however, 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