No doubt the Royal Agricultural Society was not founded for the advancement of science in general or of botany in particular. When, however, it transcends the practical limits it has imposed upon itself, and promotes a purely scientific investigation, the way it sets about it is, I suppose, a fair object of criticism in a scientific journal.

Mr. Jenkins complains that I have not taken the trouble to read the official reports published by the Society, and thinks my criticisms upon them might have had some value. As a matter of fact I have done so, and my difficulty is to be quite sure that I understand what the last and most important really means. To say nothing of the occurrence of "jonidia" for "conidia," I find the following sentence:—"Prof. de Bary expresses sanguine hopes that he has at last discovered the certain nids (*sic*) or resting-places of the oospores or active primary germs of the fungus." It would not have occurred to me to describe oospores—in other words, resting-spores—as *active*, and it has been suggested to me as not impossible that oospore may also be a misprint in place of zoospore. There is the more necessity for caution in the matter, as the publications of the Royal Agricultural Society do not seem to receive the botanical revision that might have been expected. Only last year—and it was not a solitary blunder—a fungus was figured in the Society's Journal as Aspergillum (*sic*), which was obviously no Aspergillus at all, but the common Bread-mould (*Ascophora Mucedo*). No doubt, in due course, we shall have the opportunity of reading, at full length, what Prof. de Bary has added to our knowledge of the matter; but in the meantime we should not forget what is due to those who have already worked at the subject in this country.

Mr. Jenkins denies that the Society offered prizes for disease-proof potatoes. I find that in the report of the judges on the abortive essay competition, presented to the Council, Dec. 10, 1873, it was recommended, "That valuable prizes be offered for (*a*) The best disease-proof early potato; (*b*) The best disease-proof late potato." Again, the recently published official report to which I have already referred commences: "The judges appointed to inspect the growth of the six varieties of potatoes, *which were entered for competition as disease-proof*," &c. It may be that this is "colloquial" language, and does not mean what it says; but Mr. Jenkins must know that if by any chance any one of the potatoes tried had run the gauntlet of the three years' trial, it would have been advertised far and wide as stamped with a "disease-proof" character by the Royal Agricultural Society.

Mr. Jenkins complains that I suggest an offensive spirit as actuating the Society in its communications with Prof. de Bary. I can only say that I used the Society's own language. I find that the judges, in their report, *after declining* to recommend any one of the ninety-four essayists for a prize, propose "That a sum of money (say 100*l.*) be granted *for the purpose of inducing* a competent mycologist to undertake the investigation of the life-history of the potato-fungus" (as if nothing had been done in it already). The joint Botanical and Journal Committee thereupon gave notice that they would ask for a grant of 100*l.* to carry out this recommendation. I am not aware that the British Association proceeds in this way in distributing its funds, and I leave Mr. Jenkins to reconcile what I have quoted with his statement, "that the *first* step taken by the Council of the Society was to direct me to write to Prof. de Bary."

Let me sum up the substance of my criticisms. The potato disease has been before the scientific world for thirty years, and has been investigated by Berkeley in England, Montagne and others in France, De Bary in Germany. The Royal Agricultural Society takes charge of a competition which induces ninety-four persons to write on a subject on which it was *à priori* in the last degree improbable that they could have any really important unpublished facts to bring forward within the limits of even the extended time at which the essays were to be sent in. On the failure of this scheme prizes are offered for disease-proof potatoes, "disease-proof" being subsequently defined to mean immunity from disease in twenty different districts for three years. Were a disease-proof potato a probable thing, it might clearly be trusted to establish its own reputation. Lastly, the amateur world of prize essayists having proved fruitless, the cryptogamic botanists of this country—many of them men of European fame, who would doubtless have willingly responded to an appeal from the Council to co-operate in the matter—are passed over *en bloc*, and the matter is placed in the hands of a German scientific man—highly and worthily distinguished, doubtless—but who, I am convinced, would be far from approving the slight placed on our countrymen, one of whom has accomplished what will ever be a classical research in this very subject. I submit that when I applied the expressions "spasmodic," "ill-considered," and "wanting in scientific method" to these proceedings, I was not using inappropriate language.

W. T. THISELTON DYER

Sensitive Flames

PERMIT me to thank Prof. Herschel for his all too kind acknowledgment of the aid my former brief communication to NATURE may have been to him. In a paper on Sensitive Flames that is awaiting the needful leisure to complete, I have given a brief history of this subject—which, by the way, so far as regards the discovery of sensitive flames, Prof. Herschel has partly misapprehended, though there can be no doubt the valuable letters of Prof. Herschel will play an important part in the development of these phenomena. I am glad to find that, so far as Prof. Herschel has recorded his views, they corroborate the results of my own experiments (begun as long ago as 1867) in search of the cause of the sensitiveness of various fluid jets, and the application of sensitive flames to acoustic investigation and other practical ends. For reasons, into which I will not enter here, I was led to postpone this inquiry, and it is only comparatively lately that it has been resumed.

The keynote to the whole of the phenomena is, I believe, to be found in Savart's beautiful investigations on liquid jets. Any fluid body, gaseous as well as liquid, escaping from an orifice in a tranquil stream, consists of a continuous and a discontinuous region, and is subject to the play of opposing forces which excite pulsations in the jet, the number of which is directly proportional to the velocity of the issuing stream, and inversely as the diameter of the orifice. When a note is sounded approximately in unison with the vibration number of these pulsations, the jet of water, smoke, or flame is thrown into more vigorous vibration, and a strained condition of the jet is set up.

Hence it is easy to obtain a series of sensitive flames, issuing from orifices of decreasing size, capable of responding (within a certain range) to the successive notes of the gamut; the higher notes affecting, of course, those flames from the smaller orifices, and which also require to be under greater pressure of gas than the flames responding to the lower notes. The relative rate of vibration of these flames is at once clearly seen by viewing them together in a moving mirror. But I will not weary your readers by further entering upon a subject with which already they must be somewhat tired.

W. F. BARRETT

Royal College of Science, Dublin, Nov. 30

Fossils in "Trap"

I AM much obliged by your insertion of my letter on "Fossils in Trap." You are right in supposing that the trap I referred to was crystalline augitic trap. If it had been tufa I should not have written to you as I did, as I was well aware that fossils in tufa were of common occurrence. Shortly after I wrote I found that the *Favosites gothlandica* which shows the section is still imbedded in a portion of the slate, which is olive-coloured, and closely resembling the trap. This is so intimately connected with the trap that it is impossible to trace a *line* of connection.

Halifax, Nova Scotia, Nov. 14 D. HONEYMAN

[Dr. Honeyman's discovery would appear to resolve itself into the simple fact that his "trap-dyke" has involved in its mass fragments of the fossiliferous strata through which the molten rock has risen—a fact, we presume, with which every practical geologist who has worked amongst igneous rocks must be more or less familiar.—ED.]

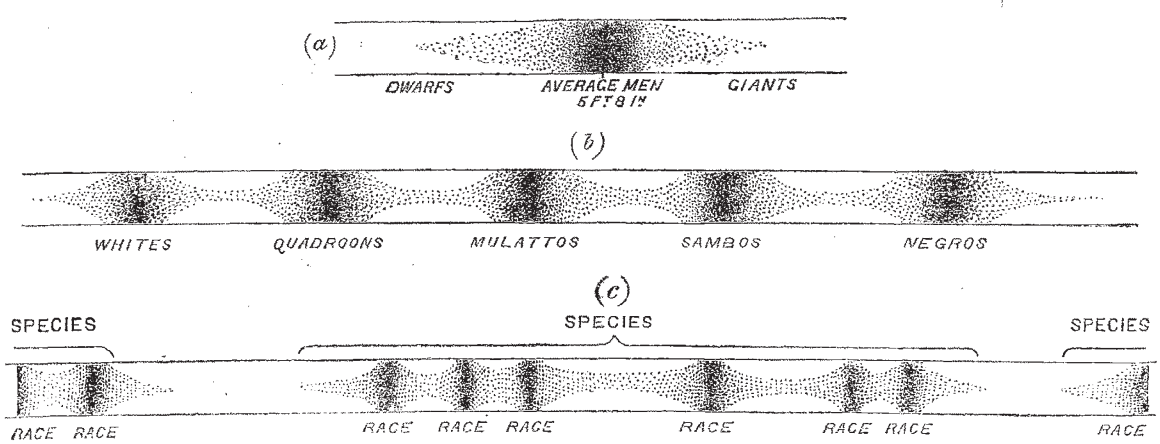
THE RELATION OF RACE TO SPECIES

IN a notice of Quetelet's works, published in NATURE, vol. v. p. 358, I raised the question whether this eminent statistician's method of defining a race or population might be applied to provide naturalists with a means of defining species. Since then, the consideration of Mr. Francis Galton's explanatory diagram, given at p. 28 of his work on "Hereditary Genius," has led me to attempt to carry this problem a stage further.

Instead of using, with Quetelet, a binomial curve to show the constitution of a race, with its central type and varieties, Mr. Galton sets before our minds the very indi-

viduals who compose the mass, each one being represented by a dot. His diagram, adapted in (*a*) of the present figure, stands for a population descended from a common ancestral stock, the individuals congregating most closely about the place of the central type or standard individual, and gradually decreasing in numbers as they become more different from that type or standard. In this graphic representation, the race can, of course, only be arranged in order as to some one quality. In the particular case for which Mr. Galton uses it, this quality is stature. The individuals of the mean or average height (say, 5 ft. 8 in.) are shown as most crowded, while the taller and shorter men become fewer and fewer as their stature becomes more unusual, till at last we come to one or two outlying giants and dwarfs, beyond whom no more individuals exist. Here, then, is set before us the distinctest idea of a race, both as to its type and as to its limits of variation on either side. I now proceed to apply the method of this diagram to a more complex state of things.

In nature we habitually find races blending into one another. Our own species shows this perfectly, when mixed breeds are considered. Let a population partly of Europeans and partly of negroes be placed on a West Indian island. These two races being classified according to colour, a few of the darkest Europeans would be seen to make some slight approach towards a few of the lightest negroes; but there would be no individual of either race who could be mistaken for one of the other. They would, therefore, at the outset be represented by two such groups of dots as (*a*), with a blank space between. But as soon as the first generation of mulattos come into existence the case will be altered. An intermediate race has arisen with its definite central type, and its variants now coming much closer to the whites on one side and to the blacks on the other. In the next generation there will be quadroons and sambos (cross between negro and mulatto, Spanish *zambo*). Now the fusion will be so complete, that of many individuals it will hardly be possible to say whether they are quadroon or mulatto, while in the same way others may be either mulatto or sambo, or either sambo or negro. One or two more generations would still further obliterate the distinction between adjoining varieties, but for convenience sake the figure (*b*), showing the blended races, is taken only in the second generation. In this way the whole human species, or any species of plants or animals, may be ideally classified into its various races, either in fact blending into one another, or capable of so blending by intercrossing. A species thus classified into its component races is shown either in (*b*) or the central part of (*c*).



Let us now attend to the effect of variation, artificial or natural. Starting with a single race, this may in the course of time and circumstance develope within itself a number of varieties or races. Nor, if variation is promoted either under domestication or by various conditions of life acting for a long series of generations, is there any difficulty in conceiving two adjacent varieties to recede from one another and the intermediate individuals to die out, till a wide gap is left between the two races. At first this gap, though real, would be capable of being at any time bridged over by cross-breeding, and thus would only be a temporary break. But as variation went on, a critical period would at last be reached, when individuals from the two sides could no longer produce fertile offspring. Then a separation of one species into two would have taken place. This change is illustrated in (*c*), where the extreme forms of two adjacent species are seen to the right and left, still perceptibly near the extremes of the original species from which they have parted, but never to be joined to it again unless by a process of backward variation most unlikely to happen across any width of interval. This ideal representation was at first intended rather to show the actual distribution of animals in existing species than to involve a hypothesis as to how these species originated. But, after consulting Mr. T. R. Stebbing, I see the desirableness of making the diagram express both facts and hypothesis, leaving those who will to take them apart. The whole figure, as it stands, contains an ideal of evolution or development from a single race of animals at (*a*), into a species made up of several races at (*b*), and thence into any number of separate species at (*c*).

EDWARD B. TYLOR

TRANSIT OF VENUS

Colonel Tennant's Station at Roorkee, India.

THE full and very able account of the preparations for observing the Transit of Venus drawn up by Prof. Forbes and published in these columns do not include those which have been made by the Government of India under the authority of the Secretary of State in Council. When Prof. Forbes wrote, these were not sufficiently advanced to admit of description. Now that they are completed it is desirable that an account of them should be made public.

At an early period Col. J. F. Tennant, R.E., F.R.S., brought the subject before the Viceroy in India, and proposed the organisation of a station in the north-west, near Roorkee, well known as the seat of the great Civil Engineering College. The Viceroy heartily responded to the suggestion, and communicated his views to the Home Government.

Some time was unfortunately lost in official correspond-