(1957). Because the uniaxial indicatrix is an ellipsoid of rotation it is impossible to establish a cleavage direction in any uniaxial mineral by optical means alone, unless the cleavage happens to be parallel to (00.1). This remains true regardless of the *kind* of optical measurement and the *number* of measurements taken. Hence the present paper does not produce any new and unequivocal evidence either for or against the existence of cleavages parallel to *r* and *z*, and the writers' conclusions are entirely unwarranted.

The occasional observation of fracture surfaces parallel to *r* and *z* reported in the literature may be regarded as evidence for exceptions rather than for the rule. If there should indeed exist cleavages parallel to *r* and *z* it should be possible to demonstrate the fact by breaking any single crystal of quartz with prominent *r* and *z* faces and show that the number of planar rupture surfaces parallel to *r* and *z* are either more numerous or more extensive than planar rupture surfaces in other orientations.

The question of the existence of cleavages other than (00.1) in uniaxial minerals can be settled only through the application of *x*-ray diffraction techniques, if the cleavages happen to be too poor to be observable in the hand specimen.

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REPLY TO "DISCUSSION OF 'CLEAVAGE IN QUARTZ' "

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Regarding the first of Dr. Hoffer's comments, we have little to say except that we find it refreshing to be accused of gathering too many data. We seriously doubt that three hundred measurements would yield results "practically identical" with ours. If it exists, we look forward to the publication of so excellent a short-cut.

From the second comment we are aware that Dr. Hoffer's position has altered from one of doubt " . . . the evidence for the presumably rational cleavages remains inconclusive . . . " (Hoffer, 1961, p. 96) to one of certainty " . . . the myth of the existence of cleavages parallel to *r* and *z* in quartz" (Hoffer, present discussion). Yet he cites no post-1961 evidence

to account for this change. His dissent therefore evidently stems entirely from the paper (Hoffer, 1961) wherein, using the atomic coordinates for Si and O in the low quartz structure, he computed the directions for 24 non-rational planes, each defined as parallel to two intersecting Si-O bonds (that is, to trios of neighboring nuclei such as O-Si-O or Si-O-Si). These 24 planes—grouped into six sets respectively at angles of 29°19′, 47°38′, 51°46′, 61°38′, and 78°56′ to the *c*-axis—he accepts as potential fracture planes. For one set of non-rational planes, θ equals 51°46′, a value like that for *r*, although unlike *r* they lie in the zone $(11\bar{2}0):(0001)$ —or its symmetry equivalents. This set, he concludes, may have produced the results attributed by us (Bloss, 1957; Bloss and Gibbs, 1963) to *r* and *z* cleavage. However, we question the likelihood that these non-rational planes of three-atom extent can be developed within the structure in sufficient area to control the optic orientation of 100–200 mesh-sized grains. Furthermore, Dr. Hoffer computed the "cohesive forces" for no planes other than the 24 non-rational planes, not comparing any other planes, let alone the various cleavage directions reported in the literature. As a matter of fact, the validity of his calculations of "cohesive force" is far from established. Nor are we reassured by his statement "The speculative nature in this assumption surely does not require extensive justification" (Hoffer, 1961, p. 99). Thus, Dr. Hoffer's evidence for considering the *r* and *z* cleavages as mythical rests on his calculation of a θ value of 51°46′ for a three-atom plane.

We do not believe that reports of *r* cleavage published by twenty different investigators (excluding ourselves) should be arbitrarily dismissed with the platitude that they are " . . . evidence for exceptions rather than for the rule" (Hoffer, preceding discussion). By contrast, no observations of cleavage parallel to Dr. Hoffer's non-rational three-atom planes have *ever* been reported to our knowledge. Yet Dr. Hoffer believes that such non-rational surfaces are produced so abundantly as to form the peaks in histograms which have been attributed to the *r* and *z* cleavages. This stand is in utter disregard of the universally accepted concept that "Cleavage is always parallel to a possible crystal face . . . " (Berry and Mason, 1959, p. 200).

We deny Dr. Hoffer's allegation that our study adds no new data regarding cleavage in quartz. In the study, a *pair* of possible cleavage directions could yield results identical to those for a second pair only if (*a*) the interfacial angle *i* is the same for each pair and (*b*) the same θ angle occurs in each pair. Thus two parameters must coincide to produce ambiguity whereas in the earlier histogram method coincidence of only one parameter (θ) might produce ambiguity. Additionally, the new method could permit a relatively weaker cleavage to become evident as a periph-

eral plane even though it seldom developed over sufficient area to produce planes of rest.

In our study we plotted as "theoretical points" (Bloss and Gibbs, 1963, p. 825–826) only combinations of planes from reported cleavage forms. We are aware that two of the non-rational 51°46′ planes, in combination, would yield theoretical points at the same sites as for *r* and/or *z* combinations—and that two of the non-rational 47°38′ planes would yield theoretical points at essentially the same sites as ξ_ξ points. However the combination of a 51°46′ plane with a 47°38′ plane would not produce theoretical points at the sites labelled ξ_z, ξ_r, z_ξ or r_ξ in our (1963) Figs. 3–6. Thus, for example, the cell in Fig. 3 containing 38 observations and theoretical point ξ_z, ξ_r can be explained to possess more observations than its neighbors on the basis of its representing combinations of ξ plus *r* and/or *z* but not on the basis of combinations of two non-rational planes.

Our empirical evidence and our bond-density calculations led us, at the 95% confidence level, to confirm the existence of *r* and/or *z* cleavages and of the ξ cleavage in quartz. We did not attain the 100% confidence level—but who does? We hope the reader will compare our paper with that by Hoffer (1961) so that he can judge for himself the validity of the allegations made in Dr. Hoffer's discussion.

Recent *x*-ray studies by Paulitsch (1963) apparently confirm cleavage parallel to $(10\bar{1}1)$ and $(10\bar{1}0)$—but not to any of Hoffer's postulated planes.

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