

tricts, they do not occur in the immediate vicinity of granite, but elvan courses are always found near them. The per-centage of silica in the two series of rocks is nearly constant; the hornblende slates contain about 10 per cent. less silica than the crystalline pyroxenic rocks, and there is an excess of iron oxides to nearly the same extent, their composition in other respects being very similar. The Killas is an acidic rock of essentially different chemical composition.

2. "On Columnar, Fissile, and Spheroidal Structure." By the Rev. T. G. Bonney, M.A., F.G.S.

Some of the above structures have comparatively recently been discussed by Mr. Mallet and Professor J. Thomson. Both these authors agree in attributing columnar structure to contraction due to loss of heat while cooling, but differ in their explanation of cross jointing and spheroidal structure. In this paper it is sought to show that the principle proved by Mr. Mallet to be the explanation of the columnar structure is capable of a wider application. After a brief notice of some instances of columnar structure, the author described cases of a fissile structure seen in certain igneous rocks (especially in the Auvergne phonolites), closely resembling true cleavage, and often mistaken for it; also the tabular jointing of rocks; a peculiar form of this, where most of the segments are of a flattened convexo-concave form; spheroidal structure and cup-and-ball structure. He showed by examples that Prof. Thomson's explanation of spheroidal structure was inadequate, and gave reasons for considering all these structures to be due to contraction. He also discussed more particularly the cup-and-ball structure, giving reasons for thinking that the spheroidal and the horizontal fissures were often to some extent independent of each other.

CORRESPONDENCE.

ON THE ORIGIN OF LAKES.

SIR,—I feel sure, that when your correspondents of last month are able to peruse my article on this subject in a somewhat calmer frame of mind, they will be prepared to acknowledge that they have done me (no doubt through inadvertence) some injustice. For, so far from attributing to them the opinion that *all* lakes were formed by glacial erosion, I was most careful to state (see page 7) that they regard the agent in question "as having originated the *greater part of the rock-basins* in which lakes lie." Whatever may be their *present* opinions—and I venture to hope that we may detect a tendency towards a more cautious treatment of the question—Mr. Geikie and Prof. Ramsay will, I am quite satisfied, altogether acquit me of any exaggeration of what they have *formerly* written on the subject. Unfortunately, the greater part of the contents of your budget of letters is devoted to the rebuke of this *purely imaginary* act of misrepresentation on my part.

I sincerely trust that, after the admonitions of Prof. Hull, and the amusing burlesque of Mr. Geikie, no one will be so foolishly bold as to attempt to *prove the negative* of the proposition that

“glaciers are able to scoop out vast depressed areas of enormous depth in their beds.” For my own part, I can honestly assure those gentlemen, that I never for one moment dreamt of making any such wild attempt; nor did I ever lose sight of the fact, that in this indictment of the glaciers for lake-making, the task of substantiating the charge against them rests with those who have advanced it. The verdict for which I contended, and with which I shall be perfectly satisfied, is one of “not proven.”

The manner in which Prof. Ramsay and Mr. Geikie endeavour to obtain a conviction against the glaciers is as follows. They urge that there are certain rock-bound basins of which the formation can be ascribed neither to marine nor to river action, and with regard to which there is a total want of evidence by which their origin can be referred either to special subsidence, to synclinal folding, or to faulting of the rocks among which they lie. This premised, they proceed to triumphantly announce their conclusion:—“But one agent is left, and that is *ice!*”

Now, it is clear that the whole force of this reasoning depends on the completeness with which the authors of it have cited and disposed of the competence, not only of every possible agency, *but also of every possible combination of agencies*, by which the lakes in question could have been formed, except that of ice-excavation. It is just at this point that I venture to join issue with them.

Just as in the well-known fable, the article was most undoubtedly stolen, though one of the culprits was able to declare that he “did not take it,” and the other that he “had not got it,” so there are lake-basins with regard to which it may be very safely asserted that meteoric action could not have excavated them, nor could subterranean forces have moulded them; but concerning which it is, nevertheless, also true that the two agents, *acting in conjunction*, are able to give a perfectly good account.

Prof. Ramsay holds that my line of argument may be “easily disposed of,” and Mr. Geikie that it has been “answered already.” They will forgive me for reminding them that the only reply that has ever been vouchsafed to it altogether and most signally failed to convince the candid and truth-loving mind of the late Sir Charles Lyell, as every reader of “*The Antiquity of Man*,” and the “*Student’s Elements of Geology*,” must be well aware. Dr. Hector, too, in replying to Captain Hutton (who reproduced this “answer” of Prof. Ramsay), follows precisely the same line of argument as Lyell—an argument of which the validity and force are admitted by almost every Alpine geologist. What a happy thought then must that have been to the author of “*The Great Ice Age*,” when it occurred to him to carefully avoid all mention of Lyell’s objections to the theory;—such a discreet, and withal *convenient*, way of dealing with the question!

Far be it from me to rely in this matter on any authority, however eminent, or to cite the opinions of a majority, however overwhelming. But when we are gravely assured that this notion of the erosion of lake-basins by ice “bids fair to become one of the

most generally accepted theories in geology," is it possible to avoid a smile at the very sanguine temperaments with which the promoters of this theory would appear to be so happily endowed?

I have, however, to thank Prof. Green for so fairly raising what is the real point at issue in this controversy—that of the relation between the effects of subterranean and subaerial forces. He tells us that he and his friends are prepared to admit that *before* denuding agents can carve out hill and valley, the subterranean forces must have brought the rock-masses within their reach; he is moreover convinced that the *original* lines of drainage must have been determined by the action of the same forces; and, still further, that, though the details of the contours of mountains are due to meteoric agencies, their superior elevation is the result of the concentration of subterranean energy beneath them.

So far well! But will my friend permit me to invite him to accompany me just one step farther in the same direction. Is it not certain that *not only before the commencement* of the slow process of sculpturing by meteoric agencies (in which we are both such firm believers), but actually *while those forces are in operation*, subterranean actions, attended by more or less local surface movements, were going on *side by side* with, and modifying the effects of, the subaerial forces? Does he shrink from this admission? If so, why? Has he any grounds for the belief that all the subterranean action took place at one period, and all the subaerial at another? Such an admission as I ask him to make would never have alarmed either Lyell or Scrope, who fought a good fight for the Huttonian doctrines before almost any of us the younger champions of the theory were born! Is there anything in it inconsistent with the teachings of Hutton and Playfair themselves?

Have not our perceptions become just a little numbed through our dwelling too long in the region of glaciers? Geology has had its day of universal deluges; it is now passing through its "*great ice age*." We are persuaded, however, that as it has survived the former, it will emerge safely from the latter; and even now we begin to see the signs of the setting in of more temperate mental conditions. I cannot help venturing to hope (for may not I too be *sanguine*, for once?) that at no very distant date I may have the pleasure of wandering with my four opponents of to-day among Alpine or Scottish lakes, all joining in a hearty laugh at the strange theory that was once maintained concerning their origin.

JOHN W. JUDD.

GLACIAL EROSION.

SIR.—In Mr. Judd's very interesting paper on Lake Balaton, there is, besides a vindication of the claims of subterranean forces to be the true originators of lakes lying in rock-basins, an attempt to show that glacial erosion can never be regarded as a *vera causa* in any case in the formation of lakes.

I am afraid that the eminent geologists who write on behalf of glacial erosion, as one of the causes producing lakes, in the current