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.*]

Atmospheric Circulation.

THE omission of references seldom leads to intelligibility, and I fear I should have been unable to trace the review mentioned by Prof. W. H. Hobbs (NATURE, Dec. 25, p. 915) had Mr. Bonacina not alluded to it in a letter to me. Prof. Hobbs asserts that my result (*Q.J.R. Met. Soc.*, 1926, 85-104) that the prevailing pressures around the poles should be low was explicitly stated by me to refer to an atmosphere circulating symmetrically over a uniform earth and unrestricted by friction. This is not the case. It is true that I worked out the solution of such a problem, but the result I obtained was that, with the highest temperatures over the equator, the pressure would increase all the way to the poles; this is diametrically opposite to Prof. Hobbs's attempted quotation. Prof. Hobbs omits to mention that I went on to examine which of the neglected factors was responsible for the chief difference between theory and observation; that I traced it to friction; and that my final inference of the existence of low pressures about the poles was the result of an argument depending essentially on friction. Incidentally, the despised frictionless theory, if adapted to cooled continents, would be in qualitative agreement with the facts as stated by Prof. Hobbs.

While this matter is under discussion reference must be made to a criticism by Mr. F. J. W. Whipple (*Q.J.R. Met. Soc.*, 1926, p. 333). I showed in my paper that if the prevailing circulation north of a given parallel of latitude is either easterly or westerly, it can be maintained against friction only by interchange of air with lower latitudes. Thinking that I had dynamical grounds for believing that this interchange would, in the conditions of the northern hemisphere, involve mainly south-west and north-east winds, I inferred that the polar circulations must be westerly. Mr. Whipple points out a weakness in the argument, and has led me to modify this conclusion. It seems clear that if there were no horizontal interchange of air, the effect of surface friction would be simply to make the air drift across the isobars until all differences of pressure at sea-level were annulled, and there would be no surface winds anywhere. Somehow this condition is forestalled by the development of irregular winds of cyclonic type, which maintain a continual interchange of air across the mean annual isobars, and the primary effect of such interchange, with an actual distribution of temperature, would probably be a transmission of angular momentum polewards, giving an equatorial belt of easterly winds with broad belts of westerly winds north and south of it. The winds at the southern boundary of the belt of prevailing westerlies in the northern hemisphere must be mainly north-east and south-west; but there seems to be no reason why they should persist all the way to the pole, and if the currents are deflected within this belt so as to become mainly north-west and south-east at its northern margin, they will be capable of maintaining an easterly circulation north of it.

Accordingly I see no great objection to anticyclonic circulations in Arctic and Antarctic regions. The main results that emerge from the discussion are that friction plays a dominating part in atmospheric circulation, and that cyclones are essential to the maintenance of any general circulation and are not disturbances superposed on it. Presumably the distribution of the belts of easterly and westerly winds would be affected by a change in the distribution of temperature in latitude.

Mr. Bonacina's quotation in his review (*Geog. Jour.*, Sept. 1926) expressed correctly the views I held at the time he wrote, but as a result of Mr. Whipple's note I have somewhat modified them in the sense indicated above. The question of the existence of glacial anticyclones strikes me as one to be settled by observation. I would only point out that the great majority of the observations quoted on the point are irrelevant. They refer to stations near the coast, where permanent outflowing antitriptic winds are to be expected whatever the winds in the interior of the continent may be. To infer an anticyclonic circulation over a glaciated continent on the basis of winds from the land at coastal stations is like finding out whether it is raining by turning the tap on. The observations in the interior of Greenland and Antarctica by Koch, de Quervain, and Scott, quoted by Prof. Hobbs in his book, are to the point, but none of the others are.

HAROLD JEFFREYS.

The Significance of Phosphorus in Muscular Contraction.

AN examination of the very extensive literature dealing with the function of phosphorus compounds in the chemical mechanism of muscular contraction reveals so many contradictory statements that it is evident that the technique in use must be subject to some serious fault. Since we have found what is probably the main cause of the discrepant results obtained in this field, it seems desirable to communicate our results without delay.

There appears to be in muscle tissue an organic phosphorus compound which, by reason of its great instability in acid solution, has been confused hitherto with inorganic phosphate, to which it gives rise in the course of the estimation of inorganic phosphates by the methods of Embden or of Briggs, or by any method involving the use of mineral acid. The confusion is increased by the fact that this substance, the organic phosphorus compound which we have designated 'phosphagen,' is intimately connected with the chemical mechanism of contraction; the estimation, therefore, of 'inorganic' phosphate by the above methods is hopelessly misleading, since by them one measures the sum total of two substances which vary independently in amount. It is possible, by avoiding the use of acid solutions, to estimate true inorganic phosphate, since 'phosphagen' appears to be stable in neutral or slightly alkaline solution. The following table, which concerns the gastrocnemius muscle of the frog, illustrates the changes in the amount of phosphate and 'phosphagen' in a muscle subjected to different treatments:

	Resting.	Rapidly Fatigued.	Heat Rigor.	Incubation in $NaHCO_3$.	
				Without NaF.	With NaF.
Inorganic phosphate . .	20	50	90	110	20
'Phosphagen'	65	25	0	0	0
Sum total .	85	75	90	110	20

The figures are given as milligrams of phosphorus per 100 gm. of muscle, and are representative of a number of experiments. The third row of figures

© 1927 Nature Publishing Group