

drift, and direction of overturned edges of shaly strata, and proved that it did not flow down the valleys to the westward, but passed across them and the ranges separating them, from the north to the south. On the other side of the Irish Sea the Rev. Mr. Close and others have shown that there also the ice did not move down the valleys, but flowed along the coast southwards. The ice-scratches still preserved on the rocks prove that the configuration of the land was nearly the same then as now, and no explanation has ever been offered of this southerly movement of the ice, excepting that it was prevented from flowing down the natural slope of the land by the whole of the Irish Sea having been at the time filled with ice up to a height of about 2,000 ft. above the present sea-level. The Isle of Man, that lay in the path of this great ice-stream, is glaciated from top to bottom, and it must have been completely buried in ice. It is to the action of this great agent that I ascribe the pushing up of sand and shells over south Lancashire, Cheshire, and North Wales on one side, and Wexford and around Dublin on the other, of the Irish Sea, where the course of the ice southward was obstructed by the narrowing of the channel.

So far from the movement of this great mass of ice being deflected or warded off by local glaciers, we have seen that in north Lancashire it was not affected by them; and long ago Prof. Ramsay proved that the glaciation of Anglesea and the west of Caernarvonshire had not radiated from the high land, but that the ice had come from the north and brought with it numerous boulders from the mountains of Cumberland.

As to the possibility of ice, pushed forward by higher accumulations behind it, thrusting before it loose sand and shells up to higher levels, I may remark that there are many proofs that it possesses this power. In the Isle of Man blocks of granite have been pushed up 600 feet above the level of their source. Mr. Tiddemann has also shown that as the ice moved across the valleys down one side and up the other, it thrust over the edges of the strata. On the other side of the great English watershed, Mr. Dakyns has shown that the ice when ascending the slope of a valley opposed to its course swept before it all the drift lying on the surface, pushing it over to the other side of the range.

Mr. Bonney would be more likely to damage my theory if, instead of protesting against it, he could explain some of the many difficulties that beset that of submergence. Where was the shore of that mythical sea under which England nearly to the Thames is supposed to have been submerged? How is it that not a single undisturbed bed of glacial shells has been found, that nearly all are broken to pieces, that many fragments of *Cyprina* exhibit glacial scratchings, and that not a single instance has been recorded of the two valves of a *Jamellibranch* having been found together? Was there no friendly cliff or cavern able to preserve a single shell from the ruthless second advance of the ice? Mr. James Geikie finds the fragile bones of water-rats and frogs in his "inter-glacial beds," and uninjured land and fresh-water shells occur in abundance; but not one marine shell has been found in the uplands that does not show proof of having been transported, by being broken, worn, or scratched.

Since my first letter was sent to NATURE, Prof. Ramsay has drawn my attention to Mr. Croll's theory, that the glacial shells of Holderness had been pushed up by ice over the land out of the German Ocean. From other papers of the same geologist, I gather that he does not dispute the submergence of much of England and Scotland during part of the glacial period, and has indeed proposed a theory to account for it. So far as I know I stand alone at present in the opinion that neither during nor since the glacial epoch has there been any submergence of a great part of the British Isles beneath the waters of the ocean, nor can I expect that a theory so contrary to what has been taught for many years by English geologists will obtain a ready acceptance.

THOMAS BELT

Ealing, May 22

Uncompensated Chronometers

WITH reference to the employment of an uncompensated chronometer to indicate the mean temperature of an accompanying compensated chronometer during a long journey, with a view to the application of the proper correction for temperature, Prof. G. Forbes remarks (NATURE, vol. x. p. 50):—

"This method is quite new, and has not been tested by any nations except the Russians."

Permit me to direct attention to the following passage in the "Report on the Coast Survey," which I extract from p. 66 of

the Proceedings of the American Association for the Advancement of Science, Springfield meeting, August 1859. The "Cambridge" referred to is Cambridge, Massachusetts.

"The difference of longitude between Cambridge and Liverpool has also been determined by means of large numbers of chronometers carried repeatedly between the two stations on the Cunard steamships. These chronometric expeditions, in the words of Mr. W. C. Bond, director of the Harvard Observatory, 'for the magnitude and completeness of their equipments, have not been equalled by any of the similar undertakings of European Governments. Even the *Expedition chronometrique* of Struve was on a scale much less extensive.' The voyages were continued through a number of successive years. The first great special expedition took place in 1849, and the most recent in 1855. In the latter the effect of temperature on the rate of the chronometers formed a subject of special investigation. For each instrument the effect of temperature on its rate was ascertained by experiment, and the average temperature during each trip was kept account of by means of a thermometric chronometer, constructed like the others, but with individual balance, so that its daily rate was affected by six seconds for a change in temperature of 1° Fahr. Fifty-two chronometers were employed in this expedition, and were transported six times between Cambridge and Liverpool."

The "Greenwich Observations" for many years past (fifteen at least) contain a record of the indications of a "chronometrical thermometer" accompanying the chronometers on trial for purchase by the Admiralty; and on p. 2 of "Rates of Chronometers" in the volume of Observations for 1871 are these words:—

"The chronometrical thermometer differs from an ordinary chronometer only in the construction of the balance, the positions of the metals forming the compensating rims being reversed. By this arrangement the effect of temperature is much magnified."

J. D. EVERETT

Malone Road, Belfast, May 22

Photographic Irradiation

IN NATURE, vol. x. p. 29, the article on the coming Transit of Venus makes mention of photographic irradiation as having "been found by Lord Lindsay and Mr. A. C. Ranyard to be mainly due to the reflection of light from the back of the glass plate. It can be almost entirely avoided," Mr. Forbes goes on to say, "by wetting the back of the plate and placing black paper against it." This subject has been investigated, explained, and the above remedy suggested years ago by practical photographers. In 1867 I used the plates of the Liverpool Dry Plate Company, then sent out with the backs painted red to prevent irradiation.

But even this is not a complete preventive. Colouring the film, as suggested by Mr. Carey Lea of Philadelphia and Henry Cooper, a well-known English amateur, is a much more effectual means, though at a loss of sensitiveness; but the most complete (where the dry emulsion process is available) is to allow the collodion to be acted on by a large excess of nitrate of silver for a considerable time and then to convert this into bromide of silver by addition of ammonium bromide. The result is that the film has a dull opaque character like unglazed porcelain, and not only stops the light more completely than an ordinary collodion film, but remedies another cause of irradiation—the molecular reflection in the film itself.

Two years ago I tested plates prepared in this way on the most difficult subjects (not astronomical) and found the halation much less than by any other means except a deep red tint in the film.

W. J. STILLMAN

Hay Fever

REFERRING to the recent article in (NATURE, vol. x. p. 26) upon hay fever, I can give my own experience as having suffered from the complaint for some years past, mainly in the months of May and June. My most severe attacks have been in the house in early morning. I am, however, near hay-fields, and a tramp, by way of experiment, through one of these has more than once satisfied me of the efficacy of the hay pollen in vastly increasing the troublesome symptoms.

The treatment I have used to myself has consisted of rather strong doses of quinine taken internally, and externally a piece of linen rag dipped in strong camphorated spirit and placed upon the nose and also partly over the nostrils.