nowever, that when the origin in glacial time of the grand Norwegian fjords is sufficiently proved, their origin by glacial forces will be more easily granted. The same may certainly be said of the far smaller lake basins in Norway, for which an analogous demonstration can be given. That the fjords now must really be of pleistocene origin is the point I wish to make in this letter. Only if anyone can, in a simple manner, explain how an inland ice could be able to pass the close set row of fjord heads, is it possible to dismiss my argument.

ANDR. M. HANSEN.

University Library, Kristiania, January 29.

A FEW words are due from me in reply to the kindly criticisms of my suggestion regarding the erosion of rock basins that have appeared in NATURE since its publication on November 9, 1893.

In the first place, I must apologise to Sir H. Howorth for having misunderstood his remarks on the plasticity of ice in his letter of July 13, a misunderstanding due, of course, to my not having had an opportunity of reading the chapter devoted to the subject in his book. Unfortunately the libraries of our small outlying stations in India do not as a rule provide us with works of scientific interest, and the conditions of life of most of us who take an interest in such subjects out here force us to content ourselves with the possession of very few books of the kind, and only those that are absolutely necessary for our work. Provided that it is admitted that the plasticity of glacier ice is sufficient to allow motion in the upper layers of a glacier, even when it rests on a nearly level surface, it does not matter, so far as my hypothesis is concerned, whether the bottom layers move or not, for a movement of the upper layers alone is required to enable the "moulins" to transfer their action from place to place, and in time to exert their force on every part of the rock surface beneath that portion of the glacier.

That the action of the "moulins" is not so restricted as would appear from Prof. Bonney's letter in NATURE of November 16, 1893, can, I think, hardly be doubted by any one who has traversed a Himalayan glacier of the kind I have described, on a hot summer's day. Hundreds of them may be seen in action in every direction, and, given sufficient time, their aggregate effect in wearing down the rock surface must be very large. I have noticed the dry shafts mentioned by Prof. Bonney in front of an active "moulin," but do not see why they should not be accounted for by the opening of a new crevasse, without having to suppose that the new crevasse was in the same position as the old one. The crevasses to which I refer are mostly very narrow, easily stepped across in many cases, and do not appear to extend far down into the glacier, so that they are probably due to some other cause than an unevenness of the rocky floor, which would cause them to form in succession at the same point, and their number would give the "moulins" plenty of opportunity to attack the whole surface in course of time. Besides, the wearing away of any inequality that did exist, would surely cause the crevasse to open at some other point, if it were due to that cause, and the "moulin" would thus be enabled to shift its point of attack. The very rarity, too, of such collections of "giant's kettles" as that at Lucerne would seem to show that it is seldom that the "moulins" keep working at one point for any length of time. I did not mean to suggest, of course, that any lake basin had been due to the action of one "moulin"; the hollow ultimately produced need not bear any relation in form to the individual "giant's kettles" that gave rise to it; indeed, there is no necessity that a real "giant's kettle" should be formed at any one point. Just as in the case of a drill moved over the surface of a piece of wood, the pattern ultimately produced need bear no relation to the form of the drill.

If we except the doubtful action of the ice itself, I do not know of any agent that will produce a rock-enclosed hollow in the course of a river channel, but falling water, aided by boulders and sediment. Such a hollow may be seen at the foot of any waterfall, even of moderate height.

In calling attention to the rarity of true rock basins in the Himalayas, an expression that Mr. Oldham takes exception to, I should have said lake basins, that is, lakes lying in true rock basins. As I pointed out, any hollows that may have been formed beneath a pre-existing glacier have been filled with *débris*, but it is very likely that such hollows do occur beneath the extensive flats found at the foot of the larger glaciers, as in the case of the one shown in the view given in my paper. Of course, where such hollows occur in positions where it is impossible that glaciers ever existed, as in eastern Baluchistan, they must be accounted for in other ways. My suggestions were not intended to account for all rock basins, but merely to apply to those which occur in now or formerly highly glaciated regions, where it seems possible that there is an intimate connection between the excavation of the basins and the existence of glaciers.

Sukkur, January 10. T. D. LATOUCHE.

A Plausible Paradox in Chances.

IT seems worth while to record the following pretty statistical paradox as a good example of the pitfalls into which persons are apt to fall, who attempt short cuts in the solution of problems of chance instead of adhering to the true and narrow road. It is true that the paradox would excite immediate suspicion in the mind of any one accustomed to such problems, but I doubt if there are many who, without recourse to paper and pen, could *distinctly* specify off-hand where the fallacy lies. It will be easy for the reader to make the experiment of his own competence to do so after reading to the end of the second of the two following paragraphs.

The question concerns the chance of three coins turning up alike, that is, all heads or else all tails. The straightforward solution is simple enough; namely, that there are 2 different and equally probable ways in which a single coin may turn up; there are 4 in which two coins may turn up, and 8 ways in which three coins may do so. Of these 8 ways, one is all-heads and another all-tails, therefore the chance of being all-alike is 2 to 8 or 1 to 4.

Against this conclusion I lately heard it urged, in perfect good faith, that as at least two of the coins must turn up alike, and as it is an even chance whether a third coin is heads or tails; therefore the chance of being all-alike is as 1 to 2, and not as 1 to 4. Where does the fallacy lie?

It lies in omitting one link in the chain of the argument as being unimportant, whereas it is vital. This omitted link is distinguished by brackets and is numbered (3) below. The reasoning then stands:—

(1) At least two of the coins must turn up alike,

(2) It is an even chance whether *a* third coin is heads or tails.

[(3) Therefore, it is an even chance whether *the* third coin is heads or tails. (Here is the error).]

The true state of the case is seen by writing out the eight several events, as in the table below.

The eight equally probable events. h = heads, t = tails.	The two letters that are alike in each case.	The third letter in each case.
h h h	*h h*	*h*
h h t	*h h*	*t*
h t h	*h h*	*t*
h t t	*t t*	*h*
t h h	*h h*	*t*
t h t	*t t*	*h*
t t h	*t t*	*h*
t t t	*t t*	*t*

No. 2 in the argument is justified by the total number of the h's in the third column being equal to that of the t's, while No. 3 is obviously not justified. In the particular 8 events with which we are concerned, an *h h* is associated with a *t three* times as often as with an *h*, and a *t t* is associated with an *h three* times as often as with a *t*. Hence as the combination *h h h* is one-third as frequent as that of any 2 *h*'s and 1 *t*, and as *t t t* is one-third as frequent as any combination of 2 *t*'s and 1 *h*, and, lastly, as the two classes of combinations are equally frequent, it follows that the frequency of the all-alike cases is to that of the remainder as 1 to 3, or to that of the total cases as 1 to 4, which is the result first arrived at.

I amused myself with testing the theoretical conclusion by making 120 throws of dice, 3 dice in each throw; the odd

numbers counted as heads, the even numbers as tails. The 120 throws were divided into 3 groups of forty in each, and gave the results of all-alike 8, 12, 8, total 28; as against not all-alike 32, 28, 32, total 92. The most probable expectation having been 30 to 90. FRANCIS GALTON.

Clerk Maxwell's Papers.

I DO not know whether the Clerk Maxwell Memorial Committee have ceased from their labours, but I cannot help thinking that more might be done towards rendering the work of Maxwell more readily accessible to students. The pair of ponderous volumes issued by the Committee are very well in their way, but they are certainly bulky, and the chronological order of papers, though eminently suited to their purpose, is not so suited to the practical needs of students.

For instance, the papers on the kinetic theory of gases seem to me far and away better than much that has been written since, and it would be very convenient to be able to procure them separately.

My suggestion is, then, that with the aid of a moderate subsidy a publisher be induced to issue Maxwell's papers on special subjects in cheap, handy, separate volumes, which might run somewhat as follows:—

On Colour and Optics.
On Graphical Statics.
On the Kinetic Theory of Gases.
On Dynamical Problems.
On Electro-dynamics.
Lectures and Addresses.
Articles and Reviews.

Under one or other of these heads almost all the papers could be included; there would be no need to include anything that did not seem likely to be of frequent use. The series of small books would be a boon to students, and a knowledge of the work of their great author would be more widely spread.

OLIVER J. LODGE.

Abnormal Eggs.

THE occurrence entitled "A Curiosity in Eggs," related in NATURE for February 1, is by no means as unusual as your correspondent imagines. It occurs in domestic poultry from over-stimulation of the system by generous feeding. The explanation of the production of one egg within another is very simple. The ovum or yolk when mature is received into the upper part of the oviduct, a tube nearly two feet in length in the domestic fowl, and in its descent is clothed successively with the layers of albumen or white, the lining membrane of the shell, and finally, on arriving at the calcifying portion of the oviduct, is enveloped in the shell. In the ordinary course of events the mature egg is then expelled, but in the case of the production of a double-yolked egg, a reverse action of the oviduct occurs. In place of being expelled, the egg is carried back again to the upper portion of the oviduct, where it meets with another mature ovum, and the two descend together, both being surrounded with a second investing series of albumen, membrane, and shell.

Some of the occurrences connected with abnormal eggs are very remarkable. I had one forwarded to me during the last month, which was alleged to contain a marble. On examination I found that the supposed marble was a small abortive yolkless egg, which in colour and form, but certainly not in weight, closely resembled a common clay toy marble. It is not unfrequent for persons to allege the occurrence of various foreign bodies in eggs. The most common substance said to be found in an egg is a horse-bean, which is closely simulated by a mass of hard coagulated blood which has escaped from the ovarium into the oviduct, and is included along with the yolk in the investing structures. I need not further allude to such circumstances as a horse-hair in an egg, or a small coin not unfrequently found at the breakfast-table, inasmuch as these are merely the result of practical joking, and require no further explanation. There is, however, one circumstance that may interest some of your physiological readers, and that is the extreme rarity of the hatching of any egg the shell of which is in the slightest degree malformed. In my own experience I have rarely, if ever, found an egg the shell of which was in the slightest degree unsymmetrical, that has been channeled at one end, or having an irregular zone around the middle, to produce a chicken. The occurrence of two ova in the same egg is by no means uncommon. It results from excessive feeding, and rarely, if ever, occurs in a state of nature. I have known two perfect birds, both chicken and pigeon, produced from such an egg, but the more general result is that the two ova, being developed together, coalesce, possibly from want of room to develop in the confined space, and thus arises the presence of two-headed, or six or eight-limbed monsters, which are much more frequent in fowls than in any other animals whatever. I have from time to time forwarded specimens of these abnormalities to the museum of the College of Surgeons, where they may be seen by those who are interested in the subject. W. B. TEGETMEIER.

North Finchley.

ON two occasions fully shelled eggs of about the size of those of the thrush have been found by myself within ordinary hen eggs, one of which is still in my possession. Several times I have hatched twin chickens from double-yoked eggs, and once a monstrosity having four legs.

Shirenewton Hall, Chepstow. E. J. LOWE.

THE PLEIADES.

AMONG the many constellations and star clusters which attracted the attention of our early ancestors, few, indeed, were so constantly observed as that small bunch of twinkling brilliants known as the "Pleiades." From a very early date, when our forefathers were not so well acquainted with the divisions of the year as we are to-day, they needed some means by which they could tell when to sow their corn, and make arrangements for other agricultural pursuits which could only be done properly in their right seasons. That they could, at any rate, get a rough approximation of such divisions of the year by means of the positions of the heavenly bodies, they soon found out, and they were thus led to observe sometimes stars, sometimes groups of stars, near the rising or setting of the sun, and even certain stars, or groups of stars, at their times of rising and setting.

That they should have chosen that group of sparkling stars, the Pleiades, to serve their purpose, does not seem at all astonishing if one considers how easily they can be recognised in the sky, and also their important position in more remote times.

The different relative positions of the sun and the Pleiades had no doubt first attracted special attention to this group of stars, and we know how important a *rôle* they played in ancient times for calendar purposes?

Let us just consider the several positions of the Pleiades as a result of the earth's rotation and revolution round the sun. Commencing about the end of May, we find that the Pleiades are altogether invisible, as they rise and set together with the sun. As time goes on, they will appear above the horizon before the sun, the difference in the time of rising of these two objects gradually increasing. In August the Pleiades cross the meridian about the time the sun rises, and by the end of November they are visible throughout the whole night, their upper culmination taking place at the same time as the lower culmination of the sun. As November draws to a conclusion, they set earlier and earlier, and by the end of February are visible only for a short time, disappearing altogether for a time after the middle of May.

Owing, however, to a slight movement of the axis of the earth, which makes a revolution round the pole of the ecliptic once in about 25,800 years, the point of intersection of the ecliptic with the equator is not fixed but movable; thus we can understand that the positions of all heavenly bodies as regards their right ascensions and declinations suffer a continual but slow alteration.

This slow movement explains the reason why the Pleiades have not always been invisible at the end of the month of May, and we have only to form a simple