

fossil cranial material of Mesozoic mammals was known and at a time when the alisphenoid-epipterygoid theory of synapsid evolution was still controversial^{3,11,12}. We now need to take account of the extent of this membrane bone, present in all mammals in a large part of the region, and to allow for its very variable pattern, in taxonomic analysis.

There seems to be no fundamental difference between therians and monotremes in the early development of this bone. Differences arise only when it fuses with its neighbours. It may well have been that the precursors of mammals had membrane bone in this region: cynodonts may have had an anterior part contributing to the broad blade of the epipterygoid and a posterior forming the smaller anterior process of the petrosal. In other groups the proportions may have varied: in triconodonts the anterior process is larger and the epipterygoid blade smaller, while in multituberculates the membrane-bone area may have contributed extensively to the petrosal to the exclusion of the diminutive alisphenoid (epipterygoid). In view of the variable pattern found in living forms and the difficulties of establishing detailed osteological homologies in fossils, it must surely be dangerous to erect two groups¹, however informally, based on

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the adult pattern of this region. Development shows that there is no 'monotone pattern' and therefore fossils may not be categorized as sharing it.

Excellent skull material of morganucodonts¹, triconodonts² and multituberculates^{3,14} is now being studied. It must be appreciated that the blade of the alisphenoid and the anterior process of the petrosal are developmentally very similar and may be morphologically almost equivalent. If Mesozoic, like Recent, mammals formed membrane bone in the area of the lamina obturans, it follows that any form in the fossil record with an expanded epipterygoid, an anterior process of the petrosal, or both, could, by a simple change in the affinity of synostosis during development¹⁵, come to possess therian anatomy. Therefore the recognition of the precursor form of therians must rest on other features. The structure of the wall of the cavum epiptericum itself in morganucodonts, triconodonts, multituberculates, cynodonts or even monotremes does not exclude close affinity to therians.

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Corrigenda

A correction to the letter '²¹⁰Pb in surface air at Enewetak and the Asian dust flux to the Pacific' by Karl K. Turekian & J. Kirk Cochran, *Nature* **292**, 522–524.

We have just discovered that the filters sent to us for ²¹⁰Pb assay and reported in our recent paper¹ were one-quarter aliquots of the total filter and that the values for volumes transmitted to us subsequently were for the whole samples. This means that Table 1 in our paper must be revised to increase the ²¹⁰Pb concentration in Enewetak air by a factor of 4: the correct version is shown below.

Table 1 ²¹⁰Pb and ²¹⁰Po in air filter samples from Enewetak, April–August 1979

Date of sampling	²¹⁰ Pb (d.p.m. 10 ⁻³ m ⁻³)	²¹⁰ Po/ ²¹⁰ Pb activity ratio
18 April	15.3 ± 2.1	—
26 April	12.1 ± 1.0	-0.02 ± 0.08
3 May	10.4 ± 0.76	-0.02 ± 0.03
10 May	12.2 ± 1.0	-0.02 ± 0.03
15 May	7.96 ± 0.72	-0.08 ± 0.03
20 June	5.96 ± 0.20	0.14 ± 0.11
8 July	8.00 ± 0.32	0.02 ± 0.28
16 July	3.17 ± 0.14	-0.26 ± 0.04
26 July	3.11 ± 0.076	0.17 ± 0.07
4 August	4.04 ± 0.16	0.05 ± 0.07
Tower blanks (filter carried up the sampling tower in exposed conditions)		
10 May	0.424 ± 0.108	-0.09 ± 0.10
26 July	0.180 ± 0.032	-0.05 ± 0.18

Sampling date represents the date corresponding to the midpoint of the collection period. Errors are 1σ counting errors.

This correction changes several relations that we discussed.

(1) Both the ordinate values for Fig. 1 and the abscissa values in Fig. 2 should be increased by a factor of 4.

(2) The corrected Enewetak filter results (present Table 1) compare more favourably with previous observations made at Hawaii². Between 15 May and 20 June 1979 at Enewetak, the concentration of ²¹⁰Pb was 6–8 d.p.m. 10⁻³ m⁻³ and for a similar period (30 May–22 June 1971) at Hawaii, the values below the trade wind inversion were 3.4–19.4 d.p.m. 10⁻³ m⁻³ with a mean of 9.3 d.p.m. 10⁻³ m⁻³.

(3) The regression equation for the calculation of the mean annual concentration of ²¹⁰Pb in air becomes

$$C \text{ [d.p.m. } ^{210}\text{Pb } 10^{-3} \text{ m}^{-3}] = 18.8 - 85.6 \times F \text{ [d.p.m. } ^{210}\text{Pb cm}^{-2} \text{ yr}^{-1}]$$

For a mean flux of ²¹⁰Pb of 0.15 d.p.m. cm⁻² yr⁻¹ this yields a mean annual concentration in air of 6 d.p.m. ²¹⁰Pb 10⁻³ m⁻³.

(4) The slope (S) of the regression equation correlating F_{Al} with F_{Pb} becomes 9.5 μg Al per d.p.m. ²¹⁰Pb. F_{Al} then becomes 0.63 μg Al cm⁻² yr⁻¹ corresponding to a dust flux (assuming 6.5% Al) of 9.6 ± 5 μg cm⁻² yr⁻¹, a closer value to that reported by Duce *et al.*³.

(5) In Table 2 of our paper¹ the increase by a factor of 4 of the mean ²¹⁰Pb concentrations reported there decreases the 'total deposition velocities' by a factor of 4. Thus 'dry' season total deposition velocity becomes 0.25 cm s⁻¹ and the 'wet' season becomes 1.0 cm s⁻¹. These values are now lower than our estimated deposition velocities for Hawaii (~3 cm s⁻¹) and Bermuda (~4 cm s⁻¹).

(6) The dust flux calculated for Hawaii using the parameters of the Enewetak ²¹⁰Pb–Al regression equation now becomes 93 μg cm⁻² yr⁻¹. This is easily in the range of accumulation rates of clay on the deep ocean floor. The conclusion regarding the dominance of the Asian dust flux to the sedimentation in the Pacific remains intact as does the suggestion that post depositional homogenization has occurred.

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In the letter 'Is the similarity of monozygotic twins due to genetic factors alone' by K. Gärtner and E. Baunack, *Nature* **292**, 646–647, in Table 1 mean ± s.d. of the MZ should be 26.41 ± 2.58 instead of 26.2 ± 2.66, and in Tables 1 and 2 the s_b² values of 51- and 61-day old DZ should be -1.18 and -2.49 instead of the printed positive values. In Table 2, the calculated F values of the s_b² of MZ and DZ are statistically insensitive; therefore the F and P values of s_b² should be disregarded.

The title of the letter by Doyle R. Watts and Andrew M. Bramall in *Nature* **293**, 638–641 should read 'Palaeomagnetic evidence for a displaced terrain in Western Antarctica'.

In the letter 'SB: a new HLA-linked human histocompatibility gene defined using HLA-mutant cell lines', *Nature* **293**, 747–749, the present address of Fritz H. Bach should read 'Immunobiology Research Center, University of Minnesota, Minneapolis, Minnesota 55455, USA'.

In the review article 'A new era in mammalian gene mapping: somatic cell genetics and recombinant DNA methodologies' by F. H. Ruddle, *Nature* **294**, 115–120, on p. 119 'α-globin' should read 'β-globin' throughout. In Table 1, the chromosome positions for the mouse α-globin pseudogenes are sited incorrectly: pseudogene ψ3 maps to chromosome 15 and pseudogene ψ4 to 17.