the thermal disintegration of ferrous anthophyllite results in the formation of aluminous hypersthene, cristobalite and water (Wittels, 1952).

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DISCUSSION OF "CLEAVAGE IN QUARTZ" BY F. D. BLOSS AND G. V. GIBBS

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There are two points in connection with the paper by Bloss and Gibbs (1963) which, in my opinion, require comment. The first concerns the relatively large number of measurements carried out by the authors which is likely to impress the uninitiated reader. It should be pointed out that the yield of *additional* statistical information per measurement diminishes quite rapidly as the number of measurements increases. Had the experiment been properly designed (from a statistical point of view) a total of three hundred measurements should have been more than adequate to give rise to practically identical results. The effort in carrying out the remainder of the 2269 measurements seems to be wasted, which is especially deplorable in view of the laborious nature of the measurements involved.

The second point concerns the conclusions of the authors which represent another attempt to perpetuate the myth of the existence of cleavages parallel to r and z in quartz. Their conclusions as to the cleavage directions do not follow logically from the result of their experiment and are no more justified than the conclusions arrived at in Bloss' earlier paper

(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