large CAVES containing fossil remains have been discovered in the limestone country between Cowra and Canowindra, on the road between these two places, and between 70 and 80 miles from Bathurst. The description of the caverns differs in no wise from those of other limestone districts where streams have dissolved more or less extensive chambers in the softer limestone, these chambers being connected by narrow passages, and lying at different elevations with regard to each other. The caverns are large, and decorated with stalactites. The roof of one chamber in the principal cave is said to be from 80 to 90 feet high, with some 12 or 14 openings leading into other cavities. They are stated to descend from 300 to 400 feet beneath the surface. Nearly all the caves and chambers have their floors composed of a thick layer of *ossiferous breccia* apparently quite undisturbed. Only one cave seems to have been dangerous from "*choke-damp.*" The writers are very enthusiastic as to their discoveries, and propose to explore another cave reached by a shaft 100 ft. perpendicular. We trust that no lives will be lost in this somewhat Quixotic fit of cave-hunting, and shall look forward with interest to Mr. Gerard Krefft's report on the bones from this extensive series of caves and fissures in New South Wales. —EDIT. GEOL. MAG.

GLACIAL ORIGIN OF LAKE-BASINS.

SIR,—I fear the points of difference between Mr. Hugh Miller and myself are hardly such as can be cleared up in the compass of a letter, but yet I should like to make one or two remarks on his paper, because I still think we are to some extent misunderstanding one another. My letter (p. 376) was chiefly devoted to the reasoning in Mr. Fisher's paper (p. 253); the paragraph alluding to Mr. Miller's letter (p. 287) was simply intended to call attention to a defect in his reasoning (where he now admits that he did himself injustice) and to guard against what seemed to me a misconception of my argument. An unfortunate printer's error, or slip in writing in my manuscript (it was not possible for me to revise a proof), made my meaning less clear than it should have been. Expanded, this is what I intended to imply—"Suppose you prove that a certain number of small-sized sheets of water (to avoid ambiguity we will say such as Grasmere, or less) are most easily explained by the hypothesis of glacial erosion, it does not follow that very large and deep sheets of water (such as Como) are most easily explained by the same hypothesis." The first paragraph of Mr. Miller's letter, and parts of the second and third on page 287, appeared to mean that he claimed to reason from the examples which he quoted to those cases which I have always disputed; and in his paper (p. 453) he seems to still maintain this: "It appears to me that no halting-place can logically be found by those who, with Sir Charles Lyell, allow only some mountain tarns to Prof. Ramsay's demand for lakes." It is this which I dispute. Perhaps the halting-place may not be impregnable to an attack on the destructive 'Sorites' method; but in science and in every-day life, we are constantly obliged to take our stand on

similar halting-places. Thus, because blown sand erodes, am I to apply this agent in all cases where there is nothing in the nature of the cases directly to contradict me; or because a pebble on a sheet of rock may most probably have been thrown there by a lad, am I to attribute a big boulder on the same to the games of heroic youths in pre-Homeric ages? I may know that the Aletsch Glacier maintains the Märjelen See, and yet doubt the existence of a vast ice-barred lake in Northern Europe. Each case, as it seems to me, must be separately judged, having regard to all the surrounding circumstances. From this position, I have never consciously receded. I admit some tarns, I admit, though with greater hesitation (for reasons which I have stated), some 'lakelets,' to be the work, wholly or in great part, of ice. I cannot believe that ice has been more than a very secondary agent in forming the great Alpine lakes.

I still venture to think that Mr. Miller's reasoning (p. 453) does not remove the difficulty which I have brought forward as to the forms of the Alpine valleys above the great lakes. I have tried to show that there the glacier is as *nearly as possible powerless* as an erosive agent, or at any rate that it has only superficially modified forms, which we agree in associating with the action of running water. The glacier has all along been "indentured" in a groove, but it has been a thoroughly idle apprentice, till some cause, no more permanent than the master's stick, has quickened it into intense but brief energy. Como, Lugano, Brienz, the König See, and many others, are vale-confined glaciers: so are the greater parts of many other lakes. But with regard to these difficulties, I must content myself by referring to what I have already written.

One more point; for I do not attempt to criticize Mr. Miller's special Scotch case, as I have not examined the district. The Alps cannot be expected to give much indication of the evidence of profile which Mr. Miller demands. Plains of marine denudation cannot, so far as I know, be recognized there. I am not aware that the sea has flowed among their summits since a period prior at least to the last great movement. Mountain contours, in the regions of most lakes that I have mentioned, are so irregular that we cannot hope to recognize clearly these curvatures in them, any more than in their disturbed strata. It is a point, however, which I have not overlooked in my investigations, and may say that, while I have found nothing in this respect opposed to my theory, I have observed a few things making for it slightly, but so slightly that I preferred not to bring them forward.

T. G. Bonney.

CARBONIFEROUS AND POST-TERTIARY POLYZOA.

Sir,—In the Geological Magazine for October, 1873 (Vol. X. p. 433), I proposed the name *Carinella* for a new genus of Carboniferous Polyzoa. I find that this term was pre-occupied, having been used by the late Dr. Johnston for a genus of recent Nemertidian Annelids (see McIntosh, Annals Nat. Hist. 1874, vol. xiv. p. 154), and I am therefore desirous of proposing in its place that of *Goniocladia*. I described one species (*G. cellulifera*), the only one at present known.