sciences of Nature must be content to recognise individuals as the only real entities, and to accept species, like genera, families, and orders, as convenient but purely artificial conceptions.

The geological study of minerals leads us to regard each specimen that we examine as possessing a distinguishing combination of properties, some of which are impressed upon it by causes operating when it came into being, while others are no less clearly the result of the long series of vicissitudes through which it has since passed.

Of all the branches of mineral morphology there is none from the study of which the geologist has gained more in the past, or from which he has greater reason to look for future aid, than that of the embryology of crystals.

In the year 1840 Link showed that the first step in the formation of crystals in a solution consists in the separation of minute spherules of supersaturated liquid in the mass; and subsequently Harting in Holland, and Rainey and Ord in this country, obtained a number of interesting experimental results, by allowing crystallisation to take place slowly in mixtures of crystalloids and colloids.

Valuable contributions to the same subject were made by Frankenheim, Leydolt, and others; but it is to Hermann Vogelsang that we owe the greatest and most important contributions to mineral-embryology. By the ingenious device of adding viscous substances to solutions in which crystallisation was going on, he succeeded in so far retarding the rate at which the operation took place as to be able to study its several stages. He thus showed how the minute "globulites," gathering themselves into nebulous masses or ranging themselves according to mathematical laws, gradually build up skeleton-crystals, by the clothing of which the perfect structures arise.

Since the early and regretted death of Vogelsang, the subject of the development of crystals from their embryos, the so-called *crystallites*, has been successfully prosecuted by Behrens, Otto Lehmann, Wichman, and other investigators.

Now in all glasses—whether of natural or artificial origin—in which the process of primary devitrification is going on, we have examples of the growth of crystals in a viscous and retarding mass, and in these, as Leydolt, Zirkel, and Vogelsang clearly saw, admirable opportunities are afforded to us for studying the formation of crystallites, and the laws which govern the union and growth of these into crystals. Two years ago, my predecessor in this chair submitted to you the interesting results of his own researches upon the devitrification of artificial glasses and slags; and the subject has since been pursued by Velain in France, and by Hermann and Rutley in this country.

The igneous rocks supply us with admirable opportunities for studying mineral embryology. In the same rock-mass we may sometimes find every possible graduation, from an almost perfect glass to a holocrystalline aggregate. By the study with the microscope of the several transitions in different parts of the mass, we obtain data for the most important conclusions concerning the phenomena of crystal-development.

There is another line of research in connection with mineral-embryology, which appears to be full of promise, and which has not yet received all the attention it deserves. In the "contact-zones" around great igneous intrusions, we find the curious so-called "spotted slates," which under the microscope are seen to contain nebulous patches, the mere ghostly presentments of crystals, struggling into being in the amorphous mass. The development of these nebulous masses into perfect crystals, exhibiting the characteristic external forms and optical properties of andalusite and kyanite, of garnet and epidote, of hornblende and mica, may be traced in some cases with the greatest facility.

More complicated still are the phenomena exhibited along the foliation-planes of the rocks, which have been made to flow in the act of mountain-making. There, as the old minerals are destroyed, new ones build themselves up from their elements. The study of all the steps of this process is an undoubtedly difficult one, but the results already obtained by Reusch, Lossen, Heim, and Lehmann, by Lapworth, Teall, Roland Irving, and Williams, lead us to look hopefully forward to the full solution of the grand but complicated problems of regional metamorphism.

The field of mineral-embryology is indeed a promising one, and its diligent cultivators may hope to gather a harvest no less rich than that which has been reaped by the workers in the same department of the biological sciences.

(*To be continued.*)

TABASHEER

I HAVE often wondered that this curious substance has never attracted more attention. But scanty references to it are to be found in books, and yet it seems to me that few more singular things are to be met with in the vegetable kingdom.

In Watts's "Dictionary of Chemistry" (vol. v. p. 653), exactly six lines are devoted to it. It is defined to be: "Hydrated silica, occurring in stony concretions in the joints of the bamboo. It resembles hydrophane, and when thrown upon water does not sink till completely saturated therewith." It is further stated to be the least refractive of all known solids, and an analysis by Rost von Tonningen of a specimen from Java gives a composition of 86·39 per cent. silica soluble in potash, 4·81 potash, 7·63 water, with traces of ferric oxide (to which I suppose its occasional yellowish colour to be due), lime, and organic matter.

There are several specimens in the Kew Museums, partly derived from the India Museum. All consist of small irregular angular fragments, varying from the size of a pea downwards, and opaque white in colour. It is obvious that these fragments are the debris of large masses.

Now, the presence of considerable solid masses of so inert a substance as hydrated silica in the plant-body is a striking fact. At first sight, one might compare it do the masses of calcium phosphate which form the endo-skeleton in the higher animals. These, however, serve an obvious mechanical purpose, which cannot be attributed to the lumps of tabasheer in the hollow joints of a bamboo. The presence of silica may sometimes serve an adaptive purpose, as in the beautiful enamelled surface of canes. And according to Dr. Vines ("Physiology of Plants," p. 21), "Struve found that it constitutes 99 per cent. of the dry epidermis of *Calamus Rotang*."[1]

In a few other groups of plants, such as *Equisetum* and the *Diatomaceæ*, it is a characteristic constituent. In all cases it principally occurs in the cell-wall (Vines, *l.c.* p. 137. This has suggested the highly ingenious speculation that, seeing the intimate chemical relationship which ob ains between silicon and carbon, there might be a silicon-cellulose. I notice that Count Castracane, in his Report on the *Diatomaceæ* collected by the *Challenger*, speaks of its "having been already shown that silica is sometimes substituted for carbon in the formation of cellulose" (p. 7). Judging from ash-analyses it might be supposed that silica was an essential constituent of gramineous plants. But by the method of water-culture Sachs has found that maize, for example, will grow with only a trace of silica. I must confess to ignorance of all that may have been done in the matter recently. But Ladenburg thought, and I think with reason, that the indifference of the plant to silica was a

[1] Sachs remarks ("Text-book," second edition, p. 700) that silica accumulates chiefly in the tissues exposed to evaporation, though this clearly does not apply to the case of diatoms.

strong argument for a silicon-cellulose in which silicon might or might not with equal physiological convenience play the part of one or more atoms of carbon. Fascinating as this hypothesis is, I am bound to say that the prolonged investigation which he devoted to the question is on the whole adverse to the idea of silicon playing any part of the kind.

It still remains then an unsolved problem why, when no adaptive end is involved, plants should take up such relatively enormous quantities of silica. The case of the frustules of *Diatomaceæ* is peculiar, as there the silicious wall is apparently a continuous plate of inorganic matter capable of resisting without impairment treatment by the most destructive and disintegrating agencies known Yet Castracane adduces evidence to show that such walls can grow; and as this can only be by interstitial growth, a molecular constitution is implied quite different from anything physical, and precisely similar to that of a cellulose membrane. He quotes, indeed, von Mohl for the opinion that the wall is not simply inorganic, "but only an organic membrane which is impregnated with silex."

Now, in the case of tabasheer, it is quite evident that the plant takes up an amount of silica beyond its powers to use, and so it is exuded into the hollow cavities of the bamboo stem. I do not mind confessing that, in so far as I had reflected on the matter at all, I had pictured to myself this as taking place by some process of secretion, so that the mass of tabasheer ultimately accumulated from successive portions of thrown-off silica. I was obliged, however, to give a little more serious thought to the matter when Prof. Cohn, of Breslau, wrote to me that he proposed to investigate the whole subject, and asked for help in the way of specimens and information. It then struck me what a very singular thing the phenomenon of the occurrence of tabasheer really was. I set to work to hunt up in the literature of Indian botany some rational account of the matter. The only ray of light I got was from the "Forest Flora of North-West and Central India," by Dr. Brandis, late Inspector-General of Forests to the Government of India. Everyone who knows Dr. Brandis knows that he gave to administration the energy he would more willingly have devoted to scientific pursuits. I was not at all surprised to find, therefore, modestly hidden in his book (p. 566) the key to the riddle. He says: It is not at all impossible that the well-known silicious deposit (*tabasheer*) which is found in the joints of this and other species [*Bambusa arundinacea*] may be the residuum of the fluid which often fills the joints." I communicated this to Prof. Cohn, and he was good enough to tell me that he quite agreed that this was the correct explanation. I at the same time wrote to Dr. King, the distinguished Superintendent of the Royal Botanic Garden, Calcutta, to know if it were possible to procure specimens of tabasheer *in situ*, as we possessed in our Museum nothing but broken fragments. I extract from several letters he has written me the following particulars:—"January 11. I have inquired of several old workers as to the situation tabasheer occupies. They all say it is found either on the *floor* of the joint, or if (as is so often the case in *B. Tulda*) the stem leans over, it is also found on the lower wall. It is never found on the roof of a joint. . . . . Tabasheer is not common in bamboo grown near Calcutta. And, besides, it is apt to be forced out of its natural position by the forced used in breaking a joint open. There is no external mark by which a tabasheer-bearing joint can be recognised prior to being opened." "January 18. I have got a specimen of tabasheer *in situ* for you. It concretes *as a jelly*, and is now being carefully dried off."

I think that these extracts (in which the italics are mine) fully confirm the explanation as far as I know first put out by Dr. Brandis. The rapidity of growth of a bamboo shoot is well known to be enormous. The root-pressure is probably equally great. The joints, at first solid, become hollow by the rending apart of the internal tissues, and water containing silica in solution is poured out into the cavities so formed. When the foliage is developed, transpiration is active: the water taken up from the ground is rapidly got rid of; not merely is the root-pressure compensated, but the water poured out into the joints is re-absorbed. It is not easy to see why the silica should not be always taken with it, as in the vast majority of cases it no doubt is. But in the cases in which it is left behind it has apparently simply undergone a process of dialysis. The determining causes of the occasional deposit of tabasheer are, I think, still obscure. But, as Prof. Cohn intends to investigate the subject, I think we may pretty confidently look forward to an exhaustive explanation.

It is a well-known fact that a large proportion of the ash-constituents of plants may have but little significance in their nutrition. The chemical constitution of plants, as far as their ash is concerned, to a large extent varies with the nature of the soil in which they are grown. It is quite certain that they will in consequence take up a vastly larger proportion of certain constituents than they can turn to any physiological account. Tabasheer is a striking instance of one such case. The calcareous masses found in the wood of many Indian trees mentioned in NATURE, vol. xxi. p. 376, affords another.

W. T. THISELTON DYER

ON THE EARLIER TRIPOS OF THE UNIVERSITY OF CAMBRIDGE

I HAVE read with great interest the papers by Mr. Glaisher in NATURE of December 2, 18, and 30, 1886 (pp. 101, 153, and 199), entitled "The Mathematical Tripos." Through the period common to Mr. Glaisher's notes and my recollections, I believe that we are strictly in agreement. I am able, however, to supply some little histories (I wish these had been more numerous and more certain) relating to transactions several years earlier than those known, personally, to Mr. Glaisher, and I am desirous that their memory should not be totally lost. There are now few persons, perhaps none, whose recollections of the University of Cambridge and of Trinity College go so far back as my own.

I first advert to the official course of undergraduates' life.

Shortly after introduction to the College in the October Term of 1819, I attended, with all other freshmen, in the Senate House or in the College Hall (I believe the latter) to take the oaths of allegiance and supremacy. With great ardour I renounced the "damnable doctrine" that the Pope of Rome could absolve subjects from their allegiance, with several similar declarations; and I also disclaimed all connection with other Universities and Colleges, and in particular with Wolsey's College at Ipswich. I believe (but have no certain knowledge) that these puerilities terminated a few years afterwards.

The undergraduates were arranged in "sides," divided under the official tutors under whom they were entered in the College Lists. There were then two "sides"; subsequently there were three. The lectures on each side were held in the College rooms of the tutor or his assistant tutor. The lectures consisted, naturally, in proposals of theorems and problems (in writing) and oral discussion of the answers in a friendly style.

The annual College examinations of the undergraduates of all sides (collected), of each year of undergraduateship, were held in the College Hall at the practical termination of the May Term. The order of merit in each year, as determined by these examinations, was published by lists of names suspended in the College Hall. Small sums of money, to be expended in honorary prizes, were assigned to the First Class of each year.

In the third year of undergraduateship arrived the time