For these reasons, I strongly disagree with Gubler's implication that the test-sample size is theoretically critical to the determination of large-volume strengths. I do agree that for extreme-value predictions of large-volume strengths, the exact distribution of the lowest strengths is critical. If Gubler's analysis could lead to the determination of which of all possible distributions is the "natural" or most accurate one, more confidence could be attributed to the predictions. One result from Gubler's definitions of fundamental units is that it eliminates the normal distribution from consideration. If "(b) Each fundamental unit acts as a force-conducting element in the snow", then, by definition, there can be no fundamental units with zero strength since strength is needed to conduct force. The normal distribution with its finite probability of zero strength is thus eliminated. The field is not narrowed very much since it is still open to the log-normal and a wide variety of truncated distributions, but it does appear to me that a step has been made.

Rocky Mountain Forest and Range Experiment Station,
U.S. Forest Service,
240 West Prospect Street,
Fort Collins, Colorado 80526, U.S.A.
27 November 1978

R. A. Sommerfeld

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Sir,

An alternate statistical interpretation of the strength of snow: reply to comments by R. A. Sommerfeld

I agree with the first comment of R. A. Sommerfeld that it would be very convenient if the large-volume strength could be derived from measurements on smaller volumes. I only showed (Gubler 1978[a], [b]) that the extrapolation to larger volumes depends strongly on the link-strength distribution chosen. Sommerfeld remarks that technical reasons impede a determination of the exact distribution type for the strength of the fundamental units or test samples from field measurements. But if future experiments allow the determination of the strength distribution of the microscopic links defined by Gubler (1978[b]), an exact extrapolation from measured smaller-volume strength to large volumes would be possible. (If the snow under investigation is homogeneous in a macroscopic sense.) Concerning shear strength and Daniels' statistics, Sommerfeld seems to imply that his test samples are not conclusively identical with the fundamental units. So each test sample may consist of an unknown number of fundamental units. If the link number per test sample is high enough, Daniels' theory predicts a constant expectation for its strength independent of the number of links per sample. If Sommerfeld's test samples consist only of several links, he has to develop a method which enables him to determine the original link-strength distribution. Daniels' suppositions clearly require a logical definition for the links. The theory implies the existence of only two states of a link: completely broken or surviving. But Sommerfeld's test volumes may break in part during natural stress increase showing that they cannot be considered as the fundamental links. For these reasons, it seems to me that one has to know the strength distribution of the logical links in order for it to be permissible to apply Daniels' statistics. There still remains a second problem: the stress-rates applied to the test samples are at least three orders of magnitude higher than the natural stress-rates. This fact together with the well-known high stress-rate dependence of strength of snow indicates that it is not possible to determine ductile shear strength using the sampling methods described by Sommerfeld together with Daniels' statistics. The problems arising from natural macroscopic weak flaws which in general are not distributed at random in the potential avalanche starting zone are already discussed in the original paper (Gubler, 1978[a]).

Eidg. Institut für Schnee- und Lawinenforschung,
7260 Weissfluhjoch/Davos, Switzerland
2 February 1979

H. GUBLER

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