note (*loc. cit.*, August) that Prof. Morse's latest corrected result (his own corrections) for the osmotic pressure at 0° C. of his 1·0 weight normal aqueous solution of cane sugar is 24·45 atmospheres, while ours, by the method under discussion, was 24·5 atmospheres.

Evian les Bains. BERKELEY.

The Rotation of a Crystal of Tourmaline by Plane Polarised Light.

(*Preliminary Note.*)

WHEN a beam of plane-polarised light is incident normally on a plate of tourmaline cut parallel to the optic axis, it will be absorbed or transmitted depending upon whether the axis of the tourmaline is parallel or perpendicular to the plane of polarisation of the incident light. If the arrangement is such that the light is absorbed, then in a given time a definite amount of heat energy will have passed from the source of light into the plate of tourmaline, and if, as is necessarily true, the former is at a high temperature while the latter is at a low temperature, it is plain that the entropy of the system will have been increased. The same increase in entropy would not have taken place if the orientation of the tourmaline had been such that the light had been transmitted.

Since the entropy of a system always tends to increase, it seemed to be of some interest to try what would happen if a plate of tourmaline was suspended so as to be free to rotate about an axis perpendicular to its faces, and then allowing a beam of plane-polarised light to fall on it in the direction of the axis of rotation, the arrangement being such that the plane of polarisation should make an angle of 45° with the optic axis of the tourmaline.

The experiment was tried by the author during the month of July, 1908, at the Rouss Physical Laboratory of the University of Virginia, and the results, though not absolutely conclusive, indicate that a moment acts on the tourmaline tending to set its optic axis parallel to the plane of polarisation of the incident light. In other words, the system tends to arrange itself so that as large a percentage of the light as possible shall be absorbed.

The apparatus used consisted of a fine plate of tourmaline, 1 cm. square, and 2·96 mm. thick, weighing almost exactly one gram. This was fastened to one end of a short straight copper wire to which was also fastened a small plane mirror. The system was suspended in a suitable vessel having a plane glass top and bottom, which could be exhausted, the suspension being such that the system had a period of 29·5 seconds. From the period and the moment of inertia of the system it was calculated that the moment necessary to give a deflection of 1 cm. with the scale at a distance of one metre was $2{\cdot}5 \times 10^{-5}$ dyne-cm. A beam of approximately parallel light from an arc was reflected in a vertical direction from a plane silver mirror, and was rendered plane polarised by a Nicol before entering the vessel.

Owing to the fact that the suspended system was not perfectly symmetrical with respect to its axis of rotation, it was found that the zero moved constantly in one direction when the light was allowed to fall on the tourmaline. This was undoubtedly due to radiometric action, and possibly also to the pressure of light. The motion was, however, much slower when the angle between the plane of polarisation and the optic axis of the crystal was +45° than when it was −45°. An average of twenty trials gave 150 seconds as the time to get a deflection of 30 cm. in the first case, while the time required for the same deflection when the angle was −45° was 90 seconds. These trials were all made with as high a vacuum in the vessel as could be obtained by means of the Gaede rotating mercury pump. The experiment was repeated at various pressures up to 10 or 15 cm. of mercury; the results were qualitatively the same, that is, the rate of deflection was much slower in every case when the angle mentioned above was +45° than when it was −45°, but the motion of the zero in one direction, although somewhat different at different pressures, could not be avoided.

If the cause of this motion of the zero is what was stated above, then it ought to disappear when the suspended system is made symmetrical about the axis of rotation.

These experiments were only preliminary, and during the coming winter a more careful investigation of the question will be carried out at the physical laboratory of the Johns Hopkins University. JOHN A. ANDERSON.

Physical Laboratory, Johns Hopkins University, August 15.

Access to Chemical Works.

STUDENTS of chemistry so often complain of the extreme difficulty of obtaining access to chemical works that it may be well to bear in mind that the universities and technical schools of the United Kingdom have perhaps a remedy for what is a serious obstacle to a proper study of chemistry. The amount of chemicals now consumed in educational laboratories must be enormous, and, as regards ordinary materials, quite sufficient to employ a large factory. A very large proportion comes from Germany. Let the universities start their own cooperative factory for the manufacture of the acids and salts they require. Let chemical students both have free access to it and put in part of their chemical course as workers there. The action of several universities in running their own farms for the benefit of agricultural students supplies a kind of precedent. When motives of education, patriotism, and economy point the same way, the scheme is worth consideration.

CHEMIST.

FLY FEVER IN AFRICA.

IN the *Times* of June 27 there appeared an article on "fly fever" in Africa, and the suggested destruction of big game. It appears that Prof. Koch has lately suggested that the African big game should be exterminated in order to destroy the principal means of nourishment of the tsetse fly. As this fly is the carrier of the infective agent of "fly fever," its extinction would, in Koch's opinion, blot out the disease. The members of the German Society for the Preservation of Game in East Africa are, naturally, opposing tooth and nail this proposed drastic measure of Prof. Koch. They deny the truth of his conclusions, and hold that the fly disease can exist where there is no big game, and, further, that there are other methods of getting rid of the fly without destroying the game.

This question is a complicated one, and perhaps if I give a short sketch of the history of the "tsetse-fly disease" I shall best clear the way to its better understanding.

When I went to Zululand in 1894 at the request of the Natal Government, to investigate an outbreak of nagana among native cattle, I was unaware that nagana and tsetse-fly disease were one and the same. Further, I believed, with the rest of the world, that the fly disease was caused by the poison of the tsetse fly, just as an animal is killed by the poison of a snake. Soon after arriving in Zululand a parasite, the *Trypanosoma brucei*, was discovered in the blood of the affected animals, and it soon became apparent that fly disease and nagana were one and the same. By suitable experiments it was demonstrated that this trypanosome is the cause of the disease. The next fact made out was that the trypanosome could be conveyed from sick to healthy animals by the local species of tsetse fly (*Glossina morsitans*). As this tsetse fly only remained infective for forty-eight hours, it was evident that it must get the parasite somewhere, and it seemed most probable that this would be from the wild animals living in the "fly country." This was found to be the case, and the *Trypanosoma brucei* was demonstrated in the blood of the buffalo, wildebeeste, koodoo, and other big game, both microscopically and by means of inoculation experiments.

Now this is a short summary of the discovery of the cause of the "tsetse-fly disease," and one point I want to bring out is that at that time, and for some time after, there was supposed to be only one "tsetse-fly disease" and one species of tsetse fly. The disease was called "the fly disease," and the fly "the tsetse fly." Now all this is changed, and "the fly disease" is now a generic term for several diseases, and the tsetse flies are found to be made up of several more or less well-marked species.

But the taking for granted that there was only one tsetse-fly disease of course led to much confusion and many mistakes. For example, when Koch studied the fly disease in German East Africa in 1898, naturally he considered he was dealing with the Zululand disease, nagana. But was he? Lately, I have studied a trypanosome from the East Coast which causes a more or less mild disease in horses and other animals. Is it not possible that Koch was dealing with this East Coast species when he said that Masai donkeys were not susceptible to nagana? So it can be easily understood how a great number of erroneous notions have crept into the literature of this subject.

It is evident that what is true of nagana, the disease caused by *Trypanosoma brucei,* need not be true of the diseases caused by *Trypanosoma theileri, dimorphon, pecaudi, congolense, vivax, nanum,* &c. But an observer comes in contact with one of these diseases in a place where there is no big game and no tsetse fly, and he at once thinks that "the fly disease" does not depend on big game or tsetse flies. When I say that "the tsetse fly" disappears from a district when the big game are killed off, and with the extinction of the big game that "the fly disease" also disappears, I only mean that *Glossina morsitans* disappears, and that a particular "fly disease"—that called by me nagana, and caused by *Trypanosoma brucei*—becomes extinct. I do not mean that the diseases caused by *Trypanosoma dimorphon,* &c., will be blotted out by the same means.

With the exception of nagana and sleeping sickness there is little real knowledge as to how the other African trypanosome diseases are spread. That they may spread by other agencies than tsetse flies is probable, since surra spreads in India, although there are no tsetse flies in that country. It is quite possible that many of these diseases in Central Africa may not be spread by the agency of tsetse flies and may not depend on the big game as a reservoir of the virus. The cattle themselves may be the reservoir, and the disease may be spread in the herd by means of any of the common biting flies, such as stomoxys or tabanus. In sleeping sickness, so far as we know, the native himself is the reservoir of the disease.

It is therefore, in my opinion, very important that, in the first place, these trypanosome diseases should be more thoroughly studied as to their distribution, their carrying agent, and the reservoir of the virus. When this is done it may well be that, by the use of this knowledge alone, owners of stock may escape damage. Now that we know the natural history of sleeping sickness, its distribution, its carrying agent, &c., any intelligent person has only himself to blame if he contracts it.

These few sentences will show how complicated a subject "the fly disease" has become, and in what a state of confusion and chaos the classification of this family of diseases at present is.

Lastly, in regard to the suggested destruction of big game. To begin with, it may be said that civilisation and big game cannot exist together. As soon as a new country is divided off into farms, either for agricultural or stock purposes, the great mass of the wild animals must go. Take, for example, the destruction of the fences by stampeding herds of zebra, wildebeeste, or buffalo, not to speak of the probability that there is not enough food to go round. Even in exceptional cases, where the wild animal has been protected from sentimental and picturesque reasons, as in the case of the herd of hippopotami preserved until lately in Natal, a time came when the neighbouring farmers could no longer put up with their destructive habits, and they had to be destroyed. We may say, then, that when a country becomes settled and civilised, the big game go. This has occurred in Cape Colony, the Orange River Colony, Transvaal, and Natal, and will occur in Zululand when that country is opened up.

But this inevitable disappearance of wild animals before the advance of civilisation is very different from the instant carrying into effect of an international measure for the wholesale destruction of big game all over Africa. Such a measure, in the present state of our knowledge, would be quite unjustifiable, and would probably fail to a great extent in its object. *Festina lente.* Let local authorities frame regulations from time to time as the exigencies of the place demand. But there ought to be room for the next thousand years in many parts of Africa for game reserves in which all the varieties of big game may live, thereby gladdening the eye and enriching the imagination and fancy of many future generations, and delaying the day when man will have for his sole companions the domestic hen, the cow, and the motor.

DAVID BRUCE.

THE LATE HENRI BECQUEREL.

ON Tuesday, August 25, 1908, died suddenly Antoine Henri Becquerel at Croisic, in Brittany, at the comparatively early age of fifty-six.

Henri Becquerel was the third of the scientific dynasty of that name. His grandfather, Antoine César Becquerel (1788–1878), a contemporary of Faraday, was a most prolific investigator of electrical and electrochemical phenomena. He was for forty-nine years a member of the Academy of Sciences, and from 1837 until 1878 professor of physics at the Musée d'Histoire naturelle in Paris. The second Becquerel, Alexandre Edmond (1820–1891), who is known chiefly for his researches in phosphorescence, which are embodied in the two volumes of his book "La Lumière," also made important investigations on thermo-electricity and on underground temperatures. He was professor at the Conservatoire des Arts et Métiers, and succeeded his father as professor and administrator of the Musée d'Histoire naturelle.

Into this distinguished family Henri Becquerel was born on December 15, 1852. He was educated first at the Lycée Louis le Grand, and at the age of twenty entered the École polytechnique. In 1875 he entered the service of the French Government as an Ingénieur des Ponts et Chaussées. Three years later, on the death of his grandfather, when his father succeeded to the full professorship at the Musée d'Histoire naturelle (the duties of which he had discharged for some years), young Becquerel was appointed his assistant under the title of "Aide-naturaliste."

Already Henri Becquerel had begun to show his powers in original research. The *Comptes rendus* for 1875 and 1876 contain his earliest papers, researches on magnetic rotatory polarisation. These were continued in 1876 in the *Journal de Physique;* while in a fourth memoir he discussed the effect on the phenomenon of using different wave-lengths. In 1878 he announced the discovery of the magnetic