

lish the proposition that all discord is wholly independent of beats. Whether it be due wholly or partly to beats or to something else quite unknown as yet, Mayer's curve is sufficient to disprove the statement that the maximum of discord is due to always the same number of beats, whatever may be the pitch.

It was not my intention to publish any detailed criticism of the point to which I took exception in my review of the book of physics under examination. But Mr. Meyer will probably agree with me in objecting to the following sentences, which may now be quoted: "One cause of discord is the presence of beats between the two notes, and the greatest discord results when the beats are about 32 per second. If the number of beats is fewer than 10 per second, they are not agreeable, but do not produce discord. Discord is caused by sounding together notes that give more than 10 and less than 70 beats per second." Whatever may be the cause of discord now agreed upon among psychologists, Mayer's law comes nearer to being a statement of the truth than the sentences just quoted. The author is creditably cautious in assigning the presence of beats as 'one cause.' Presumably it may not be the only cause. But his quantitative statements warranted my criticism that he had "defined 'discord' more sharply than the facts warrant by failure to recognize Mayer's law."

If Mr. Meyer will criticise the quoted sentences from the standpoint of the psychologist he will doubtless confer a favor upon physicists who have not kept up with recent advances in psychology.

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August 31, 1901.

MAGAZINE ENTOMOLOGY.

It has been the habit to charge newspapers with the dissemination of scientific misinformation, and undoubtedly with considerable justice. But they are not the sole sinners. In the September number of *McClure's Magazine* there is a paper entitled 'Next to the Ground; Stories and Scenes of Farm Life,' by Martha McCulloch Williams. The amount of misinformation it conveys cannot be equalled by any bit of

newspaper writing, and for ignorance on the part of the author it is certainly entitled to the palm. First, we have information about the dragon fly, and the superstition concerning snakes that is connected with it; that is all right; but we are immediately afterward informed that it begins its early life as a fat white grub, variously known as Hellgrammite, dobsen, etc. Now, in the first place, the hellgrammite is neither white, nor fat, nor is it a grub; and in the second place, it has absolutely no connection with the dragon fly.

From any elementary work on entomology, national or foreign, the writer could have obtained an accurate life history of the dragon fly, and also information as to the adult stages of the hellgrammite. She may have seen a dragon fly; she certainly never saw a hellgrammite to know it. Then we learn something about the locusts, and most interesting, we are told that the eggs are laid in the pith of dying twigs. So much has been written about these insects that it does seem as if the authoress might have known better than to make an assertion of this kind. Pithy stems are rarely used by locusts, if at all, and dying twigs are never attacked. The eggs are always laid in growing shoots, and in the wood itself. I wonder where she saw the black beetles or 'Betty bugs' that were three inches long? She speaks of them as 'Scarabs,' and the largest of these, occurring in the United States entitled to that name, are not more than one inch in length.

More marvelous than anything else is the description of the change from the tumble bug, black and loutish, into a 'June bug,' green all over, with copper yellow tints on the legs, etc. Where in the world this information came from, if it was not the product of overstrained imagination, seems incomprehensible. In fact, reading the entire paper, which covers eight pages, there is more dense ignorance and absolute misinformation crowded into it than I have seen anywhere on a similar subject within the last decade. And what there was in it anywhere, to recommend to the editor its publication, seems almost beyond finding out. There has been of late years a great revival of interest in natural history. We have had many useful and accurate books, including the topics on which Martha

McCulloch Williams writes. It would seem as if anyone intending to publish concerning natural history would at least familiarize himself or herself with the subject. It is discouraging to find a magazine like *McClure's* accepting and printing an article of this character.

JOHN B. SMITH.

NEW BRUNSWICK, September 12, 1901.

CURRENT NOTES ON PHYSIOGRAPHY.

THE RANGES OF THE GREAT BASIN.

THE mountain ranges of the Great basin in Utah and Nevada have been explained, chiefly by Gilbert and Russell, as due to block faulting, but without sufficient statement concerning the form of the region before faulting or of the amount of erosion since faulting. Hence the ranges have sometimes been imagined as presenting long, gentle back slopes where the pre-faulting surface has been tilted up, and abrupt frontal cliffs where the fault scarp is revealed; and in the absence of statement to the contrary it has been sometimes supposed that the faults by which the blocks are limited were determined by ordinary stratigraphic evidence.

Spurr now offers a new interpretation of these ranges ('Origin and Structure of the Basin Ranges,' *Bull. Geol. Soc. Amer.*, XII., 1901, 217-270, pl. 20-25). Finding monoclinial structure not persistent, finding much dissection on both slopes of the ranges, and finding no stratigraphic evidence of faulting along the base of the ranges, he discards the theory of block faulting and explains the mountains as residuals of a disordered and greatly denuded mass, the intermont depressions being regarded as valleys of erosion produced under a former greater rainfall and now clogged with waste since a drier climate has set in.

Attention is here called to the different values given by Spurr to the stratigraphic evidence of faults where both members of the faulted series are seen, and to the physiographic evidence where only one member is visible. Faults determined by ordinary stratigraphic evidence are spoken of as 'actually observed' (266), while faults announced by previous observers on physiographic evidence are altogether rejected, apparently because they are not confirmed by

stratigraphic proof. As a matter of fact, no faults (meaning thereby surfaces of fracture on which movements of dislocation have taken place) have been actually observed as such in the Great basin; faulting is there as elsewhere a matter of inference. In the case of faults proved by stratigraphy, the termination of one series of strata against another may be more or less closely observed; and then instead of believing that both sets of strata were 'made so' in the beginning, it is reasonably inferred that they both originally had greater extension, that they were brought into their present relations by dislocation, and that the dislocated mass has been carved into its present form by greater or less erosion. This demonstration is commonly accepted as so compulsory and all other explanations seem so infinitely improbable, that faulting proved by stratigraphic evidence is often treated as if it were a matter of first-hand observation, and given an equal order of verity with the plain facts of strike and dip.

In the case of faults proved by physiographic evidence, the outcrops of a series of strata in an escarpment or on a mountain side are directly observed; and then instead of believing that they were made so, it is inferred that the invisible parts of the series have been in some way removed. On finding that their removal cannot be reasonably accounted for by erosion alone, the aid of faulting is invoked, a greater or less amount of erosion being supposed to precede and to follow. This argument of course involves such a knowledge of the observable facts of structure and form, and such an understanding of the processes of erosion and of the forms resulting therefrom, that, while certain forms (such as the Appalachian ridges of Pennsylvania) may be reasonably ascribed to erosion alone acting on a deformed structure, certain other forms (such as the Basin ranges) may as reasonably be held to be beyond production by erosion, without relatively recent faulting. This demonstration of faulting may be just as logical as that based on stratigraphy alone, but it is somewhat more complicated and it is much less commonly employed.

Now inasmuch as the block faulting of the Basin ranges has been determined in nearly every case by the physiographic method alone,