Das Karstphänomen. Versuch einer morphologischen monographie von Dr. Jovan Cvijić. (Wien: Ed. Hölzel.)

NOTWITHSTANDING its rather pompous second title, this is an interesting and valuable book, which, however, is not a separate work, but the third part of the fifth volume of a geographical series (*Abhandlungen*) edited by Prof. A. Penck. Its subject may be briefly stated as follows:—In many limestone districts the surface of the rock is guttered by channels—sometimes small, sometimes large—varying from comparatively smooth to rough. Here each ends in a small pipe, which descends vertically into the rock; there they converge towards one of larger size. With this system of superficial drainage are associated hollows of various forms, "blind valleys," and the like, and caves are likely to be common. A region which exhibits some or all of these phenomena is called, from the peculiar sculpture of the surface, a *karst* region. Such may be found in various parts of the world. It is represented in England by the furrowed limestones and "swallow-holes" of Derbyshire and Yorkshire; it occurs in many parts of the Alps, the phenomena becoming more frequent eastward, till their headquarters are reached in the Julian Alps and the great "Karst plateau," north of the Gulf of Fiume. As they occur in many lands, so they bear many names. A full, exhaustive, and elaborate account of these interesting phenomena will be found in this memoir, perhaps with an affected attempt at precision in distinction and classification (for after all, though curious, they are simple in origin), together with abundant references to the literature of the subject. Its usefulness, however, would be greatly increased by an index or by a very full table of contents; and though it is paged continuously with the volume, the latter, at least, ought to have been given. T. G. B.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Origin of Lake Basins.

THE most thorough-going glacialist could find no ground for complaint that Dr. Wallace has not gone far enough in his most interesting advocacy of the glacial origin of lakes. I do not propose to enter into any general discussion of this question; that glaciers can excavate rock basins is indisputable, but there is a limit to their power, and this limit I believe to be reached far short of even the larger of our English lakes. The controversy is of long standing, and there is little new to be said on either side; nor would I have desired to re-enter it, but that Dr. Wallace's article seems to me to contain one serious fallacy and one vital misstatement which have not as yet been noticed, though they should not be left uncorrected.

The fallacy is not a new one; it may be found in the writings of more than one of the advocates of the glacial theory, and is contained in the argument that because lakes are found in regions that have been extensively glaciated, and are not found in regions precisely similar in every respect, except that there has been no great extension of glaciers, therefore the rock basins in which the lakes lie were excavated by glaciers. I trust I have not misrepresented the argument in this succinct statement of it; but such condensation is useful if we would detect a fallacy, and in this condensed form the fallacy of the undistributed middle term becomes conspicuous. The term "lake" is by no means coextensive with the term "rock basin," and it is not the water filling the lake which requires explanation so much as the basin that it fills. A rock basin filled with alluvium is a rock basin still, and requires explanation as much as if it contained water, and was consequently a lake.

The misrepresentation is to be found in Dr. Wallace's limitation of what he rightly regards as the only tenable alternative theory, that the rock basins owe their origin to deformation of the surface immediately before the advance of the ice. This limitation of time is so extraordinary that it would have passed for an accident or oversight, but that it is repeated at greater length on the very next page; did it form any essential part of the theory, this would deserve all the strictures passed upon it, but such is by no means the case. Without entering into the question of whether the geologists quoted by Dr. Wallace accept this limitation of time, I may point out that it is altogether more reasonable to regard the deformation as having taken place *after* the advance of the glaciers. We know that during the glacial period there were great changes of level, and it is reasonable to suppose that these were not absolutely uniform; moreover, had the rock basins been formed before the ice was there to fill them, they would mostly have been filled at once by river deposits, as has been the case in nonglaciated regions, and once filled up they would remain so on this theory, for if a glacier cannot erode a rock basin it cannot clean out one that has been filled up with stream deposits. This alteration of time makes the theory more natural and acceptable; when a rock basin is formed in the course of a stream by elevation or subsidence no lake arises in the great majority of cases, as either the barrier is destroyed by corrasion, or the hollow is filled up by deposition, as fast as it is formed; but when the basin arises underneath a glacier it becomes occupied by ice, and on the retreat and disappearance of the glacier a hollow is left that is at first filled by water, forming a lake, and only subsequently by degrees filled up by stream deposits. In this way the connection between the present distribution of lakes and the areas of pleistocene glaciation is easily explicable, and it is consequently not admissible as an argument to prove that the lake basins were excavated by glaciers until it is shown that in the nonglaciated regions, where there are now no lakes, there are also no rock basins.

With most of the regions quoted by Dr. Wallace I have no personal acquaintance, but in India such do certainly occur, and have as certainly not originated by glacial erosion; in some cases the existence of the rock basin has been proved by borings, but besides these there are many more instances where there can be no reasonable doubt of the existence of a rock basin, though the final test has not been made. R. D. OLDHAM.

IN his last communication Sir Henry Howorth makes two statements which are so erroneous and so misleading that I cannot allow them to pass without correction. The first is, that Mr. Deeley "repudiates Dr. Wallace's notion that regelation can in some way act as a compensating element when crushing supervenes in ice." Here is a double misstatement. Mr. Deeley "repudiated" no notion of mine, or he would, I am sure, have said so plainly, and he said nothing whatever about "crushing." Neither did I say a word about regelation acting as a "compensating element," for I do not believe in the crushing of glaciers by their own pressure. I asked Sir Henry what would happen to the ice after it was crushed, the pressure continuing; and I get no reply but the above double misstatement.

Then, further on, Sir Henry says: "Mr. Wallace confesses he does not like to face these mechanical issues." This is simply untrue. I "confessed" nothing of the kind, and I challenge Sir Henry Howorth to quote any words of mine which will bear such a meaning. I maintain that his "mechanical issues" are pure theories, and are beside the question of the actual facts of glacier notion. Lastly, he attempts to evade the real issue between us, which is, that he himself accepted Charpentier's conclusions as to the extent of the Rhone glacier, but refuses to allow me to use these same conclusions as a datum in the discussion.

I have now shown ample reason why further discussion of this matter with Sir Henry Howorth must be unprofitable.

ALFRED R. WALLACE.

The Second Law of Thermodynamics.

I AM unable to see any reason for regarding Clausius' supposed deduction of the Second Law as in any way limited by the condition stated by Mr. Burbury, viz. "that the system be conservative, that is, that the external as well as the internal forces acting on it are to be derived from a potential." No such limitation was contemplated by me when I was preparing the Report for the British Association.

It is true that this assumption is made in § 17 of the Report, in order to establish the closest possible connection between the