of sea-level from 1919 to 1926 referred to above may afford some support for Marmer's twenty-year period, a support not necessarily negatived by the higher level of 1927. Time will give the answer to this phase of the problem, as to others.

Should continued tidal observations at Fort Hamilton demonstrate the reality of a very slow rise of sea-level, thus far masked by the combination of irregular and periodic variations of the ocean surface, we would still be far from the demonstration of a general progressive rise of sea-level or subsidence of the land. Such rise might be but the upward swing of a periodic fluctuation extending over a span of time longer than any yet determined. Accurate tidal records do not go far enough back to enable us to detect a thirty-five-year or longer period. Again, the rise might be both local and temporary, due to changes in the form of shores and channels, as set forth on page 39 of the bulletin previously cited. Comparison with other tidal stations would in time reveal the nature of the rise.

Meanwhile we must recognize the following pertinent facts. (*a*) Although mean sea-level is the best known datum from which to reckon slow progressive changes in the relative levels of land and sea, it is itself an uneven surface. Furthermore, it is an extremely sensitive surface, subject to both irregular and periodic changes in altitude some of which extend over decades. (*b*) Hence the determination of slow progressive mean sea-level changes, far from being the simple operation it was once considered, is a peculiarly delicate and difficult task. (*c*) While precise tidal observations are now available for a period sufficiently long to show the absence, during such period, of any pronounced rate of subsidence, like the one or two feet per century commonly attributed to the Atlantic Coast, the occurrence of a much slower change (what the writer has called an "inappreciable" change) can be neither affirmed nor denied until many more years of precise tidal observations are at our disposal. The writer does not deny the possibility of a slight or slow change, and hopes some one may take sufficient interest in the subject to finance the maintenance of a tidal station in some position suitable for the required critical observations.

With the suggestion of Lane and Cheney that the whole question deserves further consideration we are in hearty accord. Aside from its scientific interest the problem of slow sea-level changes enters into practical affairs where the engineer must foresee increased wave activity on a subsiding coast, where title to submerged property depends on whether the submergence was due to natural or artificial causes and in other circumstances which need not be considered here. Hence every contribution to this difficult problem is doubly welcome. But it is not clear that the further suggestion relating to meanders in streams flowing at and below tide-level is pertinent to the question at issue. Even if such meanders indicate past submergence (which remains to be demonstrated), it is difficult to see how they could throw any light on slow changes in sea-level supposed to be taking place at the present time.

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ON GENUS AND SPECIES MAKING

PROFESSOR NEEDHAM'S objections to certain lengthy generic names[1] appears to have brought forth a flood of comment, mostly reactionary, not only concerning the length of generic names but also relative to the alleged overmultiplication of generic names.[2] More recently Dr. Hubbs[3] has hastened to the defense of the systematic zoologist. As a systematic zoologist working with the invertebrates I venture to offer a few comments, not only in the matter of generic names, but also in the treatment of specific variation. For the past hundred or more years the work of defining new genera has been in progress. At first these groups were founded upon characters contained only in the external parts of the animals, principally the shells of mollusks and other invertebrates. As the internal organs began to receive attention, new characters or combinations of characters were discovered which resulted in further splitting of older names, and in advancing subgenera to generic rank. Any one who has followed the development of the classification of the land Pulmonata under the epoch-making studies of Dr. Pilsbry, in which many new genera and higher groups have been diagnosed, can not but admit that the subject has been made clearer by the addition of the many generic groups.

This division into genera is a refinement of classification made necessary by our advance in knowledge of the structure of animal life. After all, classification is only for the interpretation of natural laws, including the separation of the various types of animal life into groups for purposes of use in different lines of investigation, and for this purpose nothing has been suggested that is in any manner an improvement over the modified Linnaean system now in use. Degrees of differentiation are well indicated by classes, orders, families and genera. Suggestions have been made from time to time that numbers or symbols would be an improvement, but every systematist knows full well that such systems would be totally out of the question for practical use.

[1] SCIENCE, 71: 26–28, 1930.
[2] SCIENCE, 71: 215–218, 1930.
[3] SCIENCE, 71: 317–319, 1930.

A fact of great significance in this discussion appears to have been overlooked, that this seemingly great overmultiplication of generic names may be of a transient nature, for in many groups of animals our knowledge is still so meager that no one can predict what the final groupings may be when the structure of all species is known. Dr. H. B. Baker, in his study of the minute land snails of America, has pointed out this aspect of the case, and states that until all members of the larger groups have been studied, the different divisions must be treated as genera, which may be reduced to subgenera in the light of later study. This is equally true of the fresh-water pulmonates, in which group anatomical characteristics of great significance are being found, and the newer groupings must be kept separate as genera until more of the species have been investigated. To include many of these as subgenera would be a contravention of the facts. I can not see how we are to avoid this multiplication of genera if we are to make substantial progress in our interpretation of invertebrate life. No difficulty is encountered in using the newer classifications in teaching students who have not already absorbed the older view-points. The greatest difficulty appears to be among the workers who have for years used the older forms of classification and who find it difficult to adjust themselves to the newer view-point.

The multiplication of names is not alone confined to genera. Old species, found by careful study to be composites, are being split into two or more species. The study of environmental influences is adding to the number of subspecific names. The latter feature, carried to its logical conclusion in a recent work on fresh-water mollusks,[4] has called forth the same sort of criticism as that for the division of genera.[5]

What constitutes a species is governed so largely by personal opinion that no two authorities are likely to agree on the subject. The writer has made it a rule to consider a group of individuals which are separated from all other groups by some definite combination of characteristics, without intergrading, as species; those that show intergradations as varieties. Geographic races or varieties are for the most part generally accepted without question and need not be discussed. But when an attempt is made to interpret the action of the environment on species, and names are given to these ecological forms, objection is at once made on the ground that they are trivial and should not receive distinct names, but be designated by locality. Such a procedure is obviously impossible for the reason that two or more of these variations may occur in the same region and the reader of a communication would not know which form was referred to.

For the study of animals from the ecological and distributional standpoint these variations resulting from environmental changes must receive names to be used. In many species of fresh-water mollusks, one form may be quite uniform in shape when inhabiting the river systems of the southern and central parts of the United States, and yet show quite distinct changes in form when inhabiting the lake regions of Wisconsin, Michigan and other northern states. Even closely adjoining lakes may contain different varieties or a single lake may contain two of these varieties. Many of these have been caused by isolation in the same manner that land forms have evolved when isolated on island habitats in the ocean. For the purpose of adequate interpretation of these ecological conditions the varieties must receive names even though not all the variations are of equal value. A quadrinomial system might help in some instances, but it is not necessary. Ortmann some years ago propounded a law of river development for the naiades, based on the variation in form of the shell from the headwaters of a river to its lower, larger portion. This law was applied by the writer to the river fauna of Wisconsin and Illinois and found to hold good for nearly all species of naiades. Now to study intelligently these variations coincident with size of a stream it is necessary to have definite trinomial designations for the chief variations which may be characteristic of certain portions of the river, even though these variations are connected by intermediate forms (if they were not so connected they would constitute species).

It appears difficult for many students of nature to realize that evolution is still in progress. Species, while normally reasonably fixed for a limited period, may undergo sudden and marked change when subjected to changes of environment, even when such a simple thing as a log dam is placed across a river, converting it into a pond or lake. The log dam is quite as effective as a glacial dam across a stream which forms a glacial lake, and we have abundant evidence that changes following the damming of streams in glacial times have been followed by the most diverse changes in species, especially in the glaciated areas of Wisconsin and Michigan. There is no such thing in an organic species as the stability that we find in a "title deed," nor can a species remain as fixed as "an authoritative pronouncement from the bench" (which, by the way, may be overruled by a higher court). All animal life is subject to change as soon as the normal conditions of the environment change, and there is evidence that this change may not always be a matter of long years but may take

[4] Baker, "The Fresh-water Mollusca of Wisconsin," Bull. 70, Wis. Geol. Nat. Hist. Survey, 1928.

[5] Goodrich, *Nautilus*, 42: 114–118, 1929.

place in the space of five or ten years. This statement is abundantly supported by experimental evidence. The necessity for giving names to these incipient species is, therefore, obvious.

Taxonomy is but a tool which is used for the interpretation of life processes, and yet its value is beyond question and can not be denied by any one. It is the foundation upon which all the biological sciences rest. The alleged overmultiplication of generic and varietal names is but an attempt to inquire more closely into the true relationships of organisms. More attention should be given to this subject in university curricula in order that the student may have a just appreciation of its importance and an understanding and sympathetic attitude toward the systematist who is endeavoring to make a just and true interpretation of the relation of life to the laws through which it has come into existence. This can not be accomplished by the kind of criticism which has been in vogue, but by an increase of workers who will help to untangle much of the chaos that now surrounds the classification of many groups of animals.

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EGYPTIAN MATHEMATICS

IT may be well to make generally available information supplementing, or correcting, some matters referred to in my address on "Mathematics before the Greeks" recently published in SCIENCE,[1] since the matters in question are of somewhat general interest.

(1) Following other writers I referred to the hieroglyph for 1,000 as "a lotus flower" when further investigation would have shown that I should have said "a stem with a lotus leaf."

(2) In giving references to discussions as to "whether the Egyptian had a conception of the general fraction" I might very appropriately have given a reference to a recent discussion by Kurt Vogel who concludes[2]: "Es wird also jetzt von allen Seiten anerkannt, dass der Ägypter den klaren Begriff des allgemeinen Bruches (in dem nicht-komplexen Sinn) gehabt hat."

(3) Quoting Breasted's "Ancient Times," 1916, I noted that "the earliest dated event in history was the establishment in 4241 B. C. of the Egyptian calendar of twelve months of thirty days plus five feast days." Breasted's statement is doubtless based on Eduard Meyer's "Ägegyptische Chronologie."[3] In 1917 Borchardt showed[4] that the date 4241 should be 4236 (with a possible error of two years), and up to 1925 this was the accepted date.[5] Mr. S. R. K. Glanville, of the British Museum staff, has, however, kindly pointed out to me that in a recent study of this question, Alexander Scharff[6] makes clear the possibility that 2776 instead of 4236 might in fact be the year when the Egyptian calendar was inaugurated.

(4) One of my statements concerning the Cheops pyramid needs to be revised as follows: "It is said that 100,000 workmen were kept constantly employed on this structure for thirty [not *fifty*] years, ten years of this period being used in constructing a road to the Nile, 1,017 yards [not *limestone quarry some miles*] distant." In the history of Herodotus, written in the fifth century B. C., there is a passage which informs us in this connection as follows[7]:

> Till the time of Rhampsinitus Egypt (so the priests told me) was in all ways well governed and greatly prospered, but Cheops, who was the next king, brought the people to utter misery. For first he shut up all the temples, so that none could sacrifice there; and next, he compelled all the Egyptians to work for him, appointing to some to drag stones from the quarries in the Arabian mountains to the Nile: and the stones being carried across the river in boats, others were charged to receive and drag them to the mountains called Libyan. They worked in gangs of a hundred thousand men, each gang for three months. For ten years the peoples were afflicted in making the road whereon the stones were dragged, the making of which road was to my thinking a task but a little lighter than the building of the [great] pyramid, for the road is five furlongs long and ten fathoms broad, and raised at its highest to a height of eight fathoms, and it is all of stone polished and carven with figures. The ten years aforesaid went to the making of this road and of the underground chambers on the hill whereon the pyramids stand; these the king meant to be burial-places for himself, and encompassed them with water, bringing in a channel from the Nile. The pyramid itself was twenty years in the making. Its base is square, each side eight hundred feet long, and its height is the same; the whole is of stone polished and most exactly fitted; there is no block of less than thirty feet in length.

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[1] SCIENCE, 71: 109, January 31, 1930.

[2] K. Vogel, "Die Grundlagen der ägyptischen Arithmetik in ihrem Zusammenhang mit der 2: n-Tabelle des Papyrus Rhind" (dissertation), Munich, 1929, p. 185.

[3] *Phil. u. hist. Abhandlungen d. k. preuss. Akad. d. Wiss.*, 1904, no. 1, pp. 38–44; French translation by A. Moret, Paris, 1912, pp. 48–55.

[4] L. Borchardt, "Die Annalen und die zeitliche Festlegung des alten Reiches der ägyptischen Geschichte," Berlin, 1917, p. 58.

[5] E. Meyer, "Die ältere Chronologie Babyloniens, Assyriens und Ägyptens," Stuttgart, 1925, p. 45.

[6] A. Scharff, "Grundzüge der ägyptischen Vorgeschichte," Leipzig, 1927, pp. 54–57.

[7] Herodotus with an English translation by A. D. Godley (Loeb Classical Library), London, vol. 2, 1921, pp. 424–427.