

Letters to the Editor.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Permo-Carboniferous Glaciation and the Wegener Hypothesis.

IN a recent number of NATURE (February 21, p. 255) there is a very interesting review by Prof. J. W. Gregory of an English edition of "The Origin of Continents and Oceans," in which important objections are brought forward against the supposed drift of continents and shifting of the earth's poles. The suggestion is made, however, that the theory will "probably give a new lease of life to the explanation of the Carboniferous glaciation of India and some parts of the Southern Hemisphere, by the shifting of the Pole; for arguments, which are unanswerable against that explanation with scattered continents, do not apply to Prof. Wegener's single continent."

The same view is held by Dr. du Toit, who has done excellent work in investigating the Dwyka glaciation in South Africa.

In preparing a forthcoming work on ancient glaciation, I have made a detailed study of the Permo-carboniferous ice age, and have reached conclusions which are quite at variance with this idea.

In the first place, it has been proved that ice sheets on all the continents supposed to unite about the south pole reached sea level. This is true of India, South Africa, South America and Australia, and in the latter two the ice touched the sea on both sides of the continent, as proved by fossiliferous marine deposits associated with the tillites.

The usual idea of a vast and lofty Gondwanaland on which an enormous ice sheet could arise must be given up; for the different glaciated areas were separated by oceans or at least by arms of the sea in which marine animals survived.

In the second place, an examination of maps of the supposed south polar continent prepared by Prof. Wegener (German edition) and Dr. du Toit shows an area of land far greater than Eurasia at present, with glaciation reaching a latitude of at least 45° on more than one lobe. In the case of South Africa, all the known ice motion was southward: and it is evident that the northern half of the African ice sheet has not yet been worked out, since the region is forest-covered and almost unexplored. It is altogether probable that ice reached as far to the north as it is known to have moved toward the south. This would carry the ice sheet at least ten degrees beyond the region mapped, say to latitude 35°.

It should be remarked further that, in most of the supposed Gondwanaland, as shown on the two maps mentioned, the glaciated areas would be far inland and out of reach of the moisture-laden winds necessary to deposit snow. They would be arid regions without permanent snow fields, like the interior of Asia, which was not glaciated in the Pleistocene though one of the coldest regions of the world.

It is evident, then, that the drift of continents and the shift of the poles do not help us to account for the Permo-carboniferous glaciation.

Prof. Wegener's account of the causes of Pleistocene glaciation is even less in accord with the facts. His arrangement of the shifting poles and continents gives Patagonia a mild climate while Canada was being glaciated, and allows the ice to invade South America

only at a much later time. A study of the American Pleistocene shows that two times of glaciation separated by an interglacial period occurred all the way along the Cordillera from Alaska to Tierra del Fuego, even Peru and Bolivia showing two sets of old moraines of different ages on the loftier peaks of the Andes. All geologists who have examined the Pleistocene deposits in South America as well as those of the north are agreed that they are of the same age and not separated by a million or more years as demanded by the theory of shifting poles.

It may be confidently stated that a careful study of the two greatest periods of glaciation known to geology gives no support to the theory of the drift of continents and the wandering of the poles.

A. P. COLEMAN.

University of Toronto.

I DID not mean to suggest that the lengthening of the life of the hypothesis would prove its truth, as I agree with Prof. Coleman in doubting the shift of the Pole in Carboniferous times; but the popularity of the theory will probably be increased by the removal of one line of argument against it.

J. W. GREGORY.

International Co-operation in Phenological Research.

THE response to our original appeal (NATURE, Oct. 25, 1924, p. 607) has been gratifying. Officers of co-operation range from Norway and Lithuania to Cape Colony and Australia; from Vancouver, Winnipeg, and Fargo (N. Dakota) to Lahore, Calcutta, and Batavia (Java). It has brought us into touch with many workers previously unknown to us, and ancient records, including an almost unbroken series carried on in the same family, on the same system, in the same place, from 1737 to the present day. We should like therefore to put forward for mutual discussion suggestions for future co-operation.

The plant observations made independently in different countries have been selected naturally from those best fitted for comparison with crop growth in the given country. In the same continent these lists include several common to all, allowing of a certain amount of inter-correlation. We must aim so to select the number that not only continental but intercontinental correlations will be established on a firm basis. When this is done, important equivalencies (such as Dr. A. D. Hopkins' classic example of wheat sowing and the Hessian fly) will become available over world-wide areas, where now they can only be applied locally, although he has already shown possibilities, particularly between the United States and Western Europe.

For this purpose we would suggest tentatively that an international list, including some forty subjects for observation, be drawn up, which might be expected to embrace at least some twelve to twenty fairly common in each of the associated countries, at least when situated in the temperate zones. Even ten, well distributed over the growing and fruiting months, would serve well for correlation.

For this purpose garden as well as wild flowers must be utilised, as being so widely introduced, though a number of the commonest are unsuited because of the many varieties in cultivation, and consequent variation in dates of leafing, blooming, and fruiting.

If such an international list is drawn up and each country collects and digests the results through its own Association, then in ten years or so statistics