the sheeted complex and lower pillow lavas of Troodos have $SiO_2$>52.5% and FeO*/MgO>2.0 whereas most ocean ridge volcanics have lower values for these quantities. This is "decisive" evidence against an oceanic origin for Troodos but is consistent with formation in an island arc (although it is also consistent with a continental origin). Moreover, Miyashiro says that he "cannot understand" why the geological information quoted by Hynes supports the existence of accreting plate margins in the vicinity of Mesozoic Greece, arguing instead that this evidence is "so ambiguous" that it could be taken to support either view.

More generally, Miyashiro suggests that the world's ophiolite sequences are so diverse that some were probably created in island arcs, others at oceanic ridges and yet others in hot spots and stable continents. This point is well taken by Moores (*Earth planet. Sci. Lett.*, **25**, 223; 1975) who nevertheless feels that Miyashiro is wrong about the particular example of the Troodos Massif. Moores notes, for example, that much of Miyashiro's case is based on the premise that calc-alkalic rocks are rare in the ocean floor. But the fact is that most of the rocks obtained from the deep ocean floor have been dredge samples which are predominantly surficial and thus not representative of the oceanic crust as a whole. In this respect, Miyashiro has not been comparing like with like. When a crustal cross section has been sampled (for example, on faults in the Mid-Atlantic ridge and the Ninety East ridge), the rocks have been found to have compositions comparable to those of ophiolite sequences.

On the question of alteration, Moores has re-examined sections of the Troodos rocks analysed by Moores and Vine (*Trans. R. Soc.*, **A268**, 443; 1971) and finds that almost all of the rocks are indeed altered, most of them having abundant vesicles filled with zeolites or silica and many representing greenschist facies metamorphic assemblages. Thus in spite of claims to the contrary, Miyashiro has apparently been citing data influenced by metasomatic alteration. Primary $SiO_2$-rich igneous rocks do exist in ophiolites; but they account only for a very small proportion of the total volume and are significantly different from the genuine calc-alkilic rocks found in island arcs, especially insofar as they have much lower K/Na ratios. Moores thus concludes that bulk chemistry is not an adequate basis for differentiating between the products of ridges and island arcs and that recourse must be made instead to structural and stratigraphic arguments.

In a much longer reply to Moores, Miyashiro (*Earth planet. Sci. Lett.*, **25**, 227; 1975) attempts to refute his opponents' arguments in both general and specific terms, on the basis both of "logic and evidence". On the question of ocean floor sampling, he claims (with evidence) that the Ninety East ridge is atypical of oceanic ridges and that there are no grounds for assuming that the mid-Atlantic ridge fault represents a particularly deep section of oceanic crust. At the same time, sampling is not as unrepresentative as is sometimes claimed because rocks formed at depth are exposed along median valley walls and in transverse fracture zones across oceanic ridges. On the question of alteration, he presents additional data to support the view that Troodos compositons cannot be attributed to weathering, metamorphism or metasomatism but arise in-

# Anions and membranes

*from a Correspondent*

THE identification, isolation and partial characterisation of a human erythrocyte membrane protein essential for anion transport (Cabantchik and Rothstein, *J. Membrane Biol.*, **15**, 207–226 and 227–248; 1974; Ho and Guidotti, *J. biol. Chem.*, **250**, 675–683; 1975) provides strong support for Gunn's hypothesis that the various anion exchanges observed in the red cell occur through the passive diffusion across the membrane of a common carrier–anion complex. It is of interest to compare this work with the recent work, particularly that of Narumi's group (*Biochim. biophys. Acta.*, **311**, 80–89 and 90–97; 1973), on the mechanism of HCl secretion by gastric mucosal cells. As part of this mechanism internal bicarbonate is exchanged for external chloride, a situation analogous to that in the red cell, but in contrast to the red cell there is evidence of an active bicarbonate transporting system.

For many years it was thought that simple anions crossed the red cell membrane, diffusively, as free ions and that the differential permeability of the membrane to cations and anions was the result of fixed positive charges within the membrane. This theory was particularly attractive in the light of Bangham's work showing that the permeability of a simple lipid bilayer to different ions could be altered by changing the surface charge of the bilayer. A less attractive feature of the theory was the high activation energy of ion transfer.

This activation energy was closer to those usually associated with active ion transport. When it was shown that the rate of transport of several anions reached a saturation level as the ion concentration was increased the free ion diffusion theory had to be replaced by one involving a carrier ion complex. Cabantchik and Rothstein showed that the carrier was a glycoprotein by treating red cells with a non-penetrating fluorescent marker (SITS), and demonstrating that sulphate permeability was inhibited when a relatively small population of sites on the external side of the membrane were labelled. They then isolated the label by proteolytic digestion and showed it to be attached to a glycoprotein of molecular weight roughly 9,500. Ho and Guidotti have also isolated a glycoprotein of similar size which is responsible for phosphate transport and identified it with Bretscher's component a. It is tempting to assume that these proteins are identical and are also responsible for the physiologically important chloride–bicarbonate exchange but this has yet to be shown.

As the mechanism of anion exchange in the red cell has become clearer that of the gastric mucosal cells has become more complex. The presence of a bicarbonate ATPase in gastric mucosa, and several other tissues where bicarbonate transport is important, has suggested the presence of a bicarbonate transporting mechanism analogous to the ATP-dependent cation pumps. The demonstration by Namuri and Maki that carbonic anhydrase, which is necessary as a source of bicarbonate ions in gastric mucosal cells, is activated on phosphorylation by a cyclic AMP-dependent kinase would seem to complete the picture by linking the bicarbonate pump to hormones which control gastric secretion and which are thought to act by increasing the level of cyclic AMP in the acid-producing cells. The major problem in this picture is the localisation of the ATPase: most of it is found in the mitochondrial and not the plasma membrane fraction (Soumarmon, Lewin, Cheret and Bonfils, *Biochim. biophys. Acta.*, **339**, 403–414; 1974). But a bicarbonate pump in the mitochondrion would itself be of interest and the isolation of this ATPase as a Triton extract may indicate that a comparison between a passive and active carrier for the same ion will soon be possible.