have been difficult to interpret (Hegner et al. 1995), the present authors should have exercised reasonable caution in assessing their data on just one kimberlite and one lamproite.
5. We do not clearly see any relevance of the ages of the Kotakonda and Chelima rocks to a lengthy discussion of mafic dyke emplacements not only in the basement in the immediate vicinity of the Cuddapah basin but in a much larger region covering the entire south India, Australia, and Antarctica. A few of our papers have also been misquoted in this process. For example, we have simply reproduced a report from Prof. D. York's laboratory in Canada that the age spectra of a few dykes in the Cuddapah basement do not show satisfactory plateaus, but suggest that the dykes were emplaced as early as 2400 Ma ago and partially reset about 1000 Ma ago. The present authors next dispute this ~1000 Ma secondary thermal event, as it has not apparently reset the older (1400 Ma?) Ar-Ar ages they have now measured for

Kotakonda and Chelima rocks. We do not see any conflict in this. K-Ar ages (Padmakumari and Dayal, 1987; Sarkar and Mallik, 1995) and Ar-Ar ages (Mallikharjuna Rao et al. 1995) have been measured on mafic whole-rocks consisting of many mineral phases of variable argon retentivity, whereas the present authors have analyzed a single mineral separate (phlogopite) from a kimberlite/lamproite, which could be far more retentive of its radiogenic argon. In fact, as pointed out earlier, even their age spectra do show argon loss in low temperature steps, which could be due to this post-crystallization thermal event. As for the integrity of the event older Rb-Sr ages of 1540±20 Ma for the Agnigundala granite (Crawford and Compston, 1973) and 1610±20 Ma for the Vinugunda granite (Gupta et al. 1984) on the eastern margin of the Cuddapah basin, it is too well known that Rb-Sr systematics in whole rocks are not easily reset by secondary thermal events (Faure, 1986). Regarding the Rb-Sr isochron age of 960±109 Ma reported by Crawford and Compston (1973) for a dolerite intrusive into the Lower Cuddapah, we did not argue but simply quoted the authors as suspecting that the rather high initial Sr ratio of 0.712 in this rock could imply the metamorphic equilibration at about 960 Ma of a much older dolerite. Therefore, we do not have to 'attempt to account for the higher ages of the Pulivendla sill at 1817±24 Ma' (Bhaskar Rao et al. 1995). As for the palaeomagnetic evidence for at least three separate periods of dyke emplacement in and around the Cuddapah basin later than about 1800 Ma, it is yet to be supported by direct and unambiguous dating.

6. Further, we did not make any observation of our own to claim that none of the dykes in the basement on the western margin of the Cuddapah basin cuts the basin. It is an overwhelming finding in the last two decades despite a single exception reported by Vijayam (1968).

As for their first recommendation for future research on dykes, we had already said this: "U-Pb ages for baddeleyites from dykes and Sm-Nd whole rock mineral isochron ages of least altered dykes will precisely mark the time(s) of dyke injection and reveal if there were discrete dyke activities later than 1900 Ma ago."

N.V. Chalapathi Rao, J.A. Miller, S.A. Gibson, D.M. Pyle and V. Madhavan reply:

Our paper concerns the models put forward by K. Gopalan and his co-workers regarding the contemporaneous emplacement of kimberlites and lamproites in the Indian sub-continent at 1100 Ma and contemporaneous emplacement of mafic dyke swarms in the Eastern Dharwar craton as early as 2400 Ma (Anil Kumar et al. 1993). We have found that their conclusions are at variance not only with our results but also are inconsistent with the geological, palaeomagnetic and geochronological studies undertaken by scores of previous workers, including their own earlier studies. Our view in this regard are further substantiated here:

1. Anil Kumar et al. (1993) dated just four kimberlites from Wajrakarur (south India) and one lamproite from Majhgawan (central India) to conclude that Proterozoic kimberlite and lamproite activities in the Indian sub-continent were essentially contemporaneous at 1100 Ma. Firstly, it is not clear as to how they could arrive at such a major conclusion without actually dating several other pipes. Secondly, their conclusion does not take into account the higher ages reported for Chelima lamproite (1319-1391 Ma; Murthy et al. 1987) and Lattavaram Pipe-7 (1205±10 Ma) (Anil Kumar et al. 1993).

   We have earlier dated by conventional K-Ar method phlogopite separates from four pipes – two kimberlites (Kotakonda and Muligiripalle) and two lamproites (Chelima and Ramannapeta). Since Ar-Ar dating results of the same samples of Chelima lamproite and Kotakonda kimberlite are consistent with our earlier results, within the error limits, we have re-asserted our earlier view of two generations of emplacement of kimberlites and lamproites.

Since the Wajrakarur Pipe-5 dated by U-Pb and Rb-Sr methods gave an age of 1079 Ma (Miller and Hargraves, 1994) and 1100 Ma (Anil Kumar et al. 1993) respectively, which broadly agrees with our K-Ar phlogopite age of 1140 Ma (Chalapathi Rao et al. 1996), and also since only two samples could be accommodated for Ar-Ar irradiation at the time of this study, we felt that it is only sensible to select one kimberlite (Kotakonda) and one lamproite (Chelima) to confirm whether their Ar-Ar ages are similar as their conventional K-Ar phlogopite ages. Therefore, Wajrakarur Pipe-5, whose age is any way well constrained, was excluded from the chosen samples. Regarding dating of rest of the pipes, a major project is underway to date all the pipes in the Indian sub-continent and those results would be published later.

2. $^{40}Ar/^{39}Ar$ dating allows the assumption that all ^{36}Ar is of atmospheric origin to be tested and circumvented if necessary (by plotting the results as isochrons) and also allows us to see whether or not the sample has suffered a partial overprinting during its existence (*see* Brereton, 1972).
3. Sample preparation techniques were discussed in Chalapathi Rao et al. (1996). No special treatment of the sample was made. An account of the standard techniques involved in our study, including error calculations, was described in detail in a number of papers published by one of us (JAM; *see* Fitch et al. 1969; Fitch and Miller, 1983) and hence were not provided. It is definitely nor surprising that the errors are low as the rocks are quite old and the atmospheric argon contents are minute. The Omegatron type mass spectrometer is ideally suited to measure small volumes of argon as there is no mass discrimination and no ion pumping. Column 3 in Table 1 should read as $^{40}Ar/^{36}Ar$ and not $^{40}Ar/^{39}Ar$ ratios. The terms 'total rock age', 'regression age' and 'average plateau apparent age' are being widely used for more than 30 years. It is strange indeed to suggest that 'regression age' should have been 'isochron age' when all isochrons are regression plots but not all regression plots are isochrons!
4. We have clearly pointed out in our paper (p.426) that "there is some loss of Ar in the initial steps for both the Kotakonda and Chelima samples (*see* Table 1)" and attributed this to the slightly younger ages obtained for these pipes from our earlier study (Chalapathi Rao et al. 1996). It may be instructive to refer to the works of Mitchell and Taka (1984) and Mitchell et al. (1988) who demonstrated, on the basis of correlated loss of K and Ar in biotite, that a loss of up to 20% of argon and potassium in biotite has little effect on K/Ar age. Recently, Gibson et al. (1995) have demonstrated that K_2O content of a mica can be a measure of its purity. The high K_2O content of Kotakonda (9.22±0.25 wt.%) and Chelima (8.69±0.29 wt.%) micas (*see* Chalapathi Rao et al. 1996) clearly indicates their lack of alteration, giving confidence in the accuracy of the age determinations and their geological significance.

We are aware that isochrons can be used to demonstrate the presence of extraneous ^{36}Ar but very small amounts of atmospheric argon make such an operation clearly pointless. We agree with the critics' calculations on initial ratios, but for the reason explained earlier, they have little real effect. Any errors introduced would fall within the error calculated for the age. While we are also aware that some kimberlite phlogopites have given what might be considered as extraneous results (Hegner et al. 1995), it is unscientific to reject results that appear to meet the experimental criteria without good reason. We have done our best to interpret the results in the fairest way possible. We would be certainly ready to revise our views in the light of further advances in knowledge.

5. Just because the K-Ar and Ar-Ar ages of the Kotakonda and Chelima rocks are not supportive of their proposed thermal impress model at ~ 1000 Ma, the critics comment that they do not see any relevance of the discussion of these ages with the dyke swarm emplacement in the Eastern Dharwar craton. We . trongly believe that the ages of kimberlites and lamproites are crucial in resolving the antiquity of dyke swarm emplacement in the Eastern Dharwar craton, especially in light of the thermal impress model of Anil Kumar et al. (1993). Since Eastern Dharwar craton, southern granulite terrain, Australia and Antarctica are generally presumed to be neighbours in the Proterozoic assembly of continents, comparison of their dyke swarm emplacements is imperative. Therefore, dyke activity in Eastern Dharwar craton cannot be treated in isolation.

   There is neither mention about the report of Prof. D. York in their paper nor any indication that the said interpretation was done by Prof. York.

   Critics overlook the fact that since the closure temperature of biotite is ~ 300°C (Dodson, 1973; Jager, 1979), it is prone to disturbance even during a mildest thermal episode which must be reflected in the plateau of kimberlite/lamproite micas (*see* Figs.1 and 2). Neither the plateau nor regression plots (MSWD<1) give any indication of subsequent thermal impress making it virtually certain that at least these areas where the pipes were emplaced did not experience any subsequent thermal episodes (p.428). This is excellently supported by the age determinations from recent multi-method internal isochron approach involving Sm-Nd, Pb-Pb and Rb-Sr systematics (Pandey et al.1997, p.189) on the mafic dyke swarm of Mahbubnagar which also do not show evidence for any younger regional tectonothermal event (p.428). Neo-Proterozoic dyke activity (~ 800 Ma; Rb-Sr studies) in an extensive area of ~2000 km^2 in the Eastern Dharwar craton (Devaraju et al. 1995) was considered by us as the strongest evidence for multiple episodes of dyke activity in the Eastern Dharwar craton (p.430).

   Rb-Sr studies on granites from Agnigundala (Crawford and Compston, 1973) and Vinukonda and Torakonda (Gupta et al. 1984) show very good isochrons (exceptionally low MSWD of 0.98) and do not bear evidence of any post-crystallisation thermal event. When mineral-scale Rb-Sr isotopic re-equilibration identified in the granites towards the western side of the Cuddapah basin can lead to the recognition of Palaeoproterozoic thermotectonic event(s) (*see* Bhaskar Rao et al. 1992), it is strange that Rb-Sr system in whole-rocks is not easily reset by secondary thermal events and therefore Agnigundala and Vinukonda granites do not bear evidence of subsequent thermal imprint.

   We have simply questioned in our paper (p.430) as to why the metamorphic equilibration experienced by the dolerite sill (dated 960±109 Ma by Crawford and Compston, 1973) intruding into the Lower Cuddapah, which was put forward as an evidence for their proposed 1000 Ma thermal imprint, was not, for some unspecified reasons, experienced by the Pulivendla sill (dated to be 1817±24 Ma by Bhaskar Rao et al. 1995) which is also a dolerite intrusion into the Lower Cuddapah. It is pertinent to note that Bhaskar Rao et al. (1995, p.337) point out that "~ 1800 Ma internal isochron age implies that the biotite in the Pulivendla sill did not experience even a mild thermal episode later than 1800 Ma". Since this statement is a direct contradiction of their view on thermal impress around 1000 Ma in and around the Cuddapah basin, it becomes absolutely essential to account for the age of the Pulivendla sill.

   The utility of existing palaeomagnetic data on the dyke swarms of Eastern Dharwar craton cannot be easily brushed aside. For example, Hasnain and Qureshy (1971) reported Cretaceous mafic dykes near Chitradurga based solely on palaeomagnetic and geochemical studies and the existence of such younger dyke events in the Eastern Dharwar craton was also subsequently confirmed by radiometric studies (Anil Kumar et al. 1988). Different episodes of dyke emplacement brought out by palaeomagnetic studies (Hargraves and Bhalla, 1983) in the Eastern Dharwar craton are also excellently supported by K-Ar and also Rb-Sr studies (Murthy et al. 1987; Padmakumari and Dayal, 1987; Devaraju et al. 1995; Dayal and Padmakumari, 1995; Sarkar and Mallik, 1995; Mallikarjuna Rao et al. 1995). It is also difficult to understand why argon loss, due to any thermal impress, in dyke swarms can be so systematic in assigning different ages to dykes orienting in different directions, whose temporal relationships are supported not only by palaeomagnetic but also structural studies (Chetty, 1995). When dykes belonging to the Late Cretaceous age were already recorded in the Eastern Dharwar craton (*see* Anil Kumar et al. 1988), we clearly do not see any logic in the model concerning contemporaneous emplacement of mafic dyke swarm in the Eastern Dharwar craton at ~ 2400 Ma (p.430).

6. Since the thermal impress model of Gopalan and others was entirely based on the premise that none of the dykes cut across the Cuddapah basin and hence they must necessarily be older, we have simply pointed out (p.430) that contrary evidences in the form of dykes intersecting the Dharwar craton and extending into the basin (Vijayam, 1968) also do exist and they also need to be considered in any model relating the basin formation to dyke activity. In fact, this observation of Vijayam was also supported by the field studies undertaken by Balakrishna et al. (1982) and to the best of our knowledge it was also not contradicted by any subsequent workers (*see* Murthy, 1987). Thus it is evident that there is convincing evidence for the existence of discrete dyke activity later than 1900 Ma in the Eastern Dharwar craton in the form of Neoproterozoic and Late Cretaceous dykes.

Our paper highlights the fact that despite more than a century of research, temporal variation between individual dyke swarms in the Eastern Dharwar craton and the relationship between the dykes and tectono-magmatic evolution of the Cuddapah basin is still not properly understood. We feel that apart from geochronological studies, even geological and palaeomagnetic investigations need to be urgently taken up so as to resolve these relationships.

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