tion." This implication, which Prof. Lankester calls false, we do assuredly make in the most explicit way. We assert that the teaching is deserving of the title University teaching, and I am prepared to submit to Prof. Lankester overwhelming evidence in support of this assertion.

The definition which he gives to University teaching is one which includes all the good schools, and is in no sense diagnostic of University teaching. His first condition our lecturers completely satisfy, for they have, as a rule, been men of the highest academical position, in every way the equal of those who fill ordinary College Professorships and Lectureships.

We contend that the essential characteristic of University teaching is the *method* employed in dealing with a subject; that the teaching of the Universities is directed to the elucidation of the principles of the subject taught, and to the end of bringing all the mental faculties of the student into play, so that he may be placed on the high road to pursue his studies in their higher developments; and that an important factor in producing these results is the personal intercourse between teacher and student. We assert that the University Extension method possesses these characteristics.

Our idea is that Gresham College would best fulfil its founder's intention if it were enlarged into a great central College, with permanent Professorships and all the facilities for laboratory work; the Professors, however, teaching in the evening instead of the day, so as to provide for the needs of the class which Sir Thomas Gresham intended to benefit, and for which the London Society is, in a tentative way, attempting to make imperfect provision.

R. D. ROBERTS.

Charterhouse, E.C., November 5.

I CANNOT agree with Mr. Roberts that I have written under any important misapprehension of the nature and general objects of the organization of which he is secretary. I have expressed my conviction of the excellence of the courses of lectures given through its means. I object to its profession of bringing "University" teaching into London, and to its claim to represent either the University of the future or Gresham's University of the past. The fact that the Universities of Oxford and Cambridge and London have appointed members of a Committee to arrange lectures in London does not, in my opinion, constitute those lectures as parts of the teaching of those Universities, and the suggestion that this is the case—encouraged by the use of the term "University Extension"—is, in my opinion, greatly to be regretted. It is, perhaps, difficult to be sure as to the nature of the audiences contemplated by Gresham for his Professors. But supposing that his intentions could be realized in this special point by the delivery of lectures in the evening, I am at a loss to understand what public good can be served by the introduction of a new organization into London for the purpose of giving such lectures, when there are already two public institutions—viz. King's College and University College—which are not only ready to undertake such teaching if found desirable in the future, but have actually carried on such teaching in the past. The Professors of King's and University Colleges are University graduates, they are provided with laboratories and libraries and lecture-rooms, they have numbered among them some of the most distinguished scholars and *savants* of the day, and they have produced both trained investigators and large additions to existing knowledge. They only require additional endowment and public sympathy to fulfil in every respect the ideal of a true University in and for London. Yet a certain number of gentlemen connected with Oxford and Cambridge have persuaded those Universities to nominate a Committee called a Joint Board to kindly undertake the introduction of "University" teaching into London. I cannot believe myself that this new body, competing for the support of Londoners as representing the great educational want of London, viz. a real University, will fail to do harm by dividing the support which London can give to University institutions. I cannot suppose—after observation of their proceedings—that those who form the active body of the London Society for the Extension of University Teaching are as anxious to promote a true University in London as they are to find employment for their lecturers. This is quite natural, and, if admitted, is not otherwise than creditable; but the assertion of a claim to be representatives of University teaching in London on the part of these gentlemen is not so creditable.

November 6.

E. RAY LANKESTER.

The Barbary Ape in Algeria.

IT may interest your readers to know that monkeys are still to be found wild at a place within three days' journey of London, and easily accessible to the most unenterprising traveller. Yesterday, in company with my son, I drove up the gorge of the Chifa, on the excellent main road between this place and Mediah. We halted at the spot where the appropriately named "*Ruisseau des Singes*" falls into the Chifa on its left bank, and ascended the narrow side-valley on foot. Its steep slopes are densely covered with brushwood, intermixed with a few oaks and stunted junipers. We had not proceeded more than ten minutes from the main road before we heard the chatter of a Barbary ape on the bank above us, and saw him scrambling along the rocks. Shortly afterwards, a fine large male of the same species was kind enough to mount a juniper-tree on the opposite side of the gorge to that on which we were seated, and exhibited himself to our gaze for at least fifteen minutes. His various attitudes were distinctly observable through a pair of opera-glasses, and we calculated his distance from us as not more than 400 yards in a straight line. A third ape was subsequently met with farther up the gorge, at a much nearer distance, but did not wait to be looked at.

I had previously seen Barbary apes on the Rock of Gibraltar, but they are there in a semi-protected condition, and perhaps introduced. In the gorge of the Chifa they are quite in a "state of nature," and in their native wilds.

P. L. SCLATER.

Hôtel d'Orient, Blidah, Algeria, October 29.

Are there Negritos in Celebes?

PROF. FLOWER, in his interesting lecture on "The Pygmy Races of Men" (Journ. Anthr. Inst. vol. xviii. p. 82, 1888, and NATURE, vol. xxxviii. p. 67), after having spoken of the Negritos in the Philippines, says, apparently on the authority of Quatrefages: "As the islands of these eastern seas have become better known, further discoveries of the existence of a small Negroid population have been made in Formosa, in the interior of Borneo, Sandalwood Island (Sumba), Xulla, Bouron, Ceram, Flores, Solor, Lomblem, Pantar, Ombay, the eastern peninsula of Celebes, &c."

Without discussing here the foundation of this whole statement, I only beg to remark that in my opinion no Negritos or the like exist in the eastern peninsula of Celebes, or in the Island of Celebes at all.

Already in the year 1876, in a lecture, "Die Minahassa auf Celebes" (p. 29, note 11), I said:—"Prof. Gerland places Papuans, in the map of Waitz's 'Anthropologie der Naturvölker' (vol. v. part 1, Malays), in the eastern peninsula of Celebes, but I could not find in the letterpress of the work, on whose authority he makes this entry. It was this very note of Gerland, which induced me, when on the spot (in the year 1871), to search after them, but I did not succeed in discovering the slightest positive proof for such an assertion." And (l.c. p. 8) "In Celebes . . . no autochthonic Papuan element has been discovered." Neither has Dr. Riedel, the special and foremost investigator of the whole island, obtained any trace of Celebesian Negritos. I am therefore of opinion that Celebes at least (if not many more—perhaps all—of the quoted islands) ought to be omitted from the list.

As to the occurrence of Negritos in the Philippine Islands, I only spoke of them as existing in Luzon (as generally known), in Panay, Cebu, and Negros (see *Zeitschrift für Ethnologie*, 1873, p. 90, and "Ueber die Negritos oder Aëtas der Philippinen," Dresden, 1878, p. 25).

A. B. MEYER.

Royal Museum, Dresden, October 24.

Altaic Granites.

HUMBOLDT and Rose, when descending the Irtysh between Boohartinsk and Oostkamenogorsk, saw large masses of granite lying as if poured on the ends of metamorphosed slates (S. Rose, "Reise nach d. Ural," i. 610); an observation mentioned by Zirkel ("Petrogr.," i. 506, 1866) as a famous one in relation to the age of the Altaic granite. No subsequent traveller appears to have succeeded in repeating that observation, because nobody could rediscover the actual place, which Humboldt and Rose did not define with much precision. Ritter, however, in referring to the subject, indicates the place as lying between two rivulets—Baryshnikof and Kozlovskaya.

After some unsuccessful attempts, I at last succeeded in finding this interesting locality. It is situated some five or six miles

from Boohtarminsk, on the right bank of the river, just at the entrance into the Kozlovskoe gorge, and is known among the local inhabitants under the name of Slepoy Borok (Little Blind Forest). But the illustrious travellers have made rather a blunder in defining, after only a rapid examination from the deck of an Irtyshian karbaz, the mutual relations of the rocks. A section at right angles to the stream shows that the granite lies not *on* the slates, but *in* them, and that it occurs as a main vein, with some secondary ones, all having the same strike and dip as those of the slates. The main vein is some 30 or 40 metres thick, the secondary 0.5 metre, or still less. From the river one can see only the lower limit of granite, and as the joints of this rock are nearly horizontal, whilst the bedding of slate stands almost vertical, the appearance is presented which suggested the original inference that the granite has been poured out over the ends of the slates.

My measurements gave the following results:—On the level of the current the strike of the slates varies between 9h. 45m. and 10h., dip = 82°–85° north-north-east; lower limit of granite strike, 7h. 30m., dip = 73° north-north-east; upper limit of the same, strike 8h. 45m., dip 43°–45° north-north-east. The contact of the two rocks can be observed occasionally exceedingly well. In the vicinity of the granite the slate loses all traces of fissility, and becomes a very compact rock, with abundant scales of muscovite. Generally the slates are schistose, phyllitic, and chistolitic. Both rocks are welded together. The secondary veins of granite somewhat differ in structure from the main vein, which is of a normal fine-grained variety, with little scales of biotite. The main vein is covered with young forest, consisting of pine, birch, and aspen trees, while on the slates nothing grows but rare bushes of gooseberry, honeysuckle, some species of horse-tail, and some grasses.

The conclusion of Humboldt and Rose, that some Altaic granites are younger than local schists and slates, remains indisputable. I wish to add that they are also younger than some local greenstones, as may be seen at Beshbanovskoe crags on the Irtysh, near Oostkamenogorsk, where a dioritic breccia is cemented with granitic matrix.

A. BIALOVESKI.

Oostkamenogorsk, Western Siberia, September 20.

Rankine's Investigation of Wave Velocity.

THE investigation relating to the propagation of waves contained in chapter xv. of Maxwell's "Theory of Heat," and based on that of Rankine (pp. 530–31 of "Collected Papers") presents peculiar difficulty to most readers. "The kind of waves to which the investigation applies are those in which the motion of the parts of the substance is along straight lines parallel to the direction in which the wave is propagated, and the wave is defined to be one which is propagated with constant velocity, and the type of which is not altered during its propagation."

Two cross-sections of unit area at a fixed distance apart are conceived to travel through the substance with the velocity of the wave, inclosing between them a cylindrical space within which things are always in the same condition though the matter is continually changing. The momentum of the matter which enters through the front section in the unit of time is duly expressed, and also the momentum of the matter which escapes at the rear section. The difference of these two momenta is then equated to the difference of the pressures before and behind. The puzzle is to justify this *quasi* deduction from the second law of motion; and in connection with this puzzle, the question of sign occurs. For instance, if the momentum of the entering fluid exceeds that of the issuing fluid are we to attribute the gain of momentum to the fact that the contents of the cylinder are more strongly pushed forward behind than they are pushed backward in front? Such is the impression produced on the reader's mind by Maxwell's words: "The only way in which this momentum can be produced is by the action of the external pressures"; but it is not correct.

The momentum included between the two travelling sections is changed in two distinct ways: first, by convection—that is, by gain and loss of moving matter; secondly, by pressure before and behind. *The change from pressure must be equal and opposite to the change from convection; since, by hypothesis, the momentum included between the two sections remains always the same.*

Rankine merely says, "Then in each unit of time the differ-

ence of pressure $p_2 - p_1$ impresses on the mass the acceleration $u_2 - u_1$," and gives no explanation.

I remember being puzzled by this reasoning of Maxwell's some years ago, when I was writing Note A in Part 4 of "Deschanel," and getting over the difficulty by taking the two sections very near together; but my attention has been drawn to it afresh by the receipt of a paper by Prof. MacGregor, of Nova Scotia, in which the difficulty is pointed out, and evaded in the same manner in which I evaded it. Prof. MacGregor points out that Maxwell obtains a correct result only by help of a mistake in the algebraical work—the sign of a difference being changed in obtaining equation (7) from equation (6). This is certainly true as regards the fourth and fifth editions, which are now before me. In a later edition, Prof. MacGregor remarks, the sign of the difference is changed in equation (6), thus making the algebra right, but at the expense of making equation (6) inconsistent with what goes before it. The explanation contained in the sentence printed in italics above clears up the difficulty.

J. D. EVERETT.

Belfast, November 2.

Alpine Haze.

PROF. TYNDALL'S letter in NATURE about Alpine haze induces me to say that as a non-scientific observer I have never, I think, during a residence of many years here, seen so much *local* fog or haze as this autumn.

On October 29—a perfectly clear and cloudless day here (Vevey and La Tour), with no appearance of fog, haze, or cloud, anywhere in the distance—I received a letter from Lausanne saying, "While I write (11.30) so dense a fog has suddenly come up that we fear for the boats on the lake." Other friends took a trip to Lavey les Bains. They were in perfectly clear air until a little beyond Villeneuve, when they found the whole Rhone Valley thick with fog, but on turning off at St. Maurice Station to Lavey les Bains (ten minutes' walk, and on perfectly flat ground) they again came into a quite clear atmosphere. As no fog whatever came here all day, I cannot say whether it was aqueous or not. We have both sorts here from time to time, but most commonly *dry*; this year has been rather an exception. I should say fogs had been more frequently damp than usual, and by observing the grass morning and evening I have found that there has been much more dew than is common in this locality.

Streaky hazes or "long horizontal stræ," as Prof. Tyndall calls them, have certainly been unusually prevalent this year.

La Tour de Peilz, November 4.

M. C. C.

The Animals' Institute.

THE long-continued suffering of animals fatally injured in our streets, before the services of a slaughterer can be obtained, or the owner be found to give his permission, has often been referred to. Poor animals with incurable abdominal wounds, or, it may be, complete fracture of a limb, not unfrequently lie in the streets for hours before being put out of their misery. The police have no power to order their destruction until the person in charge assents, and he frequently cannot do this until his master has been communicated with. I remember one case where eight hours elapsed. I have recently found that complete absence of pain can be easily induced by subcutaneous injection of morphia, and perhaps you would allow me to publicly state that the apparatus and drug are always here at the service of the police gratuitously in cases of street accidents.

JOHN ATKINSON.

9 Kinnerton Street, Wilton Place, Knightsbridge,
November 5.

N. M. PRJEVALSKY.

A TELEGRAM from Vyernyi—one of those small Russian towns which have grown of late in the outspurs of the Tian-Shan Mountains—announces the death of Prjevalsky, the bold and indefatigable explorer of the wildernesses of Central Asia. In September last, immediately after having terminated the work which embodies the results of his fourth great journey to Central Asia, he started on a new journey, the fifth, thus prosecuting again what has been the aim of his life during the