

CORRESPONDENCE

The Editor,

The Journal of Glaciology

SIR,

Electrical crevasse detectors

Thank you for the clipping you sent me containing Mr. Ward's review (*Journal of Glaciology*, Vol. 3, No. 22, 1958, p. 146) of my first article on the crevasse detector. This review is correct in all essential respects and brings the subject approximately up to date. I am enclosing for your retention a copy of the latest publication (The design of a crevasse detector for polar exploration. *Journal of the Franklin Institute*, Vol. 264, No. 5, 1957, p. 361-77).^{*} It is proposed that future development will take the form of simplification. We may have a third publication containing final test results in about two years.

I was in New Zealand returning from a short sojourn in the Antarctic when the news of Dr. Fuchs's successful return was published. Apparently he did not use crevasse detectors, yet he and Hillary were fairly successful in getting through by purely "mechanical exploration". On the other hand, Albert Crary used a crevasse detector throughout his 1400 mile (2250 km.) traverse of the Ross Ice Shelf, and told me he considered it indispensable. It remains to be seen what the best compromise will be between the two extremes.

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19 March 1958

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SIR,

Glacier advances apparent and real

I would like to comment on Dr. R. Streiff-Becker's conclusions regarding the advance of the Nisqually Glacier in your *Journal*, Vol. 3, No. 22, 1957, p. 151.

First he draws attention to a small nunatak—it is perhaps one-third of a mile (0.5 km.) from Camp Muir in what is generally termed the Muir snowfield or glacier—indicating that there is an increased exposure of this mass, as shown by a comparison of photographs 1951 and 1955. This is quite apparent; however, it also may be shown, with equal justification, in comparing 1953 to 1955, that there has been an increase in snow cover for this latter period.

To belabor this is fruitless, however, unless the precise time and conditions of photography are known. Even a week may produce quite striking changes in the appearance of snow fields. There is a seasonal pack of as much as 27 feet (8 m.) of dense snow, of unusually high moisture content, at Paradise (elev. approx. 5500 ft., 1700 m.). This melts entirely during the summer so that the extent of firn can easily be miscalculated depending upon the date of observations. Snow pack is probably much greater on Mt. Rainier to at least 8000 and perhaps to 10,000 ft. (2440-3050 m.) in elevation. The nunatak is approximately at 9000 ft. (2740 m.).

Furthermore, snow falls throughout the year at Mt. Rainier; I have seen the ground covered with new snow in August at the 5500 ft. elevation. Last year in August I was in the vicinity of the nunatak under discussion during a storm that left a foot (0.3 m.) of hard packed snow over that area. This snow persisted for a number of days on the rocks and longer over the old snow.

I believe therefore that there is a need for exercising caution in drawing conclusions from undated photographs. Although I agree with Dr. Harrison as to overall advance, his use of undated photographs is open to question: it may have caused Dr. Streiff-Becker and others, to reach erroneous conclusions.

Personal observations, made over the past four summers as a ranger-naturalist at Mt. Rainier National Park, lead me to differ with Dr. Streiff-Becker when he states that firn field levels are falling everywhere. Many trails, formerly snow-free early in the season, are now blocked by snow fields at least four years and probably older in age. I have found that the Paradise Glacier (to the east of the Nisqually) has made a very substantial growth in thickness since its low of some years ago. A pictorial record shows a high nunatak being overwhelmed by an ice cliff perhaps 75 ft. (23 m.) high.

I would criticize Dr. Harrison on a point which Dr. Streiff-Becker did not mention. He has

^{*} An illustration of the detector shown in this publication is reproduced on p. 290. *Ed.*

indicated (p. 681, Vol. 2, No. 19 of the *Journal*) that there was an "estimated lag of fifty years in the germination of coniferous trees at a definite trimline at the position of a very poorly defined moraine". This estimation does not agree with the findings of other investigators. Studies currently in progress, as on the Kautz (Mt. Rainier) where there is an adjacent source of seed, show lags of but a few years. In Alaska, D. B. Lawrence (Glacier fluctuations for six centuries in southeastern Alaska and its relation to solar activity, *Geographical Review*, Vol. 40, 1950, p. 202) found that "analysis of our ring counts from the stem bases of the sapling spruces revealed that the usual interval between the melting away of the ice and successful germination was three to five years on the ridge tops". This follows the general conclusion reached earlier by William S. Cooper in his work with vegetation in the Prince William Sound Region, Alaska (*Ecol. Monographs*, Vol. 12, 1942, p. 1-22): Lawrence said that a tree seedling at timberline might take a half-century after recession of the ice (*Transactions, American Geographical Union*, Vol. 31, No. 2, Ap. 1950, ref. on page 244). As the Nisqually terminus is now at or near the 4500 foot (1370 m.) level and timberline some 2000 ft. (600 m.) higher this estimate of lapse time does not fit the circumstances discussed by Harrison.

Having regularly conducted groups during the past four summers to see the Nisqually Glacier, I have an intense interest in its response to the increased snowfall and cool, wet summers of the past years. I anticipate making a detailed study of fluctuations in snow pack adjacent to the Nisqually as indicated by tree growth, this summer.

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31 January 1958

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SIR,

The comments by Professor Cross on Dr. R. Streiff-Becker's conclusions in "Glacier advances apparent and real" are in general quite valid and pertinent. The growth of the Nisqually and other glaciers in the Cascade Range is unquestionably genuine. Similar growth has also been reported from regions outside the United States. There is ample evidence to refute the belief that glaciers are shrinking everywhere. The tendency to seek other explanations for glacial advances and the reluctance to accept the possibility of climatic change, even for short intervals, are hardly justified.

Figures 2 and 3 in the original article were intended to show the nature of the changes in the active front in uniform intervals of two years. No discussion of the complex behavior at higher altitudes was included. However, the omission of complete dates for these photographs was an oversight. Figures 2 and 3c were taken 17 August 1955. Figures 3a and 3b were photographed 3 August 1951 and 12 August 1953. While an interval of two weeks can produce striking changes in the exposure of rocks above a snowfield, there are other more important factors which complicate the problem. Similar pictures taken at approximately two-week intervals throughout several seasons indicate that the altitude of maximum snowfall varies widely in different years. Accurate conclusions should not be attempted from the meagre data in three photographs.

With respect to Professor Cross's criticism of the estimated lag of 50 years in the germination of coniferous trees, used in dating the time of a previous advance, I would like to point out that the 1855 date is generally in agreement with historical glacial advances elsewhere in the world. This lag is not inconsistent with the measured lags of 35 and 50 years after the advances in 1883 and 1907. Agreement with other investigators in other regions would hardly be expected when a similar lag can be verified quickly by an inspection of the area vacated by the ice after 1907.

Soil conditions in the Kautz mudflow area and on Alaskan moraines are probably quite different from the conditions produced by the advance of a relatively clean glacier, followed by an abrupt retreat. The difference between germination lags below the Nisqually Glacier and in Alaska has been discussed with Dr. D. B. Lawrence and could be the result of a radical difference in available moisture. Chemical constituents of the rocks involved in a particular advance could also be a factor.

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