sembling the real one; this, apart from errors or transposition of labels, to which accidents all collections are more or less liable, in proportion to their age.

While, therefore, fully admitting the great value of a type, or type-figure, it is necessary to ascertain that it is really the specimen or represents the specimen originally described. If it contradicts the original description in any important respect, and especially if it is an insect known to be from a different locality to that assigned to it by the original describer, it is more than probable that it is not the original type at all, and is worse than misleading. Errors of locality are always possible; but much will depend on the author. Donovan, for instance, was extremely careless about localities, but, as he figured all his species, this matters less; on the other hand, Fabricius was far more careful than later authorities have given him credit for; and an error of this kind in his work was quite exceptional.

THE CONVEX PROFILE OF BAD-LAND DIVIDES.

BY W. M. DAVIS, HARVARD COLLEGE, CAMBRIDGE, MASS.

IN Mr. Gilbert's analysis of land sculpture, constituting chapter V. of his "Geology of the Henry Mountains," he explains why the surface of an eroded region possesses slopes that are concave upwards and steepest near the divides, and shows that it is for the reasons there stated that mountains — that is, mature and well-sculptured mountains, such as are of ordinary occurrence — are steepest at their crests (p. 116). The *arêtes* of the Alps illustrate this perfectly. Gilbert calls this generalization the "law of divides."

But in discussing the forms assumed by eroded bad-lands, or arid regions of weak structure with insignificant variety of texture, he finds an exception to the law of divides. The two lateral concave slopes of a bad-land ridge do not unite upwards at an angle, forming a sharp divide, but are joined in a curve that is convex instead of concave upwards. "Thus in the sculpture of the bad lands there is revealed an exception to the law of divides,— an exception which cannot be referred to accidents of structure, and which is as persistent in its recurrence as are the features which conform to the law,— an exception which in some unexplained way is part of the law. Our analysis of the agencies and conditions of erosion, on the one hand, has led to the conclusion that (where structure does not prevent) the declivities of a continuous drainage-slope increase as the quantities of water flowing over them decrease; and that they are great in proportion as they are near divides. Our observation, on the other hand, shows that the declivities increase as the quantities of water diminish, up to a certain point where the quantity is very small, and then decr ase: and that declivities are great in proportion as they are near divides, unless they are *very* near divides. Evidently some factor has been overlooked in the analysis,— a factor which in the main is less important than the flow of water, but which asserts its existence at those points where the flow of water is exceedingly small, and is there supreme" (pp. 122, 123).

It has for some time seemed to me that the overlooked factor is the creeping of the surface soil; and, as I have not seen mention of this process as bearing on the form of the crest-lines of divides, a brief note on the subject is here offered.

The superficial parts of rock-masses are slowly reduced to rock-waste or soil by the various processes included under the term, *weathering*. Unconsolidated materials are in the same way reduced to finer texture near their surface. The loose and often fine material thus provided at the surface is carried away by various processes, of which the chief are moving water, moving air, and occasionally moving ice; but there is an additional process of importance, involving dilatation and contraction of the soil, and in consequence of which not only the loose particles on the surface are transported, but a considerable thickness of loose material is caused to creep slowly down-hill.

Dilatation is caused by increase of temperature, by increase of moisture, and by freezing. Vegetable growth may probably be added to this list. The movements are minute and slow. They are directed outwards, about normally to the surface. Contraction follows dilatation, when the soil cools or dries, or when its frost melts. The movement of the parts is then not inward at a normal to the surface, but vertically downwards, or even downwards along the slope. As the two motions do not counterbalance each other, a slow down-hill resultant remains. This is greatest near the surface, where the dilatations and contractions are greatest; but it does not cease even at a depth of several feet, perhaps of many feet. Hence the down-hill dragging of old-weathered rock often well shown in fresh railroad cuttings in non-glaciated regions. I presume all this is familiar to most readers; although from the frequent inquiry concerning the means by which valleys are widened it is evident that the creeping process is not so generally borne in mind as that by which running water washes loose material down-hill.

The form assumed by the surface of the land depends largely on the ratio between the processes of washing and creeping. Wherever the concentration of drainage makes transportation by streams effective, the loose material is so generally carried away (except on flood-plains) that the action of creeping is relatively insignificant. But on divides, where drainage is not concentrated but dispersed, the ratio of creeping to washing is large, even though the value of creeping is still small. This is especially the case in regions of loose texture and of moderate rainfall; that is, in typical bad-lands, where the supply of loose surface-material ready to creep is large, and where the loose material is slowly taken away by washing. On the divides of such regions, the surface form is controlled by the creeping process. The sharp-edged divides, that should certainly appear if washing alone were in action, are nicely rounded off by the dilatations and contractions of the soil along the ridge-line. The result thus determined by the slow outward and downward movements of the particles might be imitated in a short time by a succession of light earthquake shocks.

Mr. Gilbert has himself given several beautiful illustrations of the close dependence of sharp or rounded divides on rainfall; structure remaining constant. If the rainfall should increase in bad-land regions, would not all their divides become sharper; and if the rainfall were continuous, so as to carry away every loose particle as soon as it is loosened, would not the divides assume the sharp ridge-line expected from Mr. Gilbert's analysis but not found in the actual arid bad-land climate? In the eastern and well-watered part of our country, I have often seen clay-banks much more sharply cut than the equally barren surface of the western bad lands; but even on clay-banks, the minute divides between the innumerable little valleys are not knife-edge sharp; they are rounded when closely looked at. Perhaps they are sharper in wet weather and duller in dry spells.

If rainfall remain constant and structure vary, then the harder the structure, the less the supply of soil for creeping and the sharper the divides; the weaker the structure, the more plentiful the supply of soil for creeping and the duller the divides. Numerous examples of this variation might be given.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Some Remarks on the Botanic Trinomial.

AN article in *Science* for September 16, signed C. H. Tyler Townsend, contains certain statements which cannot be passed, it seems to me, without some few words of discussion. It is quite evident that this article loses sight entirely of the main purpose of a biological name, and seems to imply that the name of a thing has to do with justice, right, etc. For example, I find therein the following expressions: "In no case can the name of the original erector and describer of a genus be separated therefrom without gross injustice." "There is no necessity whatever for shedding glory upon the one who has made the transfer. . . . He has no right whatever to the species." These words, "injustice," "right," belong to the field of Ethics, not that of Taxonomy.

I shall try to consider the botanic trinomial, not from the ethical point of view as Mr. Townsend seems to have done, but from the taxonomic strictly.

We find it convenient to give a name to a plant simply because the use of the name serves to call up an aggregate of characteristics when we wish, without the necessity of detailing those characteristics. The whole matter is one of convenience simply, and a name means nothing more than this.

It has been pretty universally agreed that it is more convenient to have a binomial name than a monomial one, for by this means we are enabled easily to group our plants, the first name serving to call to mind the aggregate of characteristics of the group (genus), possessed often by many sub-groups (species), and the second those characteristics possessed to a greater or less extent by the individuals that go to make up the sub-group.

So far this seems to be reasonable enough, and, following the same lines, should we choose to add a third name to our binomial, making it a trinomial, we should naturally do so for the purpose of segregating these sub-groups into still smaller ones (varieties). On this line the addition of terms might rationally be continued to the extent that the facts of observation would warrant.

But we find in the *de facto* botanic trinomial a mixture of two taxonomic principles, instead of the rational following out of the single line indicated by adding to the monomial the second term. Usually the third term is added as a compromise with existing fact, simply to avoid the possibility of having two homonomic binomials, and consists of the name of the person who first published the binomial. It is evident that this addition of such a third term serves a purpose only in comparatively rare cases; in the vast majority, were it not for the fear that some future comer would see fit to use the same binomial to designate another plant, it would be, as a name, useless. But at present the addition of the author's name is essentially a part of the naming of the plant.

It is this third name, and comparatively useless one, that is the cause of much of the trouble of the botanic taxonomists. Many seem to feel that this serving as a compromising tailpiece, the necessity for which it is confessedly the aim of the botanic world to do away with altogether, is an honor. And for this reason there is strife in a large class of cases as to the third name to be added to the binomial. For consider the following specific case. Hooker and Arnott notice a plant, which, in their judgment, is a member of the large group of plants that has been called Malva. They therefore give it the binomial name *Malva malachroides*, and first publish the characteristics which that name is to call up. Afterward Gray considers that the plant cannot belong to the group called Malva, and so gives the same plant the name *Sidalcea malachroides*. More recently Greene finds that the plant can be neither a Malva nor a Sidalcea, and calls it *Hesperalcea malachroides*.

Now suppose we have an individual of this group and wish to give it the most convenient name. For the name of the main group undeniably it matters not which of the three names we choose; if we have had the opportunity of studying the plant carefully our choice will be determined by the observed facts and our own judgment. Personally, in the present case, I chose to call the plant Hesperalcea. For the second name there is no choice, the three authors having given it the same. (Had there been a diversity of names here, the name first given the plant would have been chosen, not because this is "just," or "right," but because by this artificial rule we obtain a permanent factor in the name, without fossilizing individual opinion at all regarding the affinities of the plant.)

We now come to the third name, only added, remember, from the fear that some one has called or will call some different plant Hesperalcea malachroides. Here custom is divided, and many would write *H. malachroides, H. and A.*, and others *H. malachroides, Greene.* It is for us now to determine which of these names is the most convenient.[1] The person to whom we wish to communicate the idea, H. malachroides, upon seeing the trinomial H. malachroides, H. and A., naturally turns to the works of H. and A. to find the summing up of the characters of the plant. But here he is met with an insurmountable difficulty. He can find no trace of it. Let him look for *malachroides*, perchance Mr. Townsend would say. But it is easily possible that H. and A. have described five species by the name of *malachroides*. On the other hand, suppose we write H. malachroides, Greene, the person wishing to know of this plant would turn to the works of Greene and there would find the reference to Malva malachroides, H. and A., which would enable him to find the original description of the plant and thus obtain the idea which we wished to convey.

It seems plain enough then that the third name of this trinomial from the standpoint of convenience should be Greene and not H. and A..

Mr. Townsend disposes of this difficulty in the following words:—

"I would write Metsgeria pubescens schrank, . . . and make no more ado or trouble about it. . . . This signifies always that the authority named described the species originally and originally proposed that name. The founder and date of the genus can be ascertained by referring to any monograph."

It is obvious on a little thought that this paragraph assumes a good deal more than the facts warrant. In the first place there certainly will be no monograph of the species named *pubescens*; and it is very possible that a monograph of the generic name chosen may not exist.

But it is perhaps allowable to look at these two trinomials from a slightly different point of view. Which tells the most truth? H. malachroides, H. and A., implies that H. and A. would now choose, as we have done, the group Hesperalcea for this plant. This we have no right to imply; as a matter of fact they did choose Malva, and this is all we know or should state.

Of course, in all the preceding I have assumed that the purpose of a name is to convey from one person to another the idea of a thing, and on this hypothesis it seems to me that the conclusions arrived at are sound; but I would not wish to be understood as desiring that a name should do no more than this. If it can convey the history of the thing, well and good, as long as by trying to do this it does not entirely defeat its own purpose, as I think I have shown Hesperalcea malachroides, H. and A., would do.

C. MICHENER.

San Francisco, Oct. 7.

[1] I have not considered the writing of H. malachroides (H. and A.) Greene, as the parenthetical term is no more an essential part of the name than the date of publication or twenty other particulars which might occur in a monograph on the plant.

Notes on the Saturniidæ, or Emperor and Atlas Moths.

ALTHOUGH the family *Saturniidæ* comprises the largest and some of the handsomest of all the *Lepidoptera*, it is still very imperfectly known. The larvæ are mostly gregarious, and feed on trees. Many of them form cocoons, which are attached to the branches of the trees upon which they live, while others (at least in South Africa) are said to pupate in the ground. I am not certain whether it has yet been ascertained whether this latter habit has been proved to be peculiar to certain species or genera, or whether the same species may form its pupa in different ways, according to circumstances.

There is doubtless a much greater variety of these insects in tropical countries than we are at present aware of. Many of the most remarkable species are only received singly, and often remain unique in our collections for years. Collectors rarely have an opportunity of rearing them from the larvæ, even if they should meet with a brood, and many species probably feed on lofty trees, quite out of reach, while the perfect insects are nocturnal in their habits. Many of the larger, and especially the domesticated species of *Saturniidæ* from which silk is obtained in India, China, and Japan, vary very much, and this is another obstacle to their successful study. Many of these domesticated breeds, and the various wild or semi-domesticated forms allied to them have been simply named, and not described; or perhaps only the food-plants and localities have been indicated. These useless names find their way into our collections and from thence into our lists and papers, and form a wholly unnecessary element