

and the division of the "generative cell"; but occasionally they persist somewhat longer, and Strasburger figures a pollen-grain in which the three prothallium cells are intact, and the first of them has a partition at right-angles to the walls of the other cells. In this work Strasburger also gives the results of some experiments on the colour-reactions of the male and female nuclei. Rosen discovered that, as in animals, the male nucleus of phanerogams is kyanophilous and the female nucleus erythrophilous. Strasburger found that the small nuclei of the cells formed in the pollen grains of gymnosperms were kyanophilous, whether the cells were vegetative or destined for generation; but the nucleus of the pollentube was more or less decidedly erythrophilous. The second and larger portion of this "Beitrag" is devoted to swarmspores, gametes, vegetable spermatozoids, and the nature of fertilisation.

W. B. H.

Autres Mondes. By Amédée Guillemin. (Paris: Georges Carré, 1892.)

WHETHER the author of this small volume thought that the sequence of the subjects dealt with was really quite unimportant, or whether no order at all was intended, puzzled me considerably when glancing through these pages for the first time. To be suddenly led off without a word of warning into "L'infini dans le temps et dans l'espace," and then to be as suddenly pulled back again to a second chapter dealing with Sirius seems rather a large oscillation to commence with. The same remarks might apply to the next chapters, for they treat consecutively of "The Cluster in Hercules," "Structure of the Visible Universe," "Movement in the Universe," and "The Nebula of Orion," followed up by chapters on "The Age of Stars," and "The End of the Solar System."

That the work is written by M. Guillemin is quite sufficient guarantee that strict accuracy is throughout adhered to. The book is one that can be picked up at odd moments and a chapter or two read with delight. The illustrations are excellent copies of lunar and stellar photographs taken by the brothers Henry at the Paris Observatory.

W. J. L.

Some Lectures by the late Sir George E. Paget, K.C.B., F.R.S. Edited, with Memoir, by Charles E. Paget. (Cambridge: Macmillan and Bowes, 1893.)

THIS volume will be cordially welcomed by the late Sir George Paget's friends; and members of the medical profession, whether they knew him personally or not, will find in it much that cannot fail to interest them. The lectures deal with three subjects—the ætiology of typhoid fever, alcohol as a cause of disease, and mental causes of bodily disease. In dealing with each of these topics, the author presents the results of prolonged and most careful observation; and it is impossible not to admire the directness, lucidity, and vigour with which his facts and conclusions are set forth. The memoir, by the editor, is a short and attractive record of Sir George Paget's distinguished career, and its value is increased by the fact that it is accompanied by an excellent portrait.

LETTERS TO THE EDITOR.

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Origin of Lake Basins.

WE may all thank Mr. Alfred Wallace for putting together so concisely the main arguments on which the glacial theory of the origin of all lake basins has had a wide acceptance. My time

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is just now so occupied with "earth movements" of another kind that I am unable to marshal all the arguments on the other side. But I shall try to put the main points as clearly as I can.

I accept Mr. Wallace's correction of the word "grinding" as the best word to describe the action of glaciers. It is better than either "digging up" or "scooping." Many men who account for marine gravels on such places as Moel Trefan mountain-top by the action of glaciers, must conceive of glaciers as capable of digging out and lifting up. But I agree with Mr. Wallace that "grinding" down is the best expression for true glacier action. This is the *mode* of action; but what of the *cause* of the motion which effects the grinding? Are we agreed on this? Mr. Wallace does not explain his view on this point. I hold that the only cause of true glacier action is gravitation, and that masses of ice will not move at all, or exert any grinding action, except when impelled by gravity down gradients more or less steep. Even if they do mount up some slopes, it is only when they are violently pushed by other masses moving down slopes from behind them. If this be true, then glaciers will not tend to dig holes out of the flat bottoms of valleys. Mr. Wallace says they will, if they are exceptionally thick. This is very doubtful: and still more is it doubtful that they can dig holes of a very peculiar character, such as is now proved to be the character of Como and other lakes, with steep and sharp outlines, or with barriers left untouched. One single fact of this kind, if well ascertained, is quite enough to upset a great theory, because it may be sufficient to prove that at least *some* lake basins *cannot* have been made by glaciers. And if some have not, it is not certain that any have been made by glaciers alone.

The constant association of lake basins with glaciated countries is Mr. Wallace's grand argument. But it is explicable in the theory of earth movements quite as easily as on the theory of glacial action. Glaciated countries are generally hilly, or mountainous. If Mr. Wallace believes that all hills and valleys are due to superficial *sculpturing alone*, of course his argument is facilitated. But if hills and valleys are even in any measure due to earth movements—crumpings of the surface—then the formation of lake basins is an inevitable necessity. Every hollow must become a lake basin which has no natural outlet except at a higher level than at its own bottom. Yet if there be such a thing as earth movements at all, it is in the highest degree improbable that they should have failed in numerous cases to occasion hollows in which water would accumulate.

Mr. Wallace's unbelief that any earth movements have taken place so lately in geological time as the glacial age—say 100,000 years ago—is a declaration that does indeed astonish me. I can understand great doubt and difficulty as to the extent of these movements. But that they have taken place to some extent very lately indeed is, in my opinion, demonstrable in the country in which I now write. There is one old sea beach on the Island of Jura where the stones as left by the surf are as bare of vegetation and as unaltered in forms which show surf action, as if the ocean had beat upon it last year. And this sea beach extends for miles at elevations varying from 120 to (I believe) 160 feet. If I am not mistaken, recent surveys of the great Canadian and American lakes have proved that they lie in hollows of crumpled and distorted land surfaces. The whole of Mr. Wallace's theory on this subject seems to me to be out of date. The distribution of boulders in the Highlands can, in my opinion, be accounted for in no other way than the transport of masses of stone on floating ice. But putting aside altogether this larger question, if a "great submergence," as one of the latest events in the glacial epoch, smaller elevations of the land are among the most certain of geological facts. But if so, we have lake-basins in all hilly countries easily explained. Very often the elevation of land to a very small extent indeed, if unequal, as it is sure to be more or less, would immediately cause lakes wherever a pre-existing valley had its lower end more tilted than its upper end. The 120 feet which is represented on the coast of Jura in this county is an elevation which would fill half of our glens all over the county with lakes unless it was an elevation perfectly equal along the whole of pre-existing contours. The co-existence of lake-basins with hilly and glaciated countries, therefore, admits of an easy explanation without attributing to ice a kind of action which has never been proved to exist at all. Hilly countries are *crumpled* countries, and slight increases or decreases of the same action must of necessity produce lakes.