LETTERS TO THE EDITOR.

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The Proposed Tropical University.

THE proposal to create a tropical university which has been put forward in the columns of NATURE and elsewhere is one which requires careful scrutiny and calls for a clear appreciation of the real issues involved.

It seems at least open to question whether the advocates of the scheme really contemplate a new university, or whether they are not rather thinking of a college or institute of university rank, the work of which should be somewhat intimately associated with the promotion of the material prosperity of the great agricultural interests that are growing up in the tropics. Such a college, in addition to the function of inducting men into the various branches of tropical agriculture, should serve, if properly staffed and organised, as a centre for the dissemination of current information on matters pertaining to the industrial needs of the community, in so far as agricultural problems are concerned. For this purpose it is essential that facilities for field and other experiments should be fully provided, and if the site were suitably chosen the college would prove an invaluable training ground, not only for the population resident within its immediate geographical area, but for others also, and especially perhaps for Europeans, about to engage in agriculture in any part of the tropics. Various places have been suggested as possible sites, and there is much to be said in favour of the West Indian proposal. Easy access from Europe, as well as the variety of soil, climate, vegetable products, &c., are all points in its favour, whilst the fact that no British institution of the kind desired exists in that region is a defect which would thus be repaired. Furthermore, the possibility of securing a considerable range of advantages within a relatively small geographical area is of itself a distinct gain, for it could be more economically worked than a similar institute in a large continental area, where things are on a larger scale, quite apart from limitations imposed by a continental climate, which cannot be ignored.

Accepting for the moment the desirability of founding a college of the kind indicated, the danger that lurks in the scheme would almost certainly be found, in practice, to consist in a desire to see immediate results which would be convertible into a cash value by the planters. In order to ensure success, it is absolutely essential that a wise and far-sighted policy should guide the destiny of the institute. Agricultural problems, and especially tropical problems, are seldom simple, and while immediate practical objects need not, and should not, be lost sight of, the college would fail to justify its creation if it were to exist for these purposes alone. It must, while not neglecting the practical training of students in tropical agriculture, also include within itself, as a vitally essential part, a body of first-rate scientific investigators and teachers, who will be able to seize upon problems and work them out. There must be no attempt to limit their work to the economic questions of the moment, for in cramping the spirit of investigation lies the way of throttling material progress.

Such a staff would, of course, cost money, and often the return might seem to be slow in coming, but it is impossible to over-estimate its importance. Indeed, unless a proper staff can be provided, the

scheme is not worth pursuing, for the most that could then be hoped for would be a mere technical institute—a sort of edition in *parvo* of current planting practice, veneered over by a fallacious appearance of scientific equipment.

Now an institute such as is here foreshadowed would greatly gain by connection with leading institutions in this country. Science is growing apace, and particularly those branches of it which especially touch on agriculture. And, however able the staff, it could not hope to escape from the disadvantages inherent in a separation from the main clearing-houses of scientific thought. Some sort of association, then, with home institutions, such as the University of Cambridge and the Imperial College of Science and Technology, for example, could not fail to be of advantage to all concerned. An association of this kind ought to be a real and not a merely nominal one, for only in this sense could it serve any useful purpose and provide for an interchange of knowledge and for the stimulation of ideas. Arrangements might perhaps be made for enabling suitable students of the college to visit this country and be received for a time in the home institutions, and *vice versa*.

Such a college, conceived and maintained on generous lines, would develop into a valuable asset to the Empire, and would exert a powerful influence in furthering the interests and objects of tropical agriculture within it.

But the question of an insular tropical university is a very different one. There are perhaps already more than enough universities, and it is difficult to see how it would be possible to justify the foundation of another in the manner that has been suggested,—quite apart from the very considerable outlay that would be necessarily involved.

Moreover, a centre of real university learning, and still less of culture, can scarcely be created by the stroke of an administrative pen, and it would, in any event, be compelled to seek its justification in the existence of a population large enough and able in other respects to utilise the advantages the university ought to be in a position to confer. And it seems open to inquire whether a mere fraction of the financial resources which would be needed for the more ambitious project might not amply suffice to enable everyone of marked ability to enter an existing university elsewhere, if he (or she) were otherwise unable to do so. There would be many disadvantages inseparable from a small insular university, and it is scarcely necessary to dwell on them here. For the present it may suffice to remark that a second-rate university is not worth its upkeep, whilst a properly staffed and equipped one would demand very considerable funds, and not only so, but other claims, difficult to meet, would also have to be satisfied.

The further one reflects on the matter the weaker does the case for the establishment of the university, and the stronger the claims for the foundation of an agricultural college, appear. Almost all the arguments which can be urged against the first proposition can be used in support of the latter. But it may be that after all a substantial agreement already prevails amongst the majority of those who are advocating the scheme, and that an agricultural college of university rank is really what is desired.

J. B. F.

The Mountains and their Roots.

IN NATURE of February 27, p. 703, you honoured me with a review of my recent paper on the origin of the Himalaya Mountains. During thirty years of residence in these mountains I have continually been

confronted by the geodetic problems which they present. The highest geological authorities express doubts as to how mountains have been upraised, and geological theorists differ widely.

In my recent paper I suggested, with diffidence, the hypothesis that the long belt of Indus-Ganges alluvial plains was concealing a deep crack in the subcrustal shell of the solid earth, and that the Himalaya mountains had been crumpled up by the opening of this crack in the solid globe.

When a large mass of heated rock, or ore, or glass is cooling, its surface is apt to crack; we never see the core of any such mass shrink away from the outer shell and leave the outer shell too large and unsupported, as is often assumed to be happening in the case of the earth.

Are geologists quite sure that the earth's outer shell has not been cracking, and that the cracks are not hidden from our sight by silt? Would not the cracking of a solid globe provide a sufficient mechanical force to elevate mountains?

The earth's rotation is slowing down; the equatorial protuberance of rock is constantly straining to move polewards.

Throughout the whole length of the Indus-Ganges belt of plains earthquakes are frequently occurring, and what can be causing these earthquakes, if not the splitting asunder of the solid globe beneath?

At both extremities of the Indus-Ganges belt deep narrow submarine cañons exist extending far out to sea; they are known to sailors as "swatches." What are these swatches, if not the surface indications of a subcrustal crack?

In the review in *NATURE* my hypothesis is dismissed without any specific objection to it being raised. I am not wedded to it, and I should welcome its dismissal, if I thereby learnt its errors. But I am disappointed to see it rejected merely because my critic has himself accepted the "floating crust" hypothesis.

The reviewer has accepted as true the hypothesis of the Rev. Osmond Fisher. According to this hypothesis the solid crust of the earth is of limited thickness and floats upon a liquid magma of greater density. This assumption of a liquid substratum appears to me to be opposed to the views of the great majority of geological writers. So far as I am able to judge, the weight of the evidence seems largely in favour of a solid globe.

Furthermore, Mr. Fisher has to assume that as the central core of the earth cools down, the outer crust is left unsupported owing to the core's contraction ("Geology of India," R. D. Oldham, p. 471). This assumption of a cooling core contracting away from its shell seems to me to be more difficult to justify than the assumption of a cooling shell becoming too small for its core.

But let me descend from these great assumptions to actual geodetic figures.

Mr. Fisher assumes that the crust floats in the dense liquid, just as ice floats in water. Each mountain rising from the upper surface of the crust has a corresponding protuberance extending downwards from the lower side of the crust. The buoyancy of a protuberance suffices to support the weight of the corresponding mountain above it.

If h = height of a mountain, and if d = depth to which its protuberant root extends downwards into the liquid, then, according to Fisher, $d = 9.6 \times h$.

Now let me apply this hypothesis to the mountains of India. All these mountains are assumed to be buoyed by subcrustal protuberances of lower density than the magma, but the protuberances extend downwards to different depths, which are proportional to the several mountain heights.

Mountain	Height of mountain above crust = h Miles	Depth of protuberance below crust = d . Miles
Tibet plateau ...	3 ...	29
Himalayan range ...	4 ...	38
Vindhya ...	0.6 ...	5

Let us suppose a plumb-line to be suspended near the foot of a mountain, and let us suppose that the mass of this mountain is compensated by a deficiency of density underlying it below the crust. If that deficiency of density be wholly concentrated near sea-level, it will entirely compensate the attraction of the mountain mass, and the plumb-line will hang vertically. But if that deficiency of density be distributed to a great vertical depth, it will not compensate the mountain's attraction, first, because of its greater distance in depth from the plumb-line, and secondly, because its resultant action is more inclined to the horizontal.

If a mountain (Himalayan) is four miles high, and if its protuberance extends downwards to a depth of thirty-eight miles, the geodetic observer would report:—"There is distinct evidence of compensation, but the compensation is by no means complete."

If, however, a mountain (Vindhya) is 0.6 mile high, its compensation would take place within five miles of the crust, and the observer would report:—"The compensation is here more complete than in the case of the higher mountain."

Similarly a pendulum observer at a station (Himalayan) two miles high will (according to the Fisher hypothesis) not find the same degree of compensation as he will at a station (Vindhya) half a mile high. The underlying deficiency of density will in each case have a retarding effect on the pendulum, but at the Himalayan station the deficiency reaches downwards nineteen miles into the liquid, whilst at the Vindhya station the deficiency only extends five miles downwards.

According, then, to the "floating-crust" hypothesis our plumb-line and pendulum observers should find the attraction of small mountains more completely compensated than the attraction of high mountains. But what are the results of actual observations? Both the pendulum and plumb-line observers find the attraction of the Himalayas to be largely compensated, whilst the Vindhya mountains are not compensated at all. Actual results of observation are in direct opposition to the "floating-crust" hypothesis.

Mr. Hayford has stated that the "floating-crust" hypothesis is not true for the United States of America ("Figure of the Earth and Isostasy," p. 164), and in my opinion the evidence is sufficient to show that this hypothesis is not true for India.

S. G. BURRARD.

Surveyor-General's Office, Dehra Dun, March 29.

As an officer of the Survey of India, employed for many years in determining deflections of the plumb-line and variations in the intensity of gravity, I was interested to find in the article which appeared under this heading in *NATURE* (No. 2261, vol. xc., February (27) reference to the hypothesis suggested in 1904 by the Rev. O. Fisher as to the nature of mountain compensation, and the statement that this hypothesis goes far to explain the deflections of the plumb-line observed at the foot of the Himalayas and in the Gangetic plain. The article states that, according to Mr. Fisher's hypothesis, "the crust is of uniform density, the isostatic compensation being obtained by a variation in thickness," and that, on this hypothesis, Mr. Fisher "finds that the attraction of the visible range combined with the negative attraction of the