toid containing 38.5 percent of P_2O_5 is griphite (*4*).

However, assuming for khoharite $a = 11.637$ Å, it is possible to calculate (*5*) the amounts of P and S atoms that would have to be substituted for Si in order to obtain the measured value 11.51_5 Å, and these amounts are by no means insignificant. Although it is difficult to believe that Mason *et al.* (*2*) could have missed a major constituent on analysis with the electron microprobe, there is something enigmatic about the failure of the composition to conform to the theoretical requirements for a garnet structure. The question remains: Did Mason *et al.* (*2*) discover a complex isomorphic varient of khoharite? If so, what is its composition?

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Alternative Explanation of the Garnet Occurrence in a Meteorite

Recently, Mason, Nelen, and White reported (*1*) the occurrence of a garnet in the Coorara meteorite. They found this garnet to have a lattice constant of 11.51_5 Å and suggested that it may have the probable composition, $Mg_3Fe_2Si_3O_{12}$. I believe that this composition is very unlikely for a garnet with the given lattice constant and that if this garnet contains only Mg^{2+}, Fe^{3+}, Fe^{2+}, and Si^{4+} cations, it must have a substantial amount of octahedrally coordinated Si^{4+} ion in it with an approximate formula

$$\{<Mg,Fe^{2+}>_3\}$$
$$[Fe^{3+}_{0.4}<Mg,Fe^{2+}>_{0.8}Si_{0.8}](Si_3)O_{12}$$

where the outer braces have the meaning given by Geller [Table 1 in (*2*)], and the pointed brackets indicate a mixture of different ions (in this case, Mg^{2+} and Fe^{2+}) and of presently unknown amounts.

In addition to the lattice constant, Mason *et al.* give the results of an electron microprobe analysis, but the composition thereby obtained is not in agreement with the garnet formula they suggest. They offer the explanation that the microprobe beam may not have sufficient resolving power to give a correct analysis in this case; otherwise, they "must consider the possibility of a nonstoichiometric composition, maybe a 'stuffed' garnet with a total of six 6- and 8-coordinated cations for 12 oxygen ions instead of five as in the normal garnet structure." Apparently, the formula for the suggested "stuffed" garnet would be

$$\{Mg_3\}\ [Mg_{1.5}Fe_{1.5}]\ (Si_3)O_{12}$$

(For electrostatic charge balance the ion would be divalent.) It should suffice to say that the garnet structure cannot contain this excess of cations; there is simply no space for them in the unit cell, nor would the space group allow their presence. Nevertheless, I must accept the suggestion that the composition obtained by the microprobe analysis is probably not correct.

I am quite confident that the lattice constant for the garnet is far too small to be that of $Mg_3Fe_2Si_3O_{12}$. Unfortunately, although the synthesis of a garnet with this formula has been reported (*3*), its lattice constant was not reported, but it is not too difficult to predict what it should be. I do this in the following way: Table 1 shows that there is an almost constant difference of 0.20 Å between the lattice constant of an

$$\{A_3\}\ [Fe_2]\ (B_3)O_{12}$$

and

$$\{A_3\}\ [Al_2]\ (B_3)O_{12}$$

garnet. The lattice constant of $Mg_3Al_2Si_3O_{12}$ is 11.46 Å; therefore, the predicted value of $Mg_3Fe_2Si_3O_{12}$ is 11.66 Å. [I should point out that the lattice constant of $Mn_3Fe_2Si_3O_{12}$ had been successfully predicted (*4*) in this way.] This is 0.14 Å larger than that reported for the Coorara meteorite garnet.

Recently, Ringwood showed (*5*) that a garnet with homogeneous composition equivalent to the formula

$$\{Mg_3\}\ [Al_{0.8}Mg_{0.6}Si_{0.6}]\ (Si_3)O_{12}$$

prepared at 250 kb and 900°C has the lattice constant 11.477 Å. He also showed that this garnet begins to form at a pressure of about 90 kb, and its formation is virtually complete by 110 kb (all at 900°C). In this case, some of the Si^{4+} ion has 6-coordination; in stishovite (*6*), a high-pressure modification of SiO_2, all the silicon has 6-coordination. The replacement of one Al^{3+} by one Fe^{3+} ion per formula unit causes an increase of 0.10 Å in the lattice constant; thus a garnet

$$\{Mg_3\}\ [Fe_{0.8}Mg_{0.6}Si_{0.6}]\ (Si_3)O_{12}$$

would have the lattice constant 11.56 Å, which is only about 0.04 Å larger than that reported for the garnet found in the Coorara meteorite. Replacement of Mg^{2+} by small amounts of Fe^{2+} in either the octahedral or dodecahedral sites would not increase the lattice constant substantially, so that the garnet could contain more Fe and less Mg. A further decrease of Fe^{3+} ion and increase of $[Mg^{2+}, Si^{4+}]$ in octahedral sites would decrease the lattice constant further. In fact, on the basis of the arguments given, a more likely formula would be that given at the beginning of this communication.

Table 1. Lattice constants (*2*) of $[Fe^{3+}]$ and $[Al^{3+}]$ garnets.

Garnet	a (Å)	Δa (Å)
$Ca_3Fe_2Si_3O_{12}$	12.059	0.208
$Ca_3Al_2Si_3O_{12}$	11.851	
$Mn_3Fe_2Si_3O_{12}$	11.82	.20
$Mn_3Al_2Si_3O_{12}$	11.621	
$Ca_3Fe_2Ge_3O_{12}$	12.320	.200
$Ca_3Al_2Ge_3O_{12}$	12.120	
$Mn_3Fe_2Ge_3O_{12}$	12.087	.185
$Mn_3Al_2Ge_3O_{12}$	11.902	
$Cd_3Fe_2Ge_3O_{12}$	12.261	.184
$Cd_3Al_2Ge_3O_{12}$	12.077	
	Average	.20

Mason *et al.* (*1*) have already indirectly indicated the low probability for an olivine-garnet transformation. Ringwood (*7*) has not only demonstrated the olivine to spinel transformation, but also the likelihood of pyroxene to garnet transformation (*5*). Is it not possible that the garnet in the meteorite replaced pyroxene + olivine instead of olivine alone? An example of a possible reaction for such an occurrence is

$$7\ Mg_{1.5}Fe_{0.5}SiO_4 + 5\ MgSiO_3 \xrightarrow{+\,O} 2\{Mg_3\}\ [FeMg_{0.5}Si_{0.5}]\ (Si_3)O_{12} + 5\ Mg_{1.7}Fe_{0.3}SiO_4$$

This is just an example; compositions of the reactants may be easily varied to fit the analyses if they could be accurately made. We must still have a source of a small amount of oxygen for the $Fe^{2+} \rightarrow Fe^{3+}$ conversion, but this scheme requires even less such oxygen than that suggested by Mason *et al.* (*1*).

In any case, I believe that the lattice constant of the garnet found in the meteorite precludes its being $Mg_3Fe_2Si_3O_{12}$ as suggested by Mason *et al.* (*1*); to account for its low value requires the presence of Si^{4+} ions in octahedral sites if no other cations than Mg^{2+}, Fe^{2+}, Fe^{3+}, and Si^{4+} are present.

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8. I thank M. Blander for discussions and B. Mason for his kind remarks on the manuscript.

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Size-Frequency Distributions of Two Intertidal Species

In Jackson's (*1*) paper on the distribution and size frequency of the two intertidal clams *Gemma gemma* and *Mulinia lateralis* the date (November ?) of the collections is omitted, and it is not even quite clear whether the clams were collected on one or several occasions. When comparing size frequencies of living and dead populations it is important to know the exact time of the year when the living population was sampled. Because many mollusks have different growth rates from one year to another, one would also like to know the year of collection.

In comparing the distributions of size frequencies of living and dead (complete shells) of *Gemma gemma* Jackson concludes that the two peaks in the dead population correspond to the period in summer when growth of both the 1- and 2-year-old animals is most rapid and mortality (apparently) is at its highest. It is much more likely that the peaks represent deaths during the winter period (September through March) (*2*) when growth is virtually at a standstill. Periodic cessation of growth by itself, even without an increased rate of mortality, is sufficient to translate size-frequency peaks, caused by periodic recruitment to a living population, into similar peaks in the dead population (*3*). The left shift noticed by Jackson in the peaks of the dead population as compared with those in the living population could be the result of either the time of sampling or a slower rate of growth during life of the shells of the dead population, compared with those of the living population. Sellmer (*2*) emphasizes the variability in the average size attained by *Gemma gemma* in its 1st year of growth (1956 cohort 1.8 mm; 1957 cohort 1.3 mm).

A positively skewed size-frequency distribution of single valves of *Mulinia lateralis*, according to Jackson, indicates very high juvenile mortality. Such distribution might equally well be the result of sorting of a particular size of valve by currents.

Jackson's conclusions that "size-frequency distributions of bivalves, even roughly similar forms from the same environment, may differ greatly because of simple and basic variations in their life histories . . ." and that "generalizations on the paleoecological significance of one sort of size-frequency distribution or another seem inappropriate without some idea of the life histories of both species involved . . ." are undoubtedly true. However, they do not seem to be substantiated by his study of a reworked sample of *Mulinia lateralis* and of a collection of *Gemma gemma* which may or may not have been *in situ*.

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Craig and Oertel are correct in pointing out the importance of the time of sampling, which was unfortunately omitted in the revision of my paper. The samples were collected on 9 November 1967. However, if variation in molluscan growth rates from one year to the next is significant for the formation of a size-frequency distribution for a dead assemblage, then hopes for paleontological application of size-frequency data must be slim indeed.

It is difficult to understand how Craig and Oertel can refer to Sellmer (*1*) in stating that the peaks in the size-frequency distribution of dead *Gemma gemma* represent deaths during the winter period. Examination of Sellmer's life table for *Gemma gemma* (*1*, p. 201) shows maximum mortality in summer months (nearly 80 percent in the 1st month after release), a considerable decrease in the winter, and a subsequent rise the following summer at the time of release of a new crop of juveniles. Craig and Oertel's deterministic models (*2*) are interesting but do not seem to apply in this case.

Craig and Oertel are concerned about the slight left shifts in the size-frequency peaks for dead *Gemma gemma* relative to the live population. Their concern is inappropriate, however, inasmuch as Sellmer (*1*) and I (*3*) have shown that the size-frequency distribution for dead *Gemma gemma* should be fairly stable and therefore more indicative of the life history of the species than the rapidly changing (*1*) live size-frequency distribution.

After stating in one paragraph that the size-frequency distribution for dead *Mulinia lateralis* might "well be the result of current sorting," Craig and Oertel conclude that the *Mulinia* were reworked and the *Gemma* "possibly *in situ*." Such a statement seems a bit strong, since, to the best of my knowledge, neither of the authors has visited the collecting locality. More to the point, however, it is true that size-frequency distributions can be generated by either current sorting phenomena or biological factors, and therefore size-frequency distributions do not alone provide a reliable basis for interpretation of fossil assemblages or of my Recent assemblage. Instead, as pointed out by Johnson (*4*) and supported by the results of my own study (*3*), the only reliable approach is to consider all available lines of evidence for analysis of assemblages *in situ* as compared to those transported. The fact that the same type of size-frequency distribution can be generated by a variety of biologic and selective factors is the critical difficulty in the paleontological application of size-frequency analysis.

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