elements are more easily disturbed and broken apart than is the case among more complex organisms. The study of fossils shows the extraordinary permanency of structures in the higher animals and plants. It may well be that the cytoplasm, in addition to its other functions, serves to protect the chromosomes from such disintegrating influences as are described by bacteriologists. The phenomena of crossing over show how readily a part of a chromosome becomes attached to another, and by analogy it is easy to understand why bacteria so readily "take on" free genes. Dr. Manwaring says: "Transmissible bacterial genes are apparently widely distributed in nature, being found, for example, in almost any contaminated surface water." The "polyvalent" genes may really be aggregates of two or more. It is conceivable that we may return to a sort of Darwinian pangenesis, and postulate the existence of many kinds of "free genes," which are ready to unite with the organized systems of genes when they have a chance. Some bold experimenter, perhaps using sperm cells on account of the absence of the thick cytoplasmic covering, may one of these days succeed in adding genes to the germinal elements of the higher organisms.

What we now want to know is whether the "dissociated genes" arise *de novo* from inorganic or non-living sources or whether they are always the result of the breaking up of living systems. Just as the inorganic letters of the alphabet c, a, t, when united give us the organic cat, so it is conceivable that the genes owe their significance as vital units to their being parts of a system, and not to any special "vital" properties of their own.[2]

In any case, we have plenty of evidence to show the extraordinary stability of genes in nature, their persistence during many millions of years, under all sorts of diverse conditions. This stability may in a sense be a product of natural selection, since it is essential for the processes of evolution and adaptation. Nature can not build on a quicksand. It does not seem probable that the phenomena described by Dr. Manwaring can be ascribed to perpetual or very frequent gene mutations, or to specific changes in genes induced by particular environmental factors. According to this view, bacterial genes may be about as stable as others, and there is no "Lamarckian world of bacteriology."

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

DARWIN'S VIEW OF HEREDITY

IT seems that in the interest of modernity we ought to demonstrate the fallacies of our predecessors. One of the favorite methods adopted for this end is to schematize the theories of earlier workers and then show how modern advances have shown these schemes to be untenable. It seems to me that the time has come, however, when text-book writers ought to check over more thoroughly the written works of the author whose theory is being criticized. The particular instance of this which is rapidly becoming my private grouch is the apparent wide-spread belief that Darwin believed all variations to be inheritable, and thus grist for the natural selection mill. In a fairly recent textbook, for instance, there occurs the statement, "Darwin believed that all differences among individuals were hereditary."

I would like to call attention to some quotations from Volume 2 of Darwin's "Variation of Animals and Plants under Domestication." In the first paragraph of chapter 12: "It is obvious that a variation which is not inherited throws no light on the derivation of species, nor is of service to man, except in the case of perennial plants, which can be propagated by buds." Again, about two thirds of the way through the same chapter: "When a new peculiarity first appears, we can never predict whether it will be inherited."

It is true that he also stated, in Chapter 28, that "we are led to conclude that species have generally originated by the natural selection, not of abrupt modifications, but of extremely slight differences." This has frequently been stressed as a difference between his and more recent theories which stress mutations. Since, however, mutations no longer signify large abrupt changes alone, but simply heritable variations, however slight, and since we have found that the larger share of these are very slight alterations, it seems something of a quibble to say that, since Darwin did not believe that "sports" were especially significant in evolution—a view which modern geneticists would subscribe to if "sports" mean such large modifications as they did in Darwin's day, *e.g.*, moss roses, hornless cattle, etc.—his view differs so radically from such a view as is, for instance, incorporated in Morgan's "Scientific Basis of Evolution."

With the hope that this protest will lead an occasional biologist either to glance through for the first time or to review once more one of Darwin's most significant contributions to scientific literature, I submit it.

GEORGE M. ROBERTSON

DARTMOUTH COLLEGE

TERRACES IN THE SUSQUEHANNA VALLEY BELOW HARRISBURG, PENNSYLVANIA

DURING the summer of 1931 the writer made a study of river terraces in the Susquehanna Valley

[2] For a discussion of the gene as the unit of life, see Hurst, "The Mechanism of Creative Evolution," Chap. XVII, 1932.

south of Harrisburg, Pennsylvania. The investigation, which was sponsored by the American Geographical Society, the Carnegie Institution of Washington and Columbia University, was under the direction of Professor Douglas Johnson and was carried on in cooperation with the Pennsylvania Geological Survey. Publication of the formal report has been delayed, but certain of the conclusions have been briefly set forth in a recent bulletin[1] of the Pennsylvania Survey, prepared by the director, Dr. George H. Ashley. The writer's responsibility to the sponsors of the investigation for these conclusions prompts publication of this statement.

In 1928 Leverett published a brief note[2] in which he reported Illinoian pebbles approximately 100 to 120 feet above the Susquehanna at Harrisburg. These pebbles occur on a stream-cut, gravel-mantled rock terrace that is well developed throughout the Great Valley-Triassic Lowland section of the Susquehanna Valley. The terrace stands at the same or nearly the same elevation as broad lowlands of the well-known Somerville peneplane (400 to 420 feet above sea level, or 90 to 110 feet above the river near Harrisburg). This relationship was interpreted by the present writer to mean that the terrace was formed by the lateral swinging of the Susquehanna River during the same pause in downcutting that permitted reduction of weak limestone belts to the Somerville peneplane by weathering and solution. The presence of rock types foreign to the drainage basin of the Susquehanna in the Somerville terrace gravels (laid down *pari passu* with the cutting of the rock benches) indicated that the Somerville cycle was interrupted during or after the advance of some ice-sheet into the drainage basin. On this evidence the writer fixed the age of the Somerville peneplane as Pleistocene. If Leverett's correlation is correct, the age of the Somerville is more definitely fixed as Illinoian or post-Illinoian Pleistocene.

Topographic maps of the terrace surfaces at and below Harrisburg were prepared by the writer with the able assistance of Dr. William O. Hickok and Mr. Forrest T. Moyer, both of the Pennsylvania Survey. A part of one of these maps, printed in the bulletin, is, through oversight, specifically credited in the legend and the accompanying text to Hickok and Moyer alone.

The original purpose of the terrace study was the investigation of the "knickpoint" concept, applied in the interpretation of the erosional history of the Susquehanna Valley by E. B. Knopf[3] in 1924. Hickok has extended the application of this concept in a recent publication.[4] Ashley, in the bulletin mentioned above, leaves the issue open. The writer takes this opportunity to state his conclusions in regard to the "knickpoint" theory, in advance of the publication of the complete report. These are, briefly, that as originally found by Stose,[5] the terraces slope down-valley; that the profile breaks considered by Knopf and Hickok to be cyclic ("knickpoints") are due rather to resistant rocks in the river bed; and that only two post-Harrisburg cycles (the Somerville and the present cycle) are recognizable in the Susquehanna Valley, as opposed to four found by Knopf and eight by Hickok.

J. HOOVER MACKIN

DEPARTMENT OF GEOLOGY
COLUMBIA UNIVERSITY

[1] George H. Ashley, "The Scenery of Pennsylvania," Pennsylvania Topographic and Geologic Survey: *Bulletin G 6*, 1933.

[2] Frank Leverett, "Results of Glacial Investigations in Pennsylvania and New Jersey in 1926 and 1927" (abstract), *Bull. Geol. Soc. Am.*, 39: 151, 1928.

[3] E. B. Knopf, "Correlation of Residual Erosion Surfaces in the Eastern Appalachian Highlands," *Bull. Geol. Soc. Am.*, 35: 633–668, 1924.

[4] William O. Hickok, "Erosion Surfaces in South-Central Pennsylvania," *Am. Jour. Sci.*, 5th ser., 25: 101–122, 1933.

[5] G. W. Stose, "High Gravels of the Susquehanna River above Columbia, Pennsylvania," *Bull. Geol. Soc. Am.*, 39: 1073–1086, 1928.

A NEW HORIZON FOR THE EXTINCT GOOSE, CHENDYTES

THROUGH the kindness of Dr. Chester Stock I have been permitted from time to time to examine fossil bird remains in the collections of the California Institute of Technology. Based on a single specimen from Ventura County, this note is offered as a record of the following points:

(1) A new bird-bearing locality—the twenty-third for the state of California.
(2) The third station for *Chendytes lawi*, an extinct diving goose.
(3) An extension downward of the known range of this species from Upper San Pedro to Lower San Pedro.

The specimen referred to is a single complete left tarso-metatarsus No. $\frac{211}{1670}$, California Institute of Technology from south flank of the Ventura anticline, west bank of Sexton Cañon, 800 feet south of intersection of Lake Cañon, 350 feet above the base of the San Pedro formation. Except for a slight degree of slenderness, the specimen is identical in character with one of the same segments from Arnold's Lumber Yard Station at San Pedro (Upper San Pedro Formation). The difference in stoutness is not greater than is evident within the species limits of any of our modern geese.

The type locality of *Chendytes* is at Santa Monica in shell beds 150 feet above the sea and about a mile