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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE CAPABILITIES OF PHOTOGRAPHY NOT UNLIMITED FOR ILLUSTRATING ALL CLASSES OF OBJECTS.

BY O. G. MASON, OFFICIAL PHOTOGRAPHER AT BELLEVUE HOSPITAL, NEW YORK CITY.

THE comparatively recent departure from old methods in various fields of scientific research, has called into action agencies for solving problems of initial progress and results not known or utilized by earlier workers. Discoveries within the last few years have so advanced the lines of study, and an active scientific press has scattered so broadcast the knowledge of progress made that, although the field is boundless, he who reads has little excuse for reworking ground from which all reachable fruit has been gathered. In eagerness for the new, a desire to find some hidden, shorter paths into the mysteries of nature, do we not often fail to recognize obstacles, or to sufficiently consider the best means for their removal? With pen and pencil our predecessors sought to leave a record of their work. What they thought and what they saw have been handed down to us through the best means at their command. For the physician, the botanist, mineralogist, and the geographer the artist sketched, elaborated, and finished illustrations having a more or less amount of truth, often obscured by some personality, which rendered them valueless or even misleading. In no class of objects have such defects been more conspicuous than that requiring the use of the microscope. Therefore, he who had used with dissatisfaction the hands of the draftsman was eager to utilize the means offered by photography. He had seen the results obtained in other fields, and, without knowing the difficulties in the way, believed it easy to obtain all desired brilliancy, detail, and amplification. It may be asked, Why have not these expectations been more fully realized? When we pause to consider that color is a most important feature in photographic work, and that a majority of objects studied under the microscope reflect or transmit the least actinic rays of light, red, orange, green, and yellow, we may well understand why we do not secure brilliancy. Again, when the microscopist studies his subject for detail, he mentally eliminates all those parts which do not belong to the special point under observation. A crystal, cell, or fibre which over- or underlies his object or forms a full or partial background in the field of the objective is left out in the mental summing up of his study. The laws of chemistry and optics do not permit such selection and elimination from the photographic image. A slight tremor conveyed to the microscope by a passing vehicle in the street, a step about the room or house, may be annoying to the observer, but does not prevent securing results by longer application. But when we consider the necessity of absolute immobility of the instrument, often for a considerrble length of time, in order to impress upon even the most sensitive plate the image of many-colored objects, we can well understand one of the greatest causes of failure to secure detail; and this obstacle of motion becomes far greater as the amplification increases It is plain that motion is multiplied equally with the diameter of the object; or, in other words, if we magnify an object one thousand diameters, a motion of that object to the extent of one-thousandth of an inch becomes in the amplified image a motion of one inch, which very readily shows why good results cannot be obtained under such conditions. When observing with the microscope, it is possible and quite feasible to focus the instrument above and below the general plane of the object, in order to study any projecting points which may be within or without the general plane. This feature is not possible with the photographic process, save in so far as diaphragming the lens and modifying the light may effect the result. *Over*estimation of the possibilities of photography and *under*estimation of the careful preparation of objects have occasioned much unnecessary labor and great disappointment by failure to produce results which should be sought through different channels. When the investigator contemplates the employment of photography for illustrating his work, let him consult his photographer before preparing his objects. No one human being has yet encompassed *all* that is known. When the anatomist takes to his photographer a *thick* section of muscular or ossified tissue and asks to have the individual striæ and cells isolated and delineated with distinct outlines and minute detail, he will fail to realize his expectation. When the mineralogist or geologist prepares his sections of crystallization or deposits, he must not calculate that *all* his various planes will be perfectly shown in one photograph, even if the specimen *be* translucent. Color, mass, and position are important factors in all photographic work. With orthochromatic plates many objects heretofore impossible of proper illustration may be quite successfully treated; but, with objects of this class, another factor, that of time of exposure, offers a barrier of limitation. The mobility of life, animal and vegetable, is a most important element which cannot be ignored in exposures of hours, or even minutes, and seconds. A vegetable fibre, when placed in concentrated light, may make one or more entire revolutions during the time of exposure necessary to properly impress its image upon an orthochromatic plate; and especially is this the case when a high-power objective is used. Thin sections devoid of the less actinic colors, red, orange, yellow, and green in their darker tints, or admixtures, may be easily treated. Circulating fluids or objects changing size or position are susceptible of instantaneous exposures only. When such objectionable features as motion and non-actinic color are present, the problem becomes far more complicated, and if the photographer fails in its clear and complete solution his patron sometimes looks upon such failure as a proof of incompetency or a lack of proper effort. Like her sister handmaids in the advance and illustration of scientific thought, photography stands ready to do her proper work. She has done much, and it is believed will do more to enlarge the field of human knowledge and gather the harvest; but we should not ask her to accomplish the impossible.

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.

Worms on the Brain of a Bird.

In the issue of *Science* for June 2, is a short account of my finding thread worms in the brain cavity of *Boturus mugitans*. The title of the article should have read "on" instead of "in," as they were not in the tissue of the brain but, as I state there, in the subarachnoid space.

Since writing the short article above referred to I have received a card from Professor J. W. P. Jenks of Providence, R. I., in which he gives an account of his investigation of a similar if not the same parasite on the brain of the Snake Bird (*Plotus anhingus*). To quote a little from his communication, he says:

"In 1874 I camped for 50 days near Lake Akechobee in south Florida, and shot dozens of the Snake Birds, and in 19 out of 20 mature birds found a bunch of 10 to 20 parasitic worms just beneath the arachnoid membrane, but in no instance extending

into the substance of the brain. In young just hatched I never found any. In young from two to three weeks old I found them in their stomachs and the alimentary canal. When about ready to fly I found coiled perhaps two or three on the brain."

Further on in his note to me he says: "I was surprised to learn of your finding them in *Boturus*—but I should not have been for I consider them primarily a fish parasite and developed from the eggs taken with the fish into the stomach of the bird, and hence like *Trichina spirulis* finding their way to the brain."

Professor Jenks called my attention to a note he published on this find in his "Popular Zoology," but which I had overlooked. He also gave me the address of Dr. W. Cahall of Philadelphia who had published an article on the subject, based largely on the material Professor Jenks obtained from Florida There is only one point in Dr. Cahall's article (Journal of Nervous and Mental Diseases for June, 1889), that I wish to speak of, and that is that while 19 out of 20 Snake Birds have these brain parasites they do not seem to affect them unfavorably. This was not the case with the Bittern. It was poor in flesh, of inferior size and deficient in intelligence.

That birds do get parasites from fish I might add the following case of circumstancial evidence: When skinning a perch (*Perca flavescens*), I found in the muscles a number of encysted parasites, the cysts white and about an eighth of an inch long A short time afterwards in skinning a wild duck I found a similar if not the same parasite in the pectoral muscles. The two parasites were of the same size and color and seemed to be the same.

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The International Botanical Congress at Madison.

IN looking over the "Circular and General Programme of the Forty-Second Meeting of the American Association for the Advancement of Science" just distributed, I am surprised to read on page 12, under the heading "International Botanical Congress," the following statement: "The congress will consider questions of general botanical interest, but papers embodying the results of research will be excluded. The International Standing Committee upon Nomenclature, appointed last year at the Genoa Congress, is expected to present a report at this time." This is all that is said in the circular to indicate what we may expect to hear at the Congress.

The *Botanical Gazette*, in an editorial,[1] urges "If any botanist has a suggestion . . . now is the time to give it expression. . . . Silence means apathy." I fear a certain class of our botanists have been silent too long, judging from the above statement. It seems to me outrageous to announce a programme from which all original research is excluded. No scientific man cares to listen to papers which are merely "a play of words," not the results of research. I should consider it an insult to our foreign guests to offer such a programme. The one subject suggested, *nomenclature*, is indeed about the only one possible under such restrictions, being truly void of all scientific research.

Botanical congresses do not come every year, especially in America, this being the first ever held here, if I am rightly informed. This being the case, it seems to me, as a matter of course, that this should be the time and place for a discussion of the vital questions of physiology, morphology, anatomy, etc., that this should be the time for an extreme effort on the part of every American botanist. If we desire to gain standing as true botanists among the true botanists abroad, our supreme effort should be directed to *botany*, not as appears to be the intention, to a mere machine of botany. It would seem a better restriction if all papers *not the result of research* were excluded.

Papers from America have long presented this characteristic — no "result of research." Nomenclature and floristic is truly all that we have thus far accomplished. One is, unfortunately, compelled to believe that "Free Lance"[2] accidentally omitted to include botany when he said: "The Entomological Society is recruited very largely from the ranks of 'collectors' who notoriously infest entomology far more than any other branch of natural history." The omission is at least unfortunate. The following sentences of the paragraph are so pithy and to the point that I cannot refrain from quoting them also: "The great majority of these have probably no interest in science generally, but care only for those things relevant to butterfly collections (herbaria, in our case). They would never become Fellows of the Linnæan, and care chiefly to discuss 'collectors' topics, that would be quite out of place in that society; so that the Entomological Society affords them a sort purgatorial limbo, midway between the paradise of science and the inferno of popular nescience."

I trust that I missunderstand the word *research* as used by the committee, but it would seem desirable that they should better explain what is meant. It may be intended that all papers containing research should be presented to Section G of the American Association, fearing that if the congress were not restricted Section G would be scantily patronized. This, however, does not seem a reasonable interpretation, for if there is a limitation on the congress, we should expect it to be open only to the best papers of most general interest, which could readily be decided by a committee on programme; lesser papers and papers of local interest being referred to Section G.

The claim cannot be made with justice that nomenclature has more than a factional interest. The majority of good botanists of the world pay no attention to nomenclature, and to them a discussion of its intricacies would be dry and worthless in the extreme. If such factional questions are to be the only ones considered, the congress should not be called a "Botanical Congress," but a *Nomenclature Congress*. Whatever may be intended, it is an unfortunate use of words.

It is announced that a separate circular will shortly be distributed to botanists, giving further information. It is to be hoped that a clear explanation of this point will be given.

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[1] Botanical Gazette, vol, xvii. (November, 1892), p. 384.

[2] "On the Organization of Science," by A. Free Lance, Edinburgh, 1892, p. 25.

A Plea for a Fair Valuation of Experimental Physiology in Biological Courses.

DURING the discussion of the biology question, one point has interested me more than any other, namely, that none of the parties who have taken part in the discussion have been able to avoid speaking at the same time of evolution and of natural selection. This thinking of biology, with constant reference to those two features of Darwinian teaching, has led me to believe more strongly than ever that my view of the matter is not very much wrong. However, an article in this journal, entitled "Biology in our Colleges: A Plea for a Broader and More Liberal Biology," induces me to take up my pen once more and explain matters a little more closely.

The tendency of the above-named paper "is — a plea for systematic biology," but it is marked by such a number of wonderful views on the different lines of physiological investigation that many specialists will really be at a loss about what they shall think. "Systematic zoölogy has gone, or, if still tolerated in a few colleges, is restricted to a very subordinate position." I imagine that the biologist would not know what to do if systematic work, both zoölogical and botanical — the latter holds still, says the article, "an honored place in many universities, though evidently on the wane" — was not carried on, so that we could know how to lay our hands upon the different forms for further study. But the methods of such a work may be wrong, and, fatally, often are so, namely, when it presents itself merely as simple regristation work, which strikingly has been called museum zoölogy or botany. Systematic work of any kind is to be valued just as much as morphological or physiological work, and so, even if it is done still — as in fact it is in ninety-nine cases out of a hundred — after the old Linnæan principles. On the other hand, a biological classification, or even only a morphological classification, which employs biological characters of the forms, is to be more highly valued.

There is no doubt but that any naturalist enjoys the "delight