ated with laminated slates and containing widely scattered erratic blocks of considerable variety, there is surely ample reason for suspecting glacial origin. I think the conditions on Beaver Creek not only justify much more than a mere suspicion of glacial origin, but entitle that hypothesis to strong preference, although not as yet to full acceptance.

In the canyon of the Yukon River, between Eagle and Circle, there are two outcrops of tilloid conglomerates, only one of which is mentioned by Capps. The one omitted is on the south bank six and one half miles west of the mouth of Nation River. There a massive bed of boulder clay 80 feet thick was deposited in the midst of a series of black shales and dolomites, presumably of marine origin. The gray clay is now a hard argillite sprinkled with pebbles and subangular boulders of various rocks. During the very cursory examination that we made no striae were found. The material closely resembles till in general appearance, but I do not urge the acceptance of that theory of origin. The place should be examined more thoroughly by others.

The locality cited by Capps is on the west bank of the Yukon River, eight miles north of Woodchopper Creek, and south of Circle. There a long cliff gives an almost perfect exposure of a layer of bouldery slate more than 100 feet thick. The rock is a dull gray argillite in which slaty cleavage is only moderately developed. The microscope shows the matrix to consist of a heterogeneous mixture of minerals derived from clay with angular fragments of such rocks and minerals as quartz, dolomite, chert and feldspar. Through this matrix, which is indistinguishable from that of many well-known tillities, there are sprinkled at random subangular pebbles and boulders of dolomite, chert, quartzite and several kinds of slate, ranging up to at least 30 inches in diameter. These erratics are wholly without arrangement, and the matrix shows no sign of stratification. Recrystallization of the matrix has caused it to adhere so tightly to the surfaces of the pebbles that in the course of an hour's careful search among hundreds of them only four were found that revealed even patches of the original abraded surface. However, each of these four pebbles of fine-grained dolomite show typical glacial polish and fine parallel striae, that have fully satisfied all the glacialists to whom I have shown them. Many of the erratics the surface of which could not be examined nevertheless were soled or faceted, and some even revealed the so-called flatiron shape which is peculiar to glacial abrasion.

Whether Capps or Mertie have ever examined this cliff with adequate care is not stated in their reports and they give no reason for the expressed opinion that they doubt the glacial origin of the deposit. To me it seems that the facts in the case leave very little ground for uncertainty. The glacial origin of the deposit appears to be no more questionable than that of the generally accepted tillites at Nan-tou in China, at Squantum in Massachusetts, in the Salt Range of India, or in the Cobalt district of Ontario. The evidence is essentially alike in all these cases.

On general grounds there would seem to be no reason for not expecting to find glacial deposits of many ages in Alaska. Had the geologist described desert or tropical deposits, a skeptical attitude would be more justified than in reference to glacial beds in a subarctic region. Finally, the author's caution regarding the Paleozoic tillities in Alaska is rather out of harmony with his confident attitude toward a certain early Pleistocene deposit which he himself found in the Alaska range. This he describes[4] as "a deposit of deeply oxidized and weathered material that in composition, lack of assortment and shape of included boulders and blocks seems certainly to represent a glacial moraine. The included boulders and rock fragments, however, are all so weathered and decomposed that their original surfaces have been lost. No striae were found, but few of the rocks were firm enough to retain striae. I believe this deposit to be a remnant of an ancient glacial moraine." It is reasonable to ask whether the evidence in this case is even as good as in the Paleozoic deposits which he regards as doubtful, especially the one in the locality below Woodchopper Creek.

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IS AMMONIUM HYDROXIDE TOXIC TO COTTON PLANTS?

IN an article in the June 17 number of SCIENCE, Mr. Tiedjens has taken exception to conclusions given by us in an article on "Free Ammonia Injury with Concentrated Fertilizers."[1] The many misinterpretations of our claims and the weakness of some of the evidence he presents in contradiction of our conclusions force us to point out some of the major discrepancies.

In a previous article,[2] Tiedjens and Robbins contended that we could not have had any considerable concentration of free ammonia formed by the ammonification of cottonseed meal, as other investigators found that ammonification was a gradual process while nitrification was rapid. Our published data showed the reverse was actually the fact.

Tiedjens has said that our interpretation "would

[4] *Op. cit.*, p. 7.

[1] L. G. Willis and W. H. Rankin, *Ind. and Eng. Chem.*, 22: 1405–12, 1930.

[2] V. A. Tiedjens and W. R. Robbins, *N. J. Agr. Exp. Sta. Bul.*, 526, 1931.

imply that the cotton plant is peculiarly sensitive to extremely low concentrations of ammonia nitrogen." We had stated that "the injury is related to a high concentration of free ammonia in contact with the roots."

He grew cotton "in sand cultures with sulphate of ammonia, ammonium hydroxide and calcium nitrate, respectively. Ammonium hydroxide was supplied in a complete nutrient solution at pH 8." "The concentration of nitrogen as ammonia in these cultures was higher than that of the total nitrogen in the cottonseed meal which was employed by Willis and Rankin."

The cottonseed meal we used contained 6.02 per cent. of nitrogen. Ordinary concentrated ammonium hydroxide, with 26 per cent. NH_3 has about 20 per cent. of nitrogen. Tiedjens' cultures, in which cotton grew satisfactorily, must therefore have been made up to contain 30 per cent. of concentrated ammonium hydroxide. We question the accuracy of this statement.

No details were given of the method of limiting the cultures containing ammonium hydroxide to pH 8.0. We are of the opinion that in establishing this low value arbitrarily he has at the same time and by the same means reduced the ammonium hydroxide to a correspondingly low concentration. If we are correct, the results given by Tiedjens are valueless in support of his contention that ammonium hydroxide is not more toxic than other nutrients.

It appears that our rate of fertilization was not correctly understood, even though we stated that this "was calculated on the basis of all fertilizer being placed in furrows 6 inches wide, spaced 4 feet apart and with the fertilizer mixed with one inch of soil in the bottom of the furrow." The soil in the pots was considered to represent a section of the drill.

Tiedjens states that we used "only 16 pounds of nitrogen per acre." This is about the average initial rate of application for cotton in the field, but it should be evident that the local concentration in the drill is equivalent to eight times the apparent rate.

It was said of our results that they "were apparently obtained where the soil buffer systems was inadequate." Inasmuch as nearly half of the fertilizers used in this country are applied to soils having this limitation, we see in respect of the soil used no basis for criticism of any generalizations we have made.

We do not claim infallibility for our deductions, but we find nothing in Mr. Tiedjens' work that can logically be construed to be contradictory to any of them. We must therefore hold to the opinion that free ammonia was the cause of the injury we observed, until it is shown by competent evidence that the same effect could have been produced under the conditions of our experiment by a factor we overlooked. And until we have this evidence we must consider that the use of ammonium hydroxide as a fertilizer constitutes a hazard to plants, except under very strictly prescribed conditions.

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A POSSIBLE HORMONE-SECRETING REGION IN THE GRASS COLEOPTILE

CAREFUL experiments on phototropism in the coleoptiles of *Avena sativa* and other grasses, notably by Went and his associates, have apparently demonstrated the presence in this organ of a growth-producing hormone or "Wuchsstoff." An important aspect of the problem is a determination of the region where this substance is produced and the means of its transportation to the zone of bending. The only histological work to date is that of Tetley and Priestley,[1] who report that in coleoptiles of *Zea Mays* there seems to be no possible region for the production of such a substance, since all the tip cells of the coleoptile are highly vacuolate or seem otherwise to be inactive. These authors also believe that there is no possible path of transportation in the fibro-vascular bundles, as there is a strong upward movement of water and sap generally in the xylem, and the phloem elements are not yet fully differentiated. They conclude that phototropism here is not the result of any chemical substance but rather of differential tissue permeability on the two sides of an unequally light-stimulated coleoptile. They find no morphological regeneration in decapitated coleoptiles.

The hypotheses of the existence of a growth hormone evidently depends, at least in part, upon the demonstration of an appropriate histological structure for its production. In an endeavor to secure evidence on this point, experiments and histological studies have been carried on by the author with twenty genera of grasses as well as with certain dicotyledonous plants. The seeds were soaked in sterile distilled water and germinated in a moist chamber on moist filter paper in sterile Petri dishes. Some coleoptiles were studied directly, others after exposure to unilateral light. The length of coleoptile, time of exposure and other conditions varied with the experiments. Some ten thousand coleoptiles were used, mostly of *Avena sativa*, since this species is particularly favorable and has been the most commonly studied.

[1] U. Tetley and J. H. Priestley, "The Histology of the Coleoptile in relation to its Phototropic Response," *New Phytol.*, 26: 171–186. 1927.