sample of seed which had been sent from Senaar Province. This sample was sown at Ghizeh, and the material sent to Kew was taken from two adjacent plants in the same row, these being plants developed from embryos which had ripened on a tree in Senaar. The reason for sending material from two different plants was because this row showed two distinct leaf-forms, some of the plants having much wider leaf-lobes than the others, and these two types were represented by 213–1 and 213–2. In no other respect could any distinction be drawn between the two types, at least on any character within my grasp; moreover, they all flowered within the same week, ripened within the same week, grew to a remarkably uniform height, and had similar habits of growth; with the one exception of the leaf-shape they were far more alike than a similar group of plants taken from a field of any variety of Egyptian cotton. These two forms were separated by Watt into *G. arboreum* and *G. Nanking*, because "a yellow-flowered *G. arboreum* with deeply laciniate bracteoles and three glands on the under surface of the leaf would destroy the specific isolations" (p. 138). I incline to think that the laciniation of the bracteoles and the glandulation of the leaves should have been made the subject of comparative study—in order to ascertain their capacity for fluctuation—before such erratic characters were entrusted with the responsibility for this violent separation of the two forms into two separate species. Such comparative study would at least have been commenced had these plants been seen growing side by side in my plot.

On the other hand, we find on p. 181 that Moqui Indian cotton from Arizona (209–3) and "Hindi" weed cotton of Egypt (55 A) are placed together under *G. punctatum*. Waiving the query as to why Hindi, a naked-seeded cotton, should be placed in the fuzzy-seeded section, I should like on other grounds—but in all diffidence—to advance the opinion that if the two strains could be grown together at Kew, or examined side by side on my plot in Egypt, they would be systematically removed from one another by a wide interval.

The employment of common names has also been mentioned by Colonel Prain; the following instance, therefore, does not seem altogether pointless:—the plant referred to as 56.C.2 (p. 224) came from a sample of Afifi cotton, and bears lint of the brown Afifi colour; this colour is the characteristic and sole morphological distinction of Afifi from Abbassi, the latter bearing white lint, so that 56.C.2 could by no possibility be described legitimately as "close to Abbassi or Afifi."

The cultivated varieties of Egyptian cottons—and probably of Uplands—consist of many different strains mingled together and cross-fertilised, resembling one another in a few obvious characters of economic importance. Thus, on p. 224, Watt describes the strain 142, plant A, as being distinct from the Abbassi plant described in par. 2 of the same page. In point of fact, No. 142 was taken from a prize sample of Abbassi.

Though I wish to see an exact method adopted for the investigation of this labyrinthine genus, such method to be based on pedigree culture and statistical inquiry, I am nevertheless grateful to Sir George Watt for having gathered together the mass of detailed information which is to be found in his book, and I hope—with Colonel Prain—that we shall not have to wait long for the publication of further researches on the subject.

W. LAWRENCE BALLS.

Cairo, February 27.

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IN the courteous letter in which Mr. Balls exercises his right to criticise details in Sir G. Watt's work on cotton, as to which he considers himself a competent judge, he gives expression to some misapprehension that it may be well to remove.

It has not been affirmed that the ideas of the writer of the review which appeared in NATURE for January 16 as to "species" and "varieties" do not accord with accepted usage. What it was necessary to point out was that the reviewer had not made it clear that his interpretation of these words accords with accepted usage. There are two passages in the review in which the words are dealt with together; in one passage they are so used as to imply that the status of a variety is the same as that of a species; in the other they are so used as to indicate that a species is subordinate in status to a variety. The ideas of the reviewer may be as precise as those of Mr. Balls; they may, on the other hand, be as loose as his own phraseology; he has given us no means of deciding.

The position assumed by me has already been explicitly stated. I have reserved perfect freedom of judgment as regards the acceptance of Watt's conclusions, not as to the limits of species in the genus Gossypium alone, but as to all the issues involved. When he explains that his general position is the reverse of this, it will be felt that Mr. Balls does himself an injustice.

The name of the distinguished public servant referred to by Mr. Balls is Mr. A. F. Broun, and is not as given in Mr. Balls' letter. D. PRAIN.

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### The Isothermal Layer of the Atmosphere.

IN his letter in NATURE of February 27 Mr. Dines asks why the adiabatic conditions which prevail in the lower part of our atmosphere should suddenly cease at a height of about 40,000 feet. The answer comes more readily if the question is altered to, Why does the isothermal condition of the outer layers of our atmosphere suddenly cease at about 40,000 feet? The isothermal condition or even increased temperature with height is the condition which would naturally prevail in an atmosphere surrounding a smooth sphere. For if the sphere is a very hot one its entire gaseous envelope should acquire its temperature, whereas if the solid sphere, like our earth, is cold, and if heat from the sun is warming the atmosphere by radiation, one may expect the outer layers to be warm and the lower layers to be the coldest ones. If, however, there are irregularities, as, for instance, mountain chains on the earth's surface, then the air, whenever it is forced over them, parts with its moisture as it rises on the one side and then descends on the other side as a dry and hot *Foehn*, in which wind the conditions are perfectly adiabatic, the temperature gradient rising steadily with decreasing height. It seems, therefore, that it is our mountain ranges which prevent the isothermal condition from descending below the height at which effective mixing or moisture removing occurs.

This leads to the conclusion that if at one time our mountain ranges were lower than at present, the isothermal condition and its low temperature will also have been lower than at present. This may have been the case during Glacial periods. On the other hand, during tropical periods our mountain ranges may have been higher than they are at present; the isothermal condition will have ended at a higher level, and the steady rise of temperature below this boundary will have resulted in a very high temperature on the earth's surface.

I remember discussing this subject about twenty years ago at Aix-la-Chapelle with Dr. A. Ritter, who had only recently in Wiedemann's *Annalen* (vols. v.–viii., "Heights of Atmospheres and Conditions of Nebulæ") dealt with it very exhaustively. If I am not mistaken, it was the *Foehn* wind which had first led to these inquiries, but, strange to say, Dr. Ritter relied on molecular motions for the necessary mixing of the layers. This may have been due to his feeling that if isothermal conditions were conceded, an interstellar atmosphere would have to be postulated. We therefore almost naturally disagreed as to the possibility of condensing the so-called permanent gases, which fact had not then been accomplished. My view was that if nitrogen and oxygen should be condensable, and if the adiabatic condition existed up to the outer limits of our atmosphere, then, at the zero temperature to be found there, both gases would condense and sink to the lower levels, to be followed by further and further layers until the whole atmosphere would be deposited on the earth's surface. Dr. Ritter merely pushed this difficulty further away by saying that, even if oxygen and nitrogen could be condensed, our atmosphere might nevertheless be surrounded by hydrogen. Now that hydrogen has been condensed, helium would have to take its place, or, and this is a view not easily accepted, our earth may be surrounded by a very attenuated and possibly warm interstellar atmosphere. I think that the recent experiments