From these findings the author draws the conclusion "that there must be more than one factor controlling the distribution of whales in the Antarctic."

In view of the nature of his material Dr. Mackintosh has drawn his conclusions with great caution, and his report is very valuable and useful as the first general account of the macroplanktonic conditions in these parts of the Antarctic.

Johan T. Ruud.

H. H. Gran. "Studies on the Biology and Chemistry of the Gulf of Maine. II. Distribution of Phytoplankton in August, 1932". Biol. Bull. Vol. LXIV, No. 2. Lancaster, Pa., 1933.

Professor Gran's study is primarily an account of eight stations made in August 1932 in the Gulf of Maine. The phytoplankton was investigated by the centrifuging of water samples. Vertical hauls of the fine plankton-net were also used for preliminary investigations. Of these eight stations, five were made in the deep basin of the Gulf and three over Georges Bank.

The same species were found in both the areas mentioned, but in the deep basin a Peridinian community was the dominant one, whereas over the Georges Bank the Diatoms occupied this position. In both areas, also, the counts suggest that photosynthesis had its maximum between twenty and thirty metres. In the Georges Bank region, though there was a slight maximum between twenty and thirty metres, the Diatoms had been distributed by vertical mixing fairly evenly throughout the water columns investigated. This mixing was especially well shown by the distribution of the littoral Diatoms. Over the deeper water on the other hand, the temperatures showed that thermal layering had taken place, and Professor Gran seems to explain the preponderance of Peridinians as due to an accumulation the result of active movement to the depths where optimal life conditions prevail. However this accumulation takes place, an examination of the catch shown in Table I and coming from station 1330 over the deep water shows that the Peridinians were absolutely more important than the Diatoms at the surface. At this station with rather warm surface water, there were 11 species of Peridinians and 14 of Diatoms. On another station, where layering was not so complete, the surface not so warm, and the nutrient salts higher, there were 25 species of Diatoms to 16 of Peridinians. A Georges Bank station, at which mixing had taken place and at which there was a still lower surface temperature, had 19 species of Diatom and only 9 Peridinians. It seems as if the Peridinians can live better than the Diatoms in warm water, poor in nutrient salts. Perhaps on account of their motility, they are able to strip more food from a solution otherwise fatally low in concentration.

A second part of the paper is concerned with culture experiments to determine the effect of soluble iron compounds on the growth of certain species of Diatom. *Chaetoceras compressus, Leptocylindrus danicus, Nitzschia delicatissima, N. seriata* and *Rhizosolenia alata* all showed increased growth in sea water to which had been added nitrate and phosphate. While *Rhizosolenia alata, Chaetoceras compressus* and *Nitzschia delicatissima* showed no greater growth in solutions containing nitrate, phosphate and soil extract, or nitrate, phosphate and ferri-ligno protein, or the latter with manganese [in a second experiment with oceanic water *Leptocylindrus danicus* showed successive additional growth increments with the first and third culture solutions. *Nitzschia seriata* showed increased growth in the solution

containing nitrate, phosphate and soil extract, a less increase in the nitrate, phosphate, ferri-ligno protein and manganese] and the least, but none the less considerable one, in the nitrate and phosphate culture.

R. S. W.

H. Kuenen. "Geological Results. Pt. 2. Geology of Coral Reefs." "Snellius" Exped., Vol. 5. Utrecht, 1933.

Our author is quite frank, stating that he "spent about 40 days studying reefs and reef formations scattered all over the eastern part of the Dutch East Indies." Some 31 places were visited so that his work was of the nature of a rapid recognaissance and his charts, "simple compass and pacing surveys", show few soundings. His own observations appear to have been confined to islands and coasts and are thoroughly interesting. These entitle him to draw his conclusions as to the movements of land in respect to the sea level and as to the phenomena of the reef surface — and all careful working founded on a researcher's own observations is valuable. But our author goes further and presents a chapter "On the Formation of Atolls" which is the consideration merely of charts (to which are perhaps added the observations of others) as he himself states. I am constrained to refer to this, for my views are frequently mentioned and I cannot regard this chapter, the only one which deals with matters of importance to the question of coral reef formation, as of greater value than if written by a geologist who has never seen a coral reef. The observations made by many observers in many parts of the world are so long-continued, detailed and careful that the deductions from them cannot be skimmed in a light style. The charts here given from the East Indies are not reproduced with sufficient detail to make the topography of our author's atolls comparable with that of several well-surveyed oceanic atolls. Perhaps I am misunderstanding and our author only wishes to apply his remarks to the East Indies; and this I suspect to be the case, since I do not find any works of Murray, Agassiz and above all Dana even referred to. The theory of subsidence was applied by Darwin to *all* barrier reefs and atolls but what geologist would now claim for it such a world wide applicability? For my part while I think the foundations of many atolls have subsided, I claim that there are agencies at work to-day on coral banks that *can* transform such into atolls without movement of the earth's crust below. I do not go further and the author's *own* observations do not traverse a single one of my contentions.

Turning now to Dr. Kuenen's article, there are recognised three small negative oceanic movements of 4—5 m., 1½—2 m. and ½—1 m., seen in three sets of levels. He is cautious but he evidently believes that these negative movements are as universal in the East Indies as I showed them to be in the Maldives in 1902. Indeed, the coral islands of these two very diverse regions seem to be remarkably similar in their coastal losses in exposed situations, in the surface structure of their reefs and in their islets washed up as banks in protected situations. In effect, it would appear that the East Indian low coral islets and their reefs are in no essentials different from those of the rest of the Indo-Pacific. Abrasion of elevated reefs on their coasts is believed to be the result of solution by the surface layers of sea water. It is pointed out that solution does not extend below low tide level, but surely on an exposed coast there is little or no surface down to 20 fathoms or so that is not covered by living organisms, through the flesh of which solution could not act. In protected situations, as in the East