LETTERS TO THE EDITOR.

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Racial Problem in Nubia.

It has been objected that the sketch of the history of Nubia given in the article on "The Unveiling of Nubia" in NATURE, April 27, p. 283, does not agree in every particular with the accounts given by Mr. Firth in the sixth Bulletin of the Archaeological Survey of Nubia, p. 8, and by me at the Sheffield meeting of the British Association and in several lectures during the past year.

It must be remembered, however, that the notice in NATURE was a review of the published account of the results obtained by Dr. Reisner during the first season's work of the Archaeological Survey, and it did not come within the scope of the reviewer's task to describe the attempts that have been made during the last three years to throw further light upon the significance of the facts set forth in Dr. Reisner's report.

It may prevent such misunderstanding, however, if a brief statement is made of the bearing of recent investigations upon the meaning of the facts brought to light in Nubia. It is not without significance that the archaeologists, studying in Nubia the handiwork of the ancient people, and the anthropologists, as the result of the examination in England of the remains of the Nubians that have been sent over here, have arrived quite independently and without collusion at the same interpretation of the significance of the story the pottery and the bones respectively have to tell.

Mr. Cecil Firth's statement of the views of the archaeologists will be found in the sixth Bulletin (*op. cit.*).

The working hypothesis that I set up last summer, when all the facts derived from the study of the human remains were being collated for the first time, is not only borne out by the archaeological evidence, but has been found to be in full accordance with all the facts which further detailed study of these ancient Nubian remains has brought to light.

It is now quite clear that in pre-dynastic times there were scattered throughout the Nile Valley, not only in the territory we call Egypt, but also much farther south, many groups of people linked to the pre-dynastic Egyptians by the closest bonds of affinity, and also sharing with them a common cultural inheritance. Until the beginning of the period of the Pyramid-builders no difference can be detected either in the physical characters of the people or their achievements on the northern (Upper Egypt) or the southern (Lower Nubia) side of the First Cataract. But as the marshy territory of Egypt was drained, and the extent of its rich habitable land was thus increased ten-fold, there was a movement of population from the barren country above the First Cataract into the more fertile north.

But when the proto-dynastic Nubians emigrated into Egypt their place in Nubia was taken by the next member of the group of peoples that were scattered serially throughout the Nile Valley like beads on a string. As the result of the first season's work in Nubia, the only obvious explanation of the state of affairs revealed in these Nubian graves of the time of the Ancient Empire—the B-group of the archaeologists—was found in the hypothesis that the original population of Nubia became tainted with negro blood and fell away from the high standard of culture and technical skill attained by their forefathers. The facts then available did not justify any other explanation. But in the light of the fuller knowledge now in our possession, it is evident that the B-group people were not the direct descendants of the A-group or pre-dynastic Egyptian population of Nubia, but the next bead on the string; in other words, they were members of the southern community of kindred people, next in order in the Nile Valley south of Nubia; and there is no reason to suppose that they had lost any cunning possessed by their ancestors, but rather that they had not kept pace with their northern brethren in the advance of the latter in the paths of civilisation. The evidence for this view is

abundant and manifold in kind. I need mention only one fact here—the almost complete absence, among the human remains of the B-group people, of pure negroes, while the whole population is very definitely negroid than the Egyptians, can only be explained on the hypothesis that the process of admixture took place farther south.

After the time of the Ancient Empire, the next bead on the string—the C-group of the archaeologists—was moved north into Nubia. These Middle Nubians, as we call them, were also obviously akin to the pre-dynastic Egyptians, and their burial customs and pottery were clearly derived from the same source as those of the Egyptians: but it is equally certain that the two populations, the Egyptians and the Middle Nubians, had developed along divergent lines. Moreover, the undoubted specialisation of the physical characters of the people, no less than of their arts and customs, was emphasised by the introduction of an exotic African element into the C-group people. They became more definitely negroid than either the A- or the B-group peoples, and their pottery exhibits, no less clearly than their bones, the influence of the negro.

Mr. Firth summarises the archaeological statement in these words:—"The theory tentatively advanced in the second annual report that the C-group people represent a later wave (greatly modified by Negro influences) of the same race which founded the Pre-dynastic culture of Upper Egypt, is based on certain affinities in burial-custom and pottery-making, and requires the confirmation which a careful examination of the physical character of the human remains can alone give." The human remains have supplied this confirmation, and they did so before we were aware of the fact that Mr. Firth was asking for the support of the evidence they afford.

His further statement that "the connection between the B- and C-groups does not seem to be very close and a comparison of the two would suggest an independent origin of the C-group" may seem to suggest that there was a much wider hiatus separating the Middle Nubians (C-group) from the earlier inhabitants of Nubia than there was to divide the two groups (A and B) of the latter the one from the other. There can be no doubt there was a much greater contrast between the C-group culture than separated those of its forerunners in Nubia: but it is equally certain that the B-group people, interposed both in time and locality between the A-group (distinctively Egyptian) and the C-group (distinctively Nubian), were much more strongly influenced culturally by the higher civilisation of the former than by that of the latter. Thus the Archaic Nubian (B-group) culture has the appearance of being the direct offspring of the Archaic Egyptian (A-group), but the people themselves form a unit as distinct from its forerunner (A-group) as it is from its successor (C-group).

There is a considerable mass of evidence to suggest that, just as the B- and C-groups represent successive waves, respectively, *circa* 3000 B.C. and 2000 B.C., which moved northward in the Nile Valley, the early pre-dynastic people in Egypt were largely reinforced, perhaps about 4000 B.C., by a precisely similar wave or rather concentration of the scattered primitive Nilotic people in the most desirable part of the Nile Valley.

In these notes I have attempted to suggest the present trend of our investigations without doing more than merely hinting at one out of a multitude of varied kinds of evidence indicative of the northerly trend of the Hamites in the Nile Valley, leading to a concentration in Egypt.

G. ELLIOT SMITH.

Manchester, April 29.

Inheritance of Row-numbers in Maize Ears.

It is well known among maize-growers that the number of rows of grain on an ear of maize varies from 8 to 24, or even more, according to the breed; also that in the same breed the number may vary within certain limits, e.g. 8, 10, or 12 in some breeds, 12, 14, 16, or 18 in others and 18, 20, 22, or 24 in yet others. In some breeds the range of variation is even greater than I have indicated, while in others it seems to be more closely limited. In some breeds an ear carrying more than 8 rows is considered untrue to type, but I am not aware that any

South African grower has yet succeeded in fixing the number of rows in any breed to such a degree that *no variation* occurs in that respect.

In the course of a series of breeding experiments I am conducting, which are not yet completed, I have met with the following interesting case.

Thirty-three plants of "Arcadia" sugar-maize, each of which bore two well-developed ears, were studied as regards number of rows. On 21 plants the number on the upper ear was different from that on the lower, while on 12 plants the number was the same on each ear. Of the 21 plants on which the number of rows differed on the two ears, 13 had a larger number on the lower than on the upper, while 8 had a smaller number on the lower than on the upper. The distribution of rows was as follows:—

Class	Upper ear	Lower ear	Number of plants	Number of plants in each class
As many rows in lower as in upper	12 ... 12	... 8	...	—
	10 ... 10	... 3	...	—
	8 ... 8	... 1	...	12
More rows in lower than in upper	10 ... 14	... 1	...	—
	10 ... 12	... 6	...	—
	8 ... 12	... 3	...	—
	8 ... 10	... 3	...	13
Fewer ears in lower than in upper	12 ... 14	... 1	...	—
	12 ... 18	... 4	...	—
	10 ... 8	... 3	...	8
				33
Summary.				
8	8 ... 1	...	...	—
	10 ... 3	...	...	—
	12 ... 3	...	...	7
10	8 ... 3	...	...	—
	10 ... 3	...	...	—
	12 ... 0	...	...	—
	14 ... 1	...	...	13
12	8 ... 0	...	...	—
	10 ... 4	...	...	—
	12 ... 8	...	...	—
	14 ... 1	...	...	13
				33

The total number of ears producing any given number of rows was as follows:—

Rows ...	8	10	12	14
Ears ...	11	23	30	2
Total 66				

The "Arcadia" is a white sugar-maize obtained from a cross between a normally 8-rowed "Black Mexican" and a white flour-corn normally bearing a larger number of rows, but I do not know that either was pure bred, for row numbers and no subsequent selection in this line had been made.

It is generally supposed by maize-growers, in this country at any rate, that the number of rows is a definite, heritable character. Results obtained by crossing two other breeds, an 8-row and an 18-row (each believed to be pure as regards this character), have this year produced irregular results in the F_1 generation, for which I have not yet been able to account. However, the case described above seems to indicate that the development of rows is, within certain limits, a vegetative character depending in part on seasonal conditions and on food supply. This view is strengthened by the fact that this is the first year in which I have noticed 14-row ears in this breed, all the parent ears for two or three generations having been 8-, 10-, or 12-rowed (so far as I am aware). At the same time, there is ample indication that, within certain limits, row-numbers are inherited in the maize plant, but it is doubtful whether any South African strains are yet sufficiently pure-bred for this character to demonstrate the point with absolute certainty.

JOSEPH BURTT-DAVY.
(Government Botanist.)

Department of Agriculture, Pretoria, April 17.

NO. 2167, VOL. 86]

Absorption Markings in "K" Spectroheliograms.

IN a letter published in NATURE of March 30, Mr. Buss suggests that the evidence derived from some spectroheliograms taken by M. Deslandres at Meudon conflicts with that which I obtained from the Kodaikánal daily series.

There is no such "divergence of evidence" in reality. The dark marking shown on our plates of March 21, 1910, and described as vague and ill-defined, is doubtless much more clearly seen in the plates obtained with the Meudon high-dispersion spectroheliograph, which isolates the central absorption line K_3 .

With the dispersion available in the Kodaikánal instrument, the K_3 line is about half the width of the camera slit, and photographs taken with the slit exactly central on K integrate the light of the absorption line and of the side components of the emission line K_2 . As the dark flocculi or absorption markings seem to be entirely due to variations in intensity in the narrow absorption line, it is rather a matter for surprise that in our photographs they should be so clearly defined in many cases. In the original negatives taken on March 21, 1910, in addition to the broad, ill-defined shading already mentioned, there are clearly seen all the curious linear markings so beautifully shown in M. Deslandres' K_3 plate of this date, and I can find no appreciable differences in the contours of the markings.

With regard to the disappearance of the enormously extended marking between March 25 and 26, had Mr. Buss read the paragraph in my article referring to this with ordinary attention, he would not have suggested that the absence of the marking on the plate of March 26 was due to imperfect adjustment of the spectroheliograph slits. Very possibly the disappearance shown by our plates was not absolute, and K_3 or $H\alpha$ plates taken on the same day would have shown the marking, but if so the reduction in intensity compared with the previous day would have been marked.

The theory apparently advocated by Mr. Buss, that the absorption producing these markings takes place above the prominences, receives no support from our visual or photographic observations, and his remarkable observation of a dark flat cloud hovering over the bright prominence at each successive appearance east or west seems to be unique! No trace of so extraordinary a feature can be seen on any of our numerous photographs of this prominence.

J. EVERSLED.

Kodaikánal Observatory, April 18.

Calendar Reform.

MAY I trouble you with one or two observations on the excellent article which appeared in NATURE of April 27.

Referring to the application of the principle of the *dies non*, or the setting aside of a day annually not included in the weekly enumeration, the author of the article says "the week can boast a most ancient lineage uninterrupted by the slightest break." Is this certain? I find Dr. Hale in his "Chronology," vol. i., p. 67, says:—"If the year of the Crucifixion was A.D. 31, as is most likely, it follows from an eclipse of the moon in Pingre's tables, April 25, at 9 afternoon, that the Paschal full moon that year fell on March 27, which in the calculations of Newton, Ferguson and Lamy, and the computation of Bacon is reckoned Tuesday," &c. I might adduce other reasons for doubting if the continuity of weeks has been uninterrupted. It must be remembered that for some time, at any rate, throughout the Roman Empire the odd day in leap year was treated as a literal—not merely as a legal—*dies non*, being regarded as part and parcel of the day preceding.

Nevertheless, I agree with the author that prejudice in this matter cannot be disregarded.

But no such objections can be stated to the proposal to apply this principle to the *months*, i.e. to treat the 365th and 366th days as without the monthly enumeration, and to equalise so far as possible the lengths of the months so as to give four quarters of 91 days, or 13 exact weeks.

It is hopeless to suggest that the present arrangement of months has any scientific or religious sanction or advantage.

I hope shortly to present to the public more fully the arguments in favour of this really non-contentious part of