A reply

E. K. Morgenstern answers the criticisms of Emile R. Falkenhagen when he asked

"Is this the right approach to seed zone delimitation in Canada?"

Under this heading Emile R. Falkenhagen (Forest. Chron. 48: 9-10, 1972) criticized an article by myself and Roche entitled "Using concepts of selection to delimit seed zones", published in the Proceedings of the Second World Consultation on Forest Tree Breeding, Washington, D.C., 1969. He levels serious charges against this article that call for a reply.

First of all, Falkenhagen raises the issue of seed zone delimitation in general, neglecting the specific purpose and viewpoint of our paper. Our introduction placed the paper into perspective, mentioning the current situation in Canada when experiments are not yet available across the country and considering the question whether preliminary seed transfer rules can be developed on the basis of existing results. We therefore looked for procedures that could be applied uniformly to a variety of existing experiments, in full realization that better data will eventually become available from experiments with the major species replicated in the important forest regions. That data from these future experiments should be treated in a different way than data in our paper requires no further discussion.

The procedures applied by us are based on the general experience that natural selection is the dominant genetic process. Here Falkenhagen disagrees with us and many other investigators. Following the lead of Prof. K. Stern in Germany, we found evidence in our data indicating that parameters of artificial selection used in plant breeding can also be applied to the analysis of natural population grown in experiments. The original equation of response (R) to selection is $R = i\sigma_A h$, where i is the intensity of

selection, σ_A the standard deviation of additive genetic variance, and h the square root of heritability. Stern had used the relationship $b = i\sigma_A$, thus replacing the response, R , by the regression coefficient, b , derived from the regression of a response variable, y , on geographic latitude or elevation, x ; he had also assumed a heritability of 1. We tested the replacement of σ_A by $s_{y.x}$, i.e. the standard deviation of y for fixed x derived from the residual mean square, and came to the conclusion that this procedure leads to worthwhile results and provides useful information where σ_A is not available. Therefore $b = i s_{y.x}$ where i is the intensity of change per geographic unit (latitude or altitude). We also indicated the limitations: definite linearity is essential; this meant that the sample should be drawn from a restricted number of regions (p. 3 of our paper). Thus what we have done is merely replacing one standard deviation (namely that of additive genetic variance) by another (the residual mean square) — and we found that this procedure is useful under certain conditions.

Falkenhagen disputes the use of $s_{y.x}$. He writes on p. 9: "The authors have probably estimated their selection intensities from the residual variance about the regression of provenance means on latitude or altitude. The procedure is generally employed in provenance study." This statement appears to reflect a misunderstanding of what we have actually done. Of course we used provenance means to first calculate the usual regression equation, $y = a + bx$. Then we set $b = i s_{y.x}$. After i had been determined from the ratio $b/s_{y.x}$, the "proportion selected" was then read from a graph shown in Falconer's book on quantitative genetics, p. 193. The proportion selected corresponds to the percentage of the population that performs like its neighboring population 1 degree of latitude or 100 meters in altitude apart (our paper p. 2). Therefore we have used $s_{y.x}$ in our calculation of selection intensity, but together with other statistics,

and not in the incomplete way Falkenhagen indicates.

There are more of such unfounded criticisms. To give one example: on p. 9 Falkenhagen mentions that the experiments were not related, but a careful examination of our Table 1 should have indicated that this is not so and that the material in some of the experiments had the same origin.

One of the simple quantities used in our paper is the ratio b/y , i.e. of the regression coefficient and experimental mean. Expressed in % it reflects the percentage change in a character per unit of latitude or elevation. This was chosen by us to illustrate in the easiest fashion what provenance transfer means in terms of survival and height values. We believe that, in general, regression coefficients from experiments have a meaning and can be interpreted as a result of adaptation of populations to the photoperiods and temperatures at their place of origin. This point of view is well supported by the literature, particularly in northern climates. On this basis we claimed that origin differences of 1 degree of latitude or 100 meters in altitude may lead to changes in height or survival of 2-6% from the mean of all provenances grown in a certain locality. We therefore felt that seed zones should not be wider than about 2 degrees in latitude and 200 meters in altitude, unless long-term experiments are available to indicate that wider transfers show no harmful effects. This recommendation is similar to that given by many European and North American investigators and is therefore neither very startling nor very new. For the conditions in Canada, however, where the detrimental effects of wide provenance transfers are not yet everywhere apparent and the dangers of uncontrolled seed movement exist, it is wise to point these problems out and adopt such a conservative approach. As our paper explained, the losses avoided in this way are potentially greater than the gains made in the short run through tree breeding programs.