

matters arising

Geomorphological dating of cave openings in South Africa

IN a recent article in *Nature*¹ Partridge dates South African hominid sites with remarkable precision by relating them to retreat of cyclic nickpoints. The soundness of his analysis and results depends on the validity of his geomorphological framework, the date of 20 Myr assigned to the inception of his cyclic nickpoints and his method.

The framework is well known and provides for the existence of identifiable remnants of 'Gondwana', 'post-Gondwana', 'African' and later cyclic surfaces of former continental extent². The dating and correlation of erosion surfaces over wide areas of Africa with quite different climatic and tectonic histories rest, however, on shaky grounds³. Moreover, results of recent work connected with the South African oil search on land and offshore contradict the framework in many material respects. Lack of space makes it impossible for me to elaborate here, but, as an example, the Paleogene, during which the African surface on land reputedly stood so low that the rivers were sediment starved⁴, saw the most rapid deposition off the Natal coast from the mid-Cretaceous onwards.

The dating of 20 Myr for the inception of the 'Post African 1' cyclic nickpoint, based on transgressive sediments above an unconformity at Uloa, Natal⁵, is taken as factual. The correlation of transgressive coastal sequences with incision on land is debatable, as it presupposes rather special conditions: but if we accept that the sediments above the unconformity could record the first incision of the 'African' surface, the figure may have to be doubled: in the J (c)-1 well, drilled 24 km off Stanger, the unconformity and 'Pecten Bed' are clearly identifiable, but the age is early Oligocene⁶. Relationships at Uloa, on which so much has been based, are representative of one stand of a slowly migrating shoreline.

Briefly, Partridge's method involves the calculation of mean rates of nickpoint recession in respect of each site, which are then plotted in relation to the midpoints of the segments affected by incision. He assumes that nickpoint migration was subject to linear decline culminating in zero at the stream sources, and apparently reads off the rates of migration at the hominid sites from the graph in Fig. 2. He admits

that the assumption is erroneous, but maintains that the errors of the linear interpolation technique are greatly reduced due to the close proximity of the sites to the headwaters. He thus makes an assumption that seems to be basic to his argument and calculations, admits that it is erroneous, but brushes away any possible reservations by implying that migration rates near the sites were so low that they would have been little affected by what happened downstream anyway.

Recognition of small nickpoints so far upstream as expressions of an ancient coastal event cannot be an easy exercise in a region with numerous nickpoints clearly attributable to differential rock resistance, warping, and so forth. Moreover, a method based on nickpoint recession cannot be applicable to a major basin such as the Orange, where great differences in gradient and channel characteristics reflect the extreme variations in lithology. The present regime illustrates the influence of climate: below Prieska the flow probably decreases at normal times downstream, and nickpoint recession rates would presumably not have decreased upstream under comparable past climates. My gravest criticism, however, is that the implications of differential nickpoint recession up a main stream and its tributaries are ignored. Thus the Orange, the Vaal, the Harts and a small tributary are lumped together, although they are clearly not amenable to treatment as a single unit; this procedure presupposes that recession rates are mainly dependent on distances of watersheds and nickpoints from the main drainage outlet to the ocean, and not on conditions along the stream courses themselves.

Partridge's methods and basic assumptions are of doubtful scientific validity, and his datings can only serve to confuse and mislead South African anthropologists.

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¹ Partridge, T. C., *Nature*, **246**, 75-79 (1973).

² King, L. C., *The Morphology of the Earth* (Oliver and Boyd, Edinburgh and London, 1962).

³ De Swardt, A. M. J., and Trendall, A. F., *Oversea Geol. Miner. Resour.*, **10** (3), 241 (1969).

⁴ King, L. C., *The Natal Monocline* (University of Natal, Durban, 1972).

⁵ King, L. C., *Trans. geol. Soc. S.Afr.*, **73** (3), 151 (1970).

⁶ Du Toit, S. R., and Leith, M. J., *Trans. geol. Soc. S.Afr.* (in the press).

DR PARTRIDGE REPLIES: Dr de Swardt's comments, although relevant to the technique and results reported in my article, reflect fundamental misconceptions as well as lack of familiarity with recent geomorphological research in South Africa. It would therefore be unprofitable to take issue with him point for point, though I should like to refute certain statements of his which have direct bearing on my argument.

(1) I claimed no "remarkable precision" for my estimates for the date of first cave opening at the various hominid sites. My results were presented cautiously and with deference to limitations in accuracy imposed by availability of data for the construction of the nickpoint migration graphs, and the resulting possibility of small scale fluctuations between observation points.

(2) The geomorphological framework which de Swardt calls to question is firmly based on scientific observation and measurement, and does not rely on the unsupported views of any one worker. In particular, Professor L. C. King has never claimed that all of the cyclic surfaces referred to by de Swardt were of former continental extent. This is a totally unreasonable proposition, and was certainly not applied in the context of my work. Indeed, the study of such surfaces in southern Africa has revealed that some are represented over limited areas, others have been partially planed and most have been subject to post-formational warping. All these factors have been subjected to detailed analysis (see references in my original article), and the results have been taken into account in the application of my technique.

(3) There is no contradiction of the geomorphological framework which I applied in the records of offshore oil prospecting cited by de Swardt. Various workers, including King, have recorded sedimentation of the continental shelf, dating from mid-Cretaceous to early Tertiary times¹⁻³, which can