work and whose financial resources are so limited that they can not reach the goal toward which they started.

May I add here that I am not appealing for fellowships in any particular branch of science but for *fellowships available in any field of science.* Too often when a fellowship is available only for chemistry, the recipient may be inferior in potential ability to some one who aspires to be a physicist when no fellowship is available in that field. Neither am I appealing necessarily for fellowships at Minnesota, for the student should be permitted to select the *men* under whom he wishes to work and not be limited to a university which will grant an assistantship permitting work toward a Ph.D. degree, for, after all, graduate schools are known by their works, by the *men* on the faculty and not by the name of the university or by the ornateness of the buildings. There may be only one man in America who stands preeminent in the field in which the student wishes to do major work, in which case the fellowship should permit the student to work with this man rather than waste his time on a second-rate instructor in another university where a fellowship happens to be available.

The Graduate Group Committee for Agriculture at Minnesota awards the three available fellowships, as nearly as is possible, solely on the basis of merit of the individual and not on the basis of departments. Such a committee having similar duties could well function in our sister institutions and would be a necessity in the event that additional funds for any considerable number of fellowships became available.

In closing may I add that while I rejoice at the increasing number of $1,500 to $2,500 fellowships which are being provided for *post-doctorate* fellowships, nevertheless I can not help wondering whether *five* $500 post-baccalaureate fellowships, to assist talented students to begin post-graduate work, might not yield greater scientific dividends than does a single $2,500 post-doctorate fellowship. In any event, I'd like to see the experiment tried, for I believe I can predict the results.

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CONE-IN-CONE ON CONCRETIONS FROM THE DEVONIAN OF NEW YORK

THE fact that the concretions occurring in the Devonian of New York[1] contain a well-preserved fauna and flora is in keeping with similar occurrences both in this country and abroad. This feature of fossiliferous concretions was pointed out in my article on concretions in the "Treatise on Sedimentations" (p. 514).

[1] A. Emil Alexander, "Devonian Concretions of Western and Central New York," SCIENCE, 68: 85, 1928.

Mr. Alexander notes that some of these concretions have "the peculiar cone-in-cone structure *for which no explanation has as yet been offered.*"[2]

I should like to call Mr. Alexander's attention to the fact that the occurrence of cone-in-cone on and in concretions is mentioned in the article referred to above (p. 502), and that the origin of this interesting structure is discussed in an article on cone-in-cone in the same volume (pp. 515–518). The origin is discussed in more detail in my paper on "Cone-in-Cone," in volume 4 of the *American Journal of Science* (pp. 199–213), where there is also another list of references in addition to those on page 515 of the "Treatise on Sedimentations." I think Mr. Alexander will find that an explanation for this structure has been offered.

Attention should also be called to the statement: "In the marcasite nodules, the iron pyrite has replaced the organic, as the case may be, by its silver white metal." It is not "iron pyrite" (for pyrite is a mineral separate and distinct from marcasite), but *iron disulphide,* that has replaced the organic material.

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WINTER ROOT GROWTH OF PLANTS

IN a series of investigations at the Boyce Thompson Southwestern Arboretum, it has been found that the roots of certain plants, generally thought to be dormant in winter, make definite, continuous growth at this season. This is true of both deciduous and evergreen species and embraces cultivated and wild forms. Notable examples are *Prunus persica, Prunus armeniaca, Covillea tridentata, Simmondsia californica, Cupressus arizonica* and *Opuntia laevis.* The rate of root elongation per day, of the species under observation, was found to vary from 9 mm in November, as the maximum, to .5 mm in February, as the minimum. Growth was evidently affected by change in the seasonal temperature of the soil, but there appeared to be no direct or close correlation between daily growth and soil temperature.

In the study of individual species the average daily root elongation of the peach (*Prunus persica*), covering the winter period between November 4, 1927, and March 31, 1928, was 2.10 mm. The average daily growth for November was 5.55 mm, December 2.01 mm, January 1.65 mm, February .90 mm, and March 1.16 mm.

In contrast to this group of plants showing decided growth in winter, other plants were found which, under the same environmental conditions, make no

[2] Italics mine. W. A. T.

root growth whatever at this season. Among these are *Citrus aurantium, Vitis vinifera, Prosopis velutina* and *Parkinsonia torreyana.* The period of root inactivity begins about the first of December and lasts until the latter part of March.

These observations were made by growing the plants in large wooden and cement boxes provided with plate-glass front, which made it possible for the roots to be easily seen and checked each day. Light was excluded by a well-insulated door, easily opened and closed. The boxes were made in two series, the smaller three by three feet square and holding twenty-seven cubic feet of soil, and the larger six by six feet square and holding 216 cubic feet of soil. The amount of soil in the latter case was sufficient to accommodate a tree, such as the peach or orange, for four or five years.

F. J. CRIDER

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AN UNEXPLAINED VISUAL PHENOMENON

THE following note is written at the suggestion of Professor J. P. C. Southall, of the Department of Physics of Columbia University, in the hope that some of your readers may be able to explain an observation that I made the other day. While coming down from Minneapolis with the Air Mail, I happened to notice that the propeller became visible upon turning the visual axes of my eyes laterally. The conditions of observation were as follows: I was seated in a cabin plane, about eight feet behind the propeller and about eighteen inches lateral to the median line of the plane. About half way between my seat and the propeller was an inclined windshield, forming a lateral angle with my visual axis of about 60° and a vertical angle of about 80°.

A setting sun was directly behind the tail so that no direct rays from the sun fell either into the cabin or upon the propeller. There was smooth air at about 2,500 feet with practically no lateral motion. The tachometer reading of propeller revolutions swung between 1,650 and 1,675 revolutions per minute.

The propeller was of white metal, about eight inches wide where it first became visible above the top of the cowl and tapered to four inches at the tip, which was about two feet beyond the point of the cowl line. It was a two-bladed propeller.

The observations were made with each eye alone and then with both eyes together without any variation being found. When the eye was in the primary position and the visual axis parallel to the axis of the plane, the propeller was absolutely invisible and there was not even a blur in the line of vision. As the visual axis was turned laterally (either right or left made no difference), a blur corresponding to the arc of the propeller became visible. Upon increasing the angle, there could be distinguished within the blur the general outline of the individual propeller blades in terrifically rapid motion. These were clearest at about 45° from the primary position. As the angle was increased still more, the individuality of the blades moved into a blur corresponding with that seen at first. At about 60°, the blur was entirely lost. It seemed that for about 10°, namely, between 40° and 50°, the blades were seen as individual, and for about 10° to either side of these limits, the blades were seen as a blur. These figures are of course approximate, as I had no means of determining the angles accurately. When the accommodation of the eye was relaxed by fixing upon objects in the landscape a mile or more away from the plane, the phenomenon was much more apparent than when the eye was accommodated for points upon the plane three to ten feet away.

I must confess that I am entirely at a loss to explain the observation and would appreciate any possible explanation.

HARRY S. GRADLE

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THE MEASUREMENT OF ULTRA-VIOLET RAYS

DR. OTTO GLASSER has taken it upon himself in the issue of SCIENCE for August 3, 1928, to criticize the report by Science Service printed in the same journal for May 11, 1928, of devices for the measurement of ultra-violet rays described by us. While we are not responsible for the text of the report criticized and while no explanation of such a criticism is necessary to any one familiar with the field, we should like to make the following statement. The note in SCIENCE is a report of a paper read before the American Roentgen Ray Society in Montreal, on September 27, 1927, and published in the February issue, 1928 (Vol. 19, p. 144) of the *American Journal of Roentgenology and Radium Therapy,* that is, three months before the report appeared in the May number of SCIENCE. In the original paper full credit is given to all previous workers and a more complete bibliography than is mentioned by Glasser is published. A marked reprint of our paper has been filed with the editor of SCIENCE. Since the original paper is known to all workers in the field and since the note in SCIENCE is merely consistent with all others there published in not giving references and bibliography, Mr. Glasser's criticism is incomprehensible to us.

ERNST A. POHLE,
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