Not all degenerative change is, of course, non-adaptive. Frequently, structures of adaptive significance arise in this way, as when the anterior segments of the polychaete fused to form the arthropod head.

Under normal conditions natural selection confines degenerative changes to minor body features, as details of sculpture or color pattern, but under abnormal conditions more profound changes are likely to occur. Cave conditions, as Wright himself mentions, are a case in point. In a normal environment selection tends to keep up to par the light receptive and light protective mechanisms, *i.e.,* the visual and pigmentation systems. In caves, on the other hand, such factors are relaxed so that these systems degenerate. Tactile organs, however, such an antennae and long body-setae, are encouraged, at least in the ground beetles, so that these structures thrive and are maintained in a high degree of development. Similarly explained are the degenerative changes that occur in sedentary and parasitic animals in general and the occasional loss of wings by birds and insects in certain insular environments. Darwin took account of the more extreme of these degenerative changes under the heading of the effect of disuse. But we have long realized that "disuse" in itself has no influence on the hereditary mechanism. It is merely evidence that the parts in question are no longer useful to the organism. They are, accordingly, no longer maintained by natural selection and so are subject to degenerative modification.

The picture that we get of evolution is somewhat as follows: Mutations constitute the basic material. The upbuilding of the complexity of organic function and structure and the thoroughgoing adaptation that all organisms exhibit are the product of natural selection. The detailed specific and generic differences are largely the result of differential mutational effects in partially or completely isolated populations. Selection effects a continuous influence, keeping the organism and its parts up to a certain par and preserving characters of adaptive significance as they occur. There are, however, always important portions of every organism over which selection effects no influence, and these are subject to degenerative changes in accordance with the principle that the loss of genes is more frequent than their gain. Still unexplained, perhaps, are certain so-called orthogenetic changes, like the suture system of ammonites, but the principle of degenerative evolution in the absence of continued selective control seems to be an important factor in understanding plant and animal diversity.

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LINNÆUS ON THE NATURAL HISTORY OF MAN

DR. HRDLIČKA'S communication in SCIENCE for December 27, 1940, containing a passage from a letter written by Linnæus in 1747 on the similarity of man and the apes reminded me of a note I made a few years ago in reading Linnæus' "Tour in Lapland." In his journal of this tour, under date of July 11, 1732, Linnæus wrote, "If we contemplate the characters of our teeth, hands, fingers, and toes, it is impossible not to perceive how very nearly we are related to Baboons and Monkeys, the wild men of the woods." Thus we find Linnæus at the age of twenty-five and nearly fifteen years before his letter of February 14, 1747, to Gmelin, perceiving a close relationship between man and the monkeys. My quotation is from page 331 of the first volume of *"Lachesis Lapponica,* or A Tour in Lapland, Now First Published from the Original Manuscript Journal of the Celebrated Linnæus; by James Edward Smith, M.D., F.R.S., President of the Linnæan Society" (London, 1811). The editor's preface says that the original was written in Swedish intermixed with Latin, that a literal translation of the Swedish was made by "Mr. Charles Troilius, a young gentleman in the mercantile line, resident in London," and that he himself put the matter into its final English form. Though we have no assurance of the absolute literalness of the translation of the particular sentence I have quoted, there seems to be no reason to doubt that it expresses fairly what Linnæus wrote in his journal.

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THE SOYBEAN IN CHINA

IN your issue of January 24, 1941 (Vol. 93, No. 2404), in a report on "the soybean crop in the United States" by A. W. von Struve, I find the assertion: "The first record of the plant is in the writings of Emperor Shang Nung of China in 2838 B.C."

Elmer D. Merrill and Egbert H. Walker ("Bibliography of Eastern Asiatic Botany," 1938, p. 556) call Shên-nung (not Shang Nung) "an early mythical Chinese ruler," H. A. Giles ("Chinese Biographical Dictionary," 1898, No. 1,695) "a legendary emperor," and B. Laufer, more correctly ("Beginnings of Porcelain," 1917, p. 160) "the culture hero, who, as implied by his name, 'Divine Husbandman,' was regarded as the father of agriculture and discoverer of the healing properties of plants."

No one knows just when the herbal attributed to him was compiled, but it probably was not before the Christian era. Philip K. Reynolds and Mrs. C. Y. Fang (*Harvard Journal of Asiatic Studies,* V, 2, June,

1940, p. 171) say: "most likely in the first or second century."

As to the soybean, no indication of it has been found, I believe, in neolithic or early bronze age sites. But it seems to appear in literature of the first millennium B.C. (*cf.* Creel, "Birth of China," p. 326).

References to early dates in Chinese history are likely to be unreliable for any century prior to the eighth before our era.

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OUR SCIENCE MEETINGS AGAIN

IT is getting to be an annual custom to ask, "What *is* the matter with our scientific meetings?" Dr. Merrill asked it quite some years ago on his return to America after a long sojourn abroad. As I remember, he looked forward with enthusiasm to the meetings of the A. A. A. S., his first in many years. And when they convened he wandered from room to room hoping, ever hoping, that in the next room he would find a speaker who could hold his attention. But his wanderings availed him nothing. He made the suggestion that our A. A. A. S. gatherings should not be given over to youngsters who are making their first public appearance. This is true, but it would be just as disastrous if they were given over to the older men. What is needed is a better balance between the stability of older men and the daring enthusiasm of youth. I attempted this in arranging a symposium at which five out of nine speakers were under fifty and two under thirty years of age.

IN a recent issue of SCIENCE (93: 19, 1941), Dr. Francis H. Allen suggests that speakers should improve their delivery. This is important and would help, but it is, so it seems to me, a rather futile attempt at improvement because poor delivery is mostly due to personal traits which are very difficult to correct. To be sure, as Dr. Allen says, we can all learn to hold our heads up and direct our voices toward the back of the room, but to correct a weak voice, which mumbles along in a monotone can be done only by starting when the otherwise able scientist is six years old. If I may suggest several rules for "the acquirement of a clear and common-sense manner of communicating information and ideas to an intelligent audience," then they are the following.

First, foremost and irrevocable should be the rule forbidding the reading of a paper. This will go hard with some, but if a scientist does not know his subject well enough to present it extemporaneously then he should wait until he does. There is usually opportunity for practice on the students and at the home seminar, before the big meetings come. If this rule seems harsh, then attend the next symposium at which some six or eight papers are read, and you will approve the rule wholeheartedly. All other rules are subordinate to this one.

Second in importance is the elimination of historical introductions and lengthy summaries. The latter is as necessary and as effective as a twice-told joke. All mathematical tables and curves should be reduced to a minimum. A column of numbers may help much toward understanding what took place in an experiment, but five tables of five columns each shown one after another leave the audience utterly bewildered. Curves portray a situation with graphic clarity and are always desirable when there is time to study them. Three or four are about all that a speaker can interpret and an audience digest in a half-hour talk, yet a dozen curves at the rate of two a minute is not uncommon at our meetings. These are rules which all can follow and all will be grateful for when they are observed by others.

The greatest lack in our science meetings is discussion. An abstract is printed, the audience has read it, and the speaker repeats it, twice if he gives a summary. It is the awful routine of the papers which is so deadly. One speaker after another, and no comments. If each group could be organized in advance and discussion arranged for, this more than any other change, would make for an interesting meeting.

And all after-dinner talks should be limited to twelve minutes!

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THE PRESENTATION OF SCIENTIFIC PAPERS

THOSE who read F. H. Allen's suggestions about improving the presentation of scientific papers (SCIENCE, 93: 19, 1941) undoubtedly will be interested to learn that the Western Section of the American Society of Plant Physiologists has already taken steps to make scientific meetings more enjoyable by improving the presentation of papers. This question was brought up at the annual meeting during the summer of 1939. The following spring when the call for papers (for the Seattle meeting) was issued, I included the suggestion that prospective speakers *refrain from reading prepared manuscripts, but use a few notes instead.* This suggestion was followed up almost without exception and resulted in better contact between audience and speaker. Because the meetings were followed by an excursion (to Friday Harbor) which lasted a whole day, I had an opportunity to talk to people and inquire about their reaction to the meetings. Many stated that they did not experience the fatigue which is so common after meetings. Although this might have been partly due to the invigorating atmosphere of the Pacific Northwest, I am convinced that the informal