present level of erosion is relatively small. My alternative suggestion, which would also account for this parallelism of strike, depends on the orientation of the folds. The two folding episodes postulated (one before and the other later than the eruption of the Tertiary lavas) were very nearly co-axial. The fold axes are approximately horizontal so that, no matter how much the angles of dip in the two formations differed, their respective strikes would not be significantly different.

I do not dogmatically "reject a mechanical explanation" as Sir Edward has inferred. Maybe the margin between the Mesozoic and the Tertiary is faulted, but I think that the considerable angle of 60° difference in dip between the two formations requires that some alternative explanation should be put forward.

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CARBONIFEROUS GLACIATION IN GONDWANALAND

Sir,—My attention has recently been drawn to a paper by M. M. Anderson (1961) dealing with the question of whether a phase of this glaciation appeared in northern Brazil and Ghana during the early Carboniferous, and drawing implications from what I wrote in a synthetic paper upon this glaciation in Gondwanaland as a whole (1958) to which I would certainly not subscribe.

The original references were to Junner (1939, 1940) for glaciation in Ghana and Caster (1952) for glaciation on the Lower Tapajoz in Brazil (see below). As Anderson states, Crow (1952) has since expressed a view different from Junner (which I overlooked), but so far as I know Caster's record still stands. Anderson writes :

> "King considered (1958) that the Permo-Carboniferous glaciations began in North Brazil and Ghana. Thus he states that the great glaciations began in the early Carboniferous of Brazil and the Gold Coast".

The original passage was : "The great glaciations began in the early Carboniferous :" then a number of occurrences were mentioned, all in the same paragraph, from the Lower Tapajoz of Brazil and the Ajua shales of Ghana to western Argentina. This is merely a convenient geographical order of description, there is no statement that the glaciation began in the north, nor was such intended. If there was an ice sheet all the northern occurrences were probably peripheral, and Junner (1940) mentioned "The scattered very large boulders . . . and the recumbent folds and overfolds in the calcareous shales . . . may have been caused by icebergs". In a publication the previous year he had also referred to "varves" in the Ajua shales, which again infers a

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peripheral glacial environment. There is no suggestion by Junner or by me that a *centre* of glaciation lay over Ghana. This is surely made clear also by my phrase " discontinuous till or outwash."

The great centre of glaciation early in the Carboniferous was patently over western or north-western Argentina which then (Caster, 1952) had its most " thoroughly glacial record."

Anderson draws two implications : (*a*) " that North Brazil and Ghana must have been nearer to the South Pole in the early Carboniferous than they were in the late Carboniferous," which would be true on the above, though still about 30° from it ; and (*b*) " Gondwanaland had a northward drift during the Carboniferous period ". This statement is meaningless. As my original paper emphasised, a northward movement of one part would produce a southward movement of the trans-polar segment, and west–east or east–west movements in other parts.

If his conception (not mine) is of a polar centre moving direct from Ghana to South Africa then that is negatived by the absence of glacials over most of the region in between. There is, indeed, a great stratigraphic hiatus over most of Brazil and central Africa at this time. If Mr. Anderson will compare Text-figs. 1 and 2 of my paper and then extrapolate to an early Carboniferous stage with the pole about north-west Argentina he will appreciate the impossibility of my ever supposing a polar centre in Ghana.

Anderson's doubt of early Carboniferous glacial activity in North Brazil does not give credit to Caster's very careful statement " The presence of tillite on the lower Amazon, at the base of the Carboniferous terrain, has been largely overlooked since it was first discovered and published. However, the well-core samples still extant in Rio de Janeiro, seem to the writer fully to substantiate Moura's (1933) interpretation ". This is preceeded by records of diluted and chilled faunas " in the midst of Stephanian plant-bearing strata of the state of Piauhy ", and the tillite itself is described as sited " between the basal white sandstone of the marine Itaituba series and the so-called Upper Devonian black shales " (Caster, 1952). Nor is the suggestion of early Carboniferous glaciation for Northern Brazil, with minor extension to Ghana outrageous considering the widespread tillite in the Upper Devonian Longa beds of north-eastern Brazil (Kegel, 1957).

I am quite happy to be relieved of responsibility for synthesizing glaciation in Ghana and North Brazil into early Carboniferous Gondwanaland as a whole, if the evidence quoted by earlier field workers be found dubious. As I have made clear, in my view it would have been peripheral only to the great glaciation, and certainly I have never intended in my writings to imply that it was a polar centre of the main icecaps. Having been in Antarctica when Mr. Anderson's paper was published, I have too much respect for polar icecaps.

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TEMPERATURE DEPENDENCE OF THE DISTRIBUTION COEFFICIENT

Sir,—The mineral data presented by Frost (1962) form a valuable contribution to the study of mineral assemblages. His analysis of these data is however open to criticism from various directions. The present criticism is confined to his treatment of the temperature dependence of the distribution coefficient.

An exchange equilibrium between garnet of formula $(Fe, Mg)_3Al_2Si_3O_{12}$ and biotite of formula $K(Fe, Mg)_3AlSi_3O_{10}(OH)_2$ may be expressed :—

$$\text{Fe garnet} + \text{Mg biotite} \rightleftharpoons \text{Mg garnet} + \text{Fe biotite} \qquad (1)$$

Provided both phases are ideal mixtures, the distribution coefficient (K_D) takes the form :—

$$K_D = \frac{X_b}{1 - X_b} \cdot \frac{1 - X_a}{X_a} = \exp\left[\frac{\Delta G}{RT}\right] \qquad (2)$$

where $X = Mg/(Fe + Mg)$, a and b denote phases garnet and biotite respectively, ΔG is the Gibbs free energy change of reaction (1), R is the gas constant, and T is absolute temperature. The temperature dependence of K_D may be obtained as follows :—

$$\frac{\partial \ln K_D}{\partial T} = \frac{1}{R}\frac{\partial(\Delta G/T)}{\partial T} = -\frac{\Delta H}{RT^2} \qquad (3)$$

where ΔH is the enthalpy change of reaction (1). Note that equation (3) does not express a linear relationship between $\ln K_D$ and T. A linear relationship, if desired, may be obtained as follows :—

$$\frac{\partial \ln K_D}{\partial(1/T)} = \frac{1}{R}\frac{\partial(\Delta G/T)}{\partial(1/T)} = \frac{\Delta H}{R} \qquad (4)$$

Provided ΔH remains independent of T, as it does in some experimental systems, a plot of $\ln K_D$ against the reciprocal of absolute temperature must produce a straight line.

Hence the supposed linear relationship between $\log_{10} K_D$ and T (Frost, 1962, p. 434) finds no support in theory. Provided a displacement of K_D can definitely be attributed to a variation in T (and not to variation in the Mn and Ca content of garnet), an explicit linear relationship can be expected only if the abscissa is the reciprocal of absolute temperature (not temperature or metamorphic grade) and the ordinate is $\ln K_D$ (not $\log_{10} K_D$).

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