shock. On the second day, each animal received conventional passive-avoidance training in which they were punished with footshock (1.6 ma, 2 seconds) for stepping from the small lit to the large dark compartment; ECS (35 to 50 ma, 0.3 second) was delivered through ear snaps 1 second after training. Following ECS the animals were divided among five groups and were given different test procedures for the next 6 days. Two control groups, one given training but no ECS and the other given ECS but no training, received conventional testing (*6*), one trial per day, for the next 6 days.

The retention data are presented in Fig. 1; as step-through latencies increase, retention is taken to increase (*7*). In the groups given ECS, a significant reduction in step-through latencies relative to the trained controls (*Train*) is taken to reflect amnesia, and a significant increase in step-through latencies relative to the nontrained controls (*N.T.*) is taken to reflect recovery.

It can be seen from Fig. 1 (top panel) that we replicated the basic retrograde amnesic and recovery effects. Group *T.T.* (test trials), given conventional testing, one trial per day, showed amnesia until day 4, when retention finally recovered to the level of the trained controls. We also reaffirmed the importance of the test trials for recovery. Group *H.C.* (home cage), given no test trials but rather returned to their home cage for 4 days and then tested on days 5 and 6, showed no signs of recovery.

Data presented in Fig. 1 (lower panel) comparing the remaining three ECS-treated groups with the conventional recovery group (*T.T.*) provide support for the learning interpretation of recovery. Each group received a different manipulation calculated to weaken the instrumental conditioning properties of the test trials and in each case recovery was suppressed. For some animals (*D.P.*, direct placement) we eliminated the step-through response during testing. The animals were placed directly in the large dark compartment during the first four test trials, a 5-second exposure per trial, and then were given conventional test trials on days 5 and 6. For other animals we used two different procedures in an attempt to weaken the conditioned-aversive properties of the apparatus during testing. In one case (*A.S.*, altered stimulus) we changed the stimulus conditions from training to testing. During testing the animals stepped from the small to the large compartment. The small compartment was lit as usual, but the large compartment, which was dark during training, was now also lit. In the other case (*Ext.*, extinction), in an attempt to extinguish the aversive properties of the apparatus, we gave the animals prolonged exposure to the apparatus during the first test trial. Specifically, the animals, as usual, stepped from the small to the large compartment, but once in the large compartment they were confined for 8 minutes before being removed (*8*). On the next 5 days the animals received conventional test trials.

The results are clear: animals recover from retrograde amnesia as long as we preserve during testing both the step-through response and the conditioned-aversive properties of the apparatus (*9*). The implications are also clear: (i) test trials induce recovery by promoting learning and (ii) ECS disrupts storage of instrumental but not classical conditioning. Here, however, caution must be exercised. We do not mean to imply that storage of the classical association is indestructible. In fact, classical storage may be just as vulnerable as instrumental storage, but, unlike instrumental storage, may involve neural circuits outside the influence of ECS (at least 35 to 50 ma, 0.3 second). Indeed, others have found that removal of cortex in rats prevents instrumental but not classical conditioning, affirming at least a cortical distinction between the two associations (*10*).

Finally, although our conclusion regarding the differential sensitivity of the two types of conditioning to ECS is based on inferences from the recovery data, it should be emphasized that these data do not stand alone in support of the notion. Recently it has been reported that ECS delivered immediately after passive-avoidance training disrupts retention of skeletal (that is, step-through response) but not visceral (that is, heart-rate suppression) responses (*11*). Given that retention for each type of response calls for a different type of conditioning, skeletal calling for instrumental and visceral for classical conditioning, then these data, too, may be taken as evidence for the notion that classical associations lie outside the influence of ECS.

ALLEN M. SCHNEIDER
JONATHAN TYLER
DANIEL JINICH

Department of Psychology, Swarthmore College, Swarthmore, Pennsylvania 19081

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6. Conventional testing consisted of allowing animals to step from the small to the large compartment, not delivering footshock, and removing them 5 seconds later. If an animal remained in the small compartment for 180 seconds, the test trial was terminated.
7. A Mann-Whitney U test (two-tailed) was used for statistical evaluation. For each statistically significant result the *P* value was less than .05.
8. Control animals confined to the small lit rather than the large dark compartment during the first test day showed normal recovery, thereby ruling out the possibility that confinement per se is sufficient to prevent recovery.
9. We have also found that recovery can be obtained within four test trials by (i) reinstating normal stimulus conditions (that is, those stimuli operating during training) after the animals show no recovery with altered stimulus conditions and (ii) confining the trials, using a 1-minute intertrial interval, to the first test day.
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18 September 1973; revised 26 December 1973

North Carolina Glacier: Evidence Disputed

A field check of the grooves reported by Berkland and Raymond (*1*) indicates that they are man-made rather than glacial. Characteristics observed include the following: (i) a relatively narrow range in width, 1.5 to 2.0 cm [figure 2 in (*1*) shows not one 15-cm-wide groove but rather a single groove to the left of a triple groove]; (ii) the presence of a ferric stain in the bottom of some grooves; (iii) polish on groove bottoms but not on intergroove surfaces; (iv) the presence of at least one groove that continues partially down the vertical face of the outcrop; and (v) the absence of grooves in shallow lows of the outcrop surface [figure 3 in (*1*)]. These points plus the discovery of weathered wire cable of two diameters —1.1 and 2.0 cm—0.5 km downslope imply that the grooves are a product of building stone quarrying (*2*), logging, or survey activities during the last two centuries.

Berkland and Raymond neglected to utilize records from a weather station (elevation, 1615 m above sea level) also on Grandfather Mountain (*3*). Use of a moist adiabatic lapse rate of $-0.6°C$ per 100 m (*4*) and the 17-year average of the mean daily temperature for July of 16.6°C fixes the present 0°C July isotherm at 4381 m above sea level. If the elevation of the Boone Fork cirque (?) floor is at least as low as that of the grooves (1370 m above sea level), then during glaciation the 0°C July isotherm had to be 3011 m lower than at present, which implies that the glacial age temperature had to be 18.1°C cooler. This value is three times as great as those derived from glacial studies around the world (*4*).

Clearly, the significance of this discovery is to be questioned.

JOHN B. MCKEON
Department of Geology and Mineralogy, Ohio State University, Columbia 43210

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20 August 1973

Berkland and Raymond (*1*) have reported finding "an outcrop with glacial polish, grooves, striations, and chatter marks" in the valley of Boone Fork, a headwater of the Watauga River in North Carolina. They called this find "the first tangible physical evidence of Pleistocene glaciation in the Blue Ridge Province of the Appalachian Mountains." Having had some field experience in the southern Appalachians, including the area in question, we were surprised by this report and not satisfied by the evidence presented in the photographs [figures 2 and 3 in (*1*)].

We visited the outcrop on 18 September 1973 and examined and photographed the reported features. We found several dozen well-formed grooves on a group of outcrops at the site. We recognized the grooves as having been made by moving cables used in logging operations. They in no way resemble the polished, striated, and grooved outcrops we have seen in glaciated areas. Berkland and Raymond's grooves are straight, have a narrow range in width, and in cross section have the shape of a short, circular arc. In our judgment all of them could have been carved by the 0.875-inch or 0.75-inch (2.2-cm or 1.9-cm) steel cables that we found broken and rusting at several places below the outcrop. Most of the grooves are on small ridges and knobs where they would be expected if cut by a cable dragging loosely over the rock surface. An example is the row of short grooves shown on a narrow ridgelike protuberance in figure 3 (*1*). Individual grooves of this example are traceable in linear paths across other high points of the outcrop. Some grooves are next to or under small overhangs on the outcrop, like the pair of grooves shown in figure 2 (*1*). We found iron oxide stains in several grooves. The grooves are generally discrete features occurring in rows, pairs, or singly. They are separated by areas of solution-pitted, weathered, or lichen-covered rock, and the grooves seem to be younger than the solution features as well as at least some of the lichens. A few small smoothed areas were found between some of the grooves that could have been made by cables.

Grooves close to each other are almost exactly parallel. The general trend of all the grooves is downslope, parallel to a gully that adjoins the outcrop, as would be expected if the grooves were made by a dragging overhead cable system used to transport logs. The main grooved outcrop overhangs the slope below it, providing a rock shelter about 4.6 m wide. The overhang faces the head of the valley, and it is difficult to imagine that it could have survived glaciation.

After our observations of Berkland and Raymond's outcrop, we interviewed Charles Coffey, a retired lumberman now living in Blowing Rock. Coffey was employed in lumbering operations in the area for many years and had participated in the last logging of Boone Fork during the 1930's. He had been the operator of a steam-powered cable system called an "overhead skidder," a device used extensively in Boone Fork. This system involved several cables suspended between winches at one end and a tall stationary mast as much as several hundred yards upslope. In operation, slack cables were repeatedly dragged along the ground while the aerially suspended logs traveled down the mountain. It seems to us that such a system could have produced the grooves we saw on the outcrop. When asked whether the cables of a skidder could cut grooves, Coffey told us that we could find cable grooves on rocks all through the woods.

In our opinion, these features are entirely man-made and of recent origin. In view of the fact that their occurrence was the only tangible evidence described (*1*), no need exists for philosophical discussion of the possibility of alpine glaciation in the head of Boone Fork.

JOHN T. HACK
U.S. Geological Survey, 8300 Colesville Road, Silver Spring, Maryland 20901
WAYNE L. NEWELL
U.S. Geological Survey, National Center, 12201 Sunrise Valley Drive, Stop 920, Reston, Virginia 22092

Reference

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28 September 1973

We welcome the opportunity to reply to the technical comments of McKeon (*1*) and Hack and Newell (*2*) who dispute our evidence for glaciation in the southern Appalachian Mountains (*3*). There is a wealth of evidence—physical, biological, and paleoclimatic—to support such glaciation (*4*), but the grooves and polish we reported (*3*) no longer may be included with that evidence. Thus, although we disagree with many of the comments and observations of McKeon (*1*) and Hack and Newell (*2*), we concur with their conclusion that the grooves and polish were probably produced by repeated abrasion from logging cables. For our error, we apologize to our colleagues. However, we believe it is vital to demonstrate that our work was a sincere, scientific effort and thereby avoid unwarranted negative reactions to other investigations of glacial and near-glacial Pleistocene conditions in the southeastern states.

In February 1973 when we found grooves and polish on a bedrock outcrop on Grandfather Mountain, we considered this to be the best local evidence for glaciation, enhancing other accumulated evidence (*3, 4*). Later we discovered (in May 1973) similar grooves in a V-shaped valley at a lower elevation on the northwestern side of Grandfather Mountain—grooves which unquestionably were produced by logging cables.

Although we knew of logging operations in Boone Fork, we discounted a

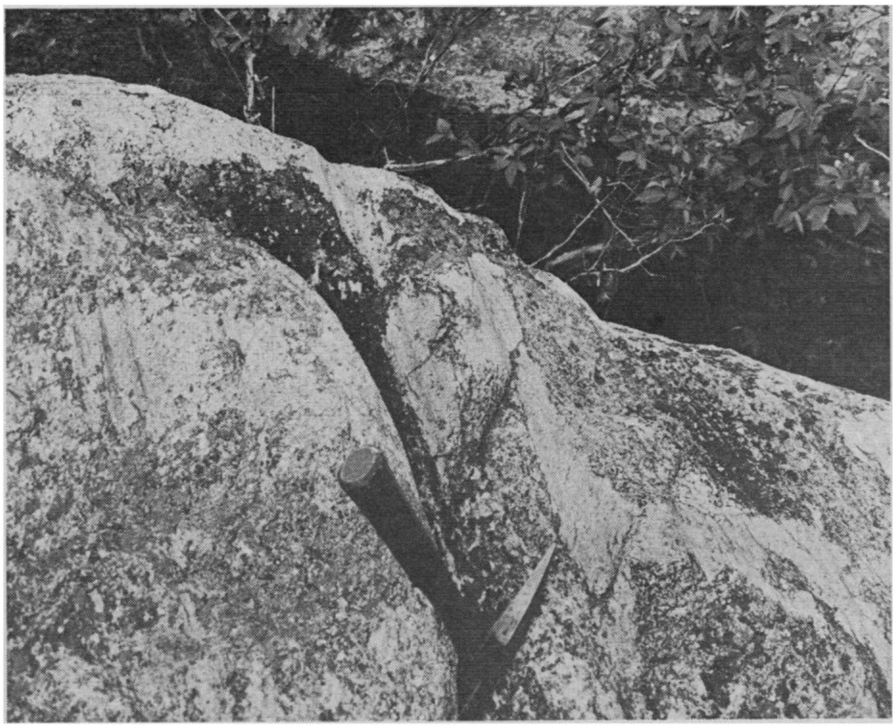

Fig. 1. Top of an overhanging outcrop at an elevation of 1370 m in "Boone Fork Corrie." Note the smoothed convex surface against the hammer handle and the polished planar surfaces between the grooves. These pseudoglacial phenomena are rapidly disappearing as a result of weathering, although they are only about 50 years old rather than 15,000 years old as we had earlier thought. [Photograph by Richard Lowder]

cable origin for the grooves and polish there for several reasons. (i) Originally, grooves were known only in the cirque-like basin of Boone Fork. (ii) Intergroove polish is common in Boone Fork [a fact disputed by McKeon (*1*) but partially verified by Hack and Newell (*2*)]. This polish is well displayed in Fig. 1, as well as in figure 2 of (*3*). (iii) The orientation of the grooves is not directly downslope but trends toward the outlet of the basin. Hack and Newell contested this observation. However, the main valley trends N85°E, the side gully adjoining the outcrop trends S36°E, and the grooves, located on the north slope of the valley, trend between S80°E and S40°E with an average trend of about S60°E. The geometry of this configuration verifies our view. (iv) The grooves have a variety of shapes and sizes. Both McKeon (*1*) and Hack and Newell (*2*) have disputed this point, although Raymond's measurements of more than two dozen grooves show widths ranging from 1.5 to 5.5 cm and many grooves with parabolic rather than circular cross sections. (v) Convexly rounded and polished surfaces on the sides of overhanging ledges did not seem to support the concept of polish by logging cables. (vi) An 86-year-old logger, Wayne Harmon, familiar with local lumbering practices, examined our photographs and asserted that our grooves "looked natural" and that loggers valued their cables too highly to have allowed them to drag along rocks for any length of time.

We would now like to answer some additional objections raised by McKeon (*1*) and Hack and Newell (*2*). First, iron stain is present in some grooves (*1*, *2*) and also in nongroove areas. Second, McKeon's suggestion (*1*) that at least one groove continues partially down the vertical face of the outcrop was not verified during a joint visit to the outcrop by McKeon and Raymond. Third, it is true that the grooves occur on elevated portions of the outcrop (*2*), but soil and vegetative cover are minimal there, reducing attack by humic acids. True glacial grooves are often best preserved under similar conditions in areas such as the Rockies and the Sierra Nevada. Fourth, arguments on whether the polish or the weathering came first are often chicken-and-egg situations, possibly resolvable by detailed lichenometric studies. Finally, the fact that the main outcrop is overhanging (*2*) is attributable to postglacial frost action which has pried away many huge slabs of rock that underlie the outcrop and contribute to the block fields below.

Contrary to McKeon's assumption (*1*), we did make extensive use of the weather station data from Grandfather Mountain (*3*). However, we do not understand the basis for McKeon's paleotemperature interpretations which imply that glaciation is restricted to a maximum July isotherm of 0°C. If the average temperature of the warmest month were no higher than the freezing level, when could the necessary freeze-thaw activity take place at the bergschrund? Charlesworth has described as "manifestly erroneous" the concept that the snow line is controlled by the average annual isotherm of 0°C (*5*, p. 9). He pointed out that in the Sierra Nevada the snow line temperature is at the annual isotherm of 8.5°C and on Mount St. Elias, Alaska, it is 10°C. Such temperatures obviously require a much higher mean July temperature and illustrate the importance of orographic and precipitation influences upon the accumulations of snow and glacial ice. Charlesworth further described the temperature regime for cirques which would require July temperatures far in excess of freezing: "The optimum conditions for cirque development occur . . . in a zone in which the average annual temperature is somewhere near the freezing point" (*5*, p. 296).

Despite these contrary opinions on temperature requirements for cirques, McKeon's interpretation of the necessary Wisconsinan cooling in order for Grandfather Mountain to be glaciated is similar to our own. The mean July cooling of 18.1°C required by McKeon is excessive by several degrees, because it is based on the mistaken assumption that cirque glaciers need a mean July (rather than a mean annual) temperature of about 0°C. Yet his figure can be compared with the 18°C mean January cooling suggested by Watts (*6*) for a Pleistocene spruce-fir forest near Atlanta. It is also comparable with the mean annual cooling of 16.5°C suggested by Goldthwait (*7*) for near the ice front in Ohio, with the mean annual cooling of 15° to 18°C reported by Kaiser (*8*) for central Europe, and with the 15°C cooling of parts of the central United States suggested by Flint (*9*). Finally, Schroeder and Bada (*10*) reported a minimum cooling of 15°C in the southeastern United States during glacial times. Thus, McKeon's statement that for Grandfather Mountain to have been glaciated the necessary cooling would have been three times as great as those derived from other glacial studies must be regarded as not all-inclusive.

In conclusion, we wish to emphasize the reasonableness of Pleistocene glaciation in the southern Appalachian Mountains to at least some experienced Appalachian geologists. Wentworth (*11*) declared "The case against the hypothesis of local glaciation consists merely of lack of any other evidence than striated cobbles favoring it, and much detailed work with accurate maps is needed in the higher areas of the Southern Appalachians before this hypothesis can be discarded." Hack has studied the geology of the Appalachians more extensively than most workers. Yet Hack and Newell state (*2*) that they "were surprised" by the report of our discovery of evidence for Pleistocene glaciation south of the Laurentide ice sheet. The surprise could not have been of major proportions judging from a letter Hack wrote to us (*12*) prior to visiting the grooved outcrop on Grandfather Mountain. In the letter he stated: "We have known for some years through palynological evidence that the climate of the Appalachians was extremely poisonous as far south as Georgia . . . . Considering this fossil evidence, it is surprising that we haven't found evidence of glaciation in the southern Appalachians . . . it would not be surprising if some hollows were occupied by ice during the Pleistocene and glacial processes were active on a small scale."

In view of the reasonableness of the hypothesis of alpine glaciation in the southeastern mountains and for the sake of renewed interest in the subject by workers in many fields, we hope that this discussion will encourage open-mindedness on this issue. We also hope that we have demonstrated that a need does exist "for philosophical discussion of the possibility of alpine glaciation in the head of Boone Fork" (*2*).

JAMES O. BERKLAND
Department of Public Works,
Santa Clara County,
1555 Berger Drive,
San Jose, California 95112

LOREN A. RAYMOND
Department of Geography-Geology,
Appalachian State University,
Boone, North Carolina 28608

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4 February 1974

Structure of Silica Glass

In their recent report Konnert *et al.* (*1*) purport to find "tridymite-like regions" in SiO_2 and GeO_2 glasses. I believe that they have gone somewhat too far in attempting to specify the structure of the "crystalline regions," as their own data will show. "Tridymite" and "cristobalite" are names which describe two families of SiO_2 phases. The relationship between these two structural groups, as first pointed out by Bragg (*2*) and elaborated on by Flörke (*3*) and Jagodzinski and Laves (*4*) is analogous to that between wurtzite and sphalerite, the two-layer hexagonal (2H) and three-layer cubic (3C) forms of ZnS, respectively. In fact [for details, see Hill and Roy (*5*)], as noted by Konnert *et al.*, there exist a variety of tridymites. Just how wide a variety, they possibly did not suspect. Furthermore, these tridymites undergo a wide variety of phase transitions in the range from 25° to 150°C. The actual pure 2H end-member tridymite has been reported in some high-temperature patterns (*6*). The complexity of nomenclature and structural identity is, however, increased still further in two dimensions. First, the probability of random stacking faults and ordered polytypes between 3C and 2H is very high, and, indeed, a mixture of such structures always exists in most samples (*4*). In fact, Konnert *et al.* used for the powder pattern a sample which was a 20-layer orthorhombic phase. [Buerger and Lukesh have reported on a 20H phase (*7*).] The ordered mixtures of cubic and hexagonal stacking are, of course, structurally intermediate between cristobalite and tridymite. Presumably a 4H phase would still be a "tridymite." But, is a 15-layer rhombohedral phase to be called tridymite or cristobalite? When one prepares or heat treats at a high temperature compositions which easily form polytypes, one usually obtains a distribution of polytypes. These observations are intended to demonstrate the meaninglessness of the concept that in a disordered glass at the highest temperature all the possibly "crystalline" units would ever have the same stacking to a degree that would justify a specific label.

Moreover, an enormous body of experimental evidence both on the difficulty of making "tridymite" at all and on the universality of cristobalite-like phases being formed from every type of noncrystalline SiO_2 renders the model proposed by Konnert *et al.* physically implausible.

An even more serious difficulty, however, is found in deciding the degree of perfection of stacking. Because of the enormous activation energy for the stacking changes compared to the kT (k is the Boltzmann constant and T is the absolute temperature) range of the α-β transition in cristobalites (50° to 300°C), it has been shown that it is possible to prepare cristobalites with a tremendous range of stacking fault order [Flörke (*3*) and Hill and Roy (*8*)]. The structural differences in the infinity of these individual phases (which can easily be prepared and preserved indefinitely at room temperatures) find a ready indicator in the tetragonal-cubic transition in cristobalite. This can vary from below room temperature to 268°C (*8*).

Let us return now to the limitations which must be placed on the conclusions of Konnert *et al.* The first limitation comes, as noted above, from the fuzziness in the meaning of the word "tridymite." The use of the term "tridymite" simply cannot be justified on such a minor coincidence. The second limitation exhibits sharply the deficiency of the radial distribution function (RDF) method for structure analysis; this deficiency is only partly acknowledged in the last paragraph of the report. The RDF approach makes it possible to