analysis, without being convinced that this hypothesis is nothing but a delusion and a snare, and that the quicker it is thrown aside and abandoned the better it will be for geological science" (*American Journal of Science*, vol. xxiii. p. 287).

I take this opportunity of pointing out a mistake in my book. At page 156 the number 1127 ought to be 1734; and consequently the number 0·996 ought to be 0·965. The argument will still hold. O. FISHER

Harlton, Cambridge, November 9

P.S.—Since forwarding the above I have observed a note at p. 912 of Dr. Geikie's "Text Book of Geology," in which he says that I have "endeavoured" to show that the secular contraction of a solid globe through mere cooling will not account for the phenomena. The word "endeavoured," does not express the attitude of my mind upon the question. Forty-two years ago the contraction theory occurred to myself independently. I remember that in my youthful joy at what I thought thought a discovery, I forthwith vaulted over a gate! In 1868 I read my paper on "The Elevation of Mountains by lateral Pressure," fully believing that I was elucidating the cause which had produced them in the contraction through secular cooling. In 1873 I began my paper on "The Inequalities of the earth's Surface viewed in connection with the Secular Cooling," while still under the same impression. I first of all estimated the actual elevations, and, this done, I calculated the amount of those which would be formed upon Sir William Thomson's view of the mode of solidification. To my excessive surprise, the result showed the utter inadequacy of the contraction hypothesis. I thought I must have made some error in the calculations, but could find none. I still, however, adhered to the original idea of contraction, and suggested, towards the end of that paper, a fluid condition of the interior at some *former* period, thinking that sufficient contraction might be perhaps obtained by that means; for I had not yet dared to question Sir Wm. Thomson's dictum of the *present* complete solidity of the earth. It was not until about a year ago, when I wrote the chapter in my book about the "Amount of Compression," that I perceived that even the condition of a liquid substratum would not give the necessary degree of contraction to produce the compression. I have thus been driven from the contraction hypothesis step by step, and have by no means been endeavouring to support a preconceived opinion against it.—O. F.

Shadows after Sunset

HAPPENING by chance to look into "Loomis's Meteorology," after reading M. Dechevren's account of the blue, white, and red bands visible before sunrise and after sunset at Zikawei, I noticed under the above heading the following account of shadow-bands, which not only appear to be very similar to those observed by Dechevrens, but are explained in identically the same way ("Loomis's Meteorology," p. 107): "A similar phenomenon [to the water-bands described in the preceding paragraph] is frequently noticed about fifteen minutes after sunset, when the shadows of clouds near the horizon are projected upon the western sky in the form of radiant beams diverging from the sun. These beams are parallel lines of indefinite length, but from the effect of perspective they seem to diverge from the sun, and if they could be traced entirely across the sky, they would for the same reason converge to a point directly opposite to the sun. Such cases are sometimes, though not very frequently noticed. Similar shadows are sometimes seen in the morning before sunrise, and form a conspicuous feature of the morning twilight. This effect is sometimes noticed in nearly every part of the world. It must have attracted the attention of the ancient Greeks, and is thought to explain that poetic expression "the rosy-fingered dawn."

M. Dechevrens appears to think the phenomenon does not occur in Europe or temperate latitudes generally, but from what Loomis says, one would infer that he may be mistaken in this, and that to a modified extent it may be visible in Europe and America. Perhaps some of your readers who are in the habit of observing the face of the sky will be able to verify this supposition. For my own part I have not remarked it in England, but have occasionally witnessed it in Bengal during the rains, very markedly. The explanation offered by M. Dechevrens seems the only reasonable one under the circumstances, but he hardly seems to lay sufficient stress upon the fact that when the sun is below the horizon his rays can only illuminate a shallow stratum of partially condensed vapour in the upper atmosphere. Any obstruction of his rays will consequently shut off the *whole* of the reflected light from this stratum, and cause the blue sky to appear through the shadow, all the more cerulean by contact with the whitish or rosy colour of the adjacent portions which still bask in the solar rays. E. DOUGLAS ARCHIBALD

An Abnormal Fruit of Opuntia Ficus-Indica

THE accompanying figure represents a fruit of *Opuntia Ficus-Indica*, which is wholly inclosed in one of the well-known flat branches of this plant; normally the fruits appear as exserted obovate bodies on the margin, or on either side, of the branches. The figure is exactly half natural size; the fruit is therefore full grown. There is no interruption in the ascending curves of spinous tubercles, only they are somewhat smaller on the fruit, which has also a less wrinkled skin than the remainder of the branch. It is of rather uncommon occurrence, nobody having seen here anything alike in the extensive *tunales* or Indian fig-plantations of our neighbourhood; nor have I been able to find any mention of such a case in the books at my disposal. It is evidently an instance of non-development of peduncle, a special case of suppression of axile organs (Masters, "Teratology," p. 393). But I think it throws also some light on the nature of what generally is taken to be the pericarp of the Opuntia fruit, which, after all, seems to be a slightly modified branch, bearing the ovary of the flower in a cavity on its

Abnormal Fruit of *Opuntia Ficus-Indica* from Carácas.

upper end. A similar view is held forth by Dr. Noll in a paper published in the *Annual Report* of the Senkenbergische Gesellschaft (Frankfurt, 1872, pp. 118-121, with two plates), where he describes and figures two abnormal fruits of *Opuntia coccinellifera* from the Canary Islands, with branches growing from the exterior part of the fruits. Their apparent pericarp is therefore an axile organ of a certain order, say of the order n, whilst the additional branch is of the next order, $n+1$. The case which forms the object of the present note is quite the reverse of those mentioned by Dr. Noll, as the branch of order n, or the exterior part of the normal fruit, is not developed independently, being represented by its parent-branch of order, $n-1$.

If this view be correct, there can no longer be any reason for speaking of an *exserted ovary* in Opuntia (Hooker and Bentham, "Genera plantarum," I., 851), as this organ is wholly sunk in the interior of a branch, just as it happens in other Cacteæ with an *ovarium immersum*. A. ERNST

Carácas, October 4