entire Arctic circle, just as the Arctic flora is now; Asia, Japan, and North-West America being then continuous; that as the climate became cooler in the pliocene epoch, it was driven southwards along every meridian, its descendants now existing in the localities above mentioned. It will be seen that these form a belt all round the globe, roughly speaking, between the 30th and 40th parallels of latitude. Migration to some extent might have taken place along that belt, but the great migration I suggest was probably from north to south, and not from east to west, or *vice versa*.

The above theory is simply an adaptation of that given by Dr. Hooker for a similar dispersion of the existing Arctic flora, which replaced the temperate floras of Europe, Asia, and America; but which on the return of a temperate climate retreated northwards as well as up temperate and even tropical mountains, perishing, however, in the low lands.

GEORGE HENSLOW

The Fertilisation of Orchids

WHILE botanising this spring in Portugal, I was struck with the fact that scarcely one of the orchids—species of Ophrys principally—that I had collected for my herbarium, or examined in the field, seemed to be fertilised, for none presented the least indication of having had pollen applied to its stigmatic surface; and I examined flowers in every stage of expansion, from the opening of the bud to the withered and shrunk up floral envelopes. Each one, I remarked besides, contained its own pollinia, their caudicles in their respective glands and in their natural position. I was so struck with this, that one day (March 31) I gathered and examined forty-five different flower-heads, and of all these only one was found to have pollen-grains on its stigma, and all, the fertilised one included, had their pollinia intact. The locality was the Tapada d'Ajuda, or Royal Park, situated just outside the city walls of Lisbon, an inclosure containing many acres of land, clothed in spring with a rich flora, and a favourite entomological hunting ground, teeming with Coleoptera, Hymenoptera, and the commoner Lepidoptera. Several of the orchids contained aphides, and a few harboured a species of small red ant.

On April 21 I again made similar observations, selecting the same place, as it was most accessible to me, and because several orchideæ grew there in the greatest profusion. On this occasion I examined over thirty flowers—none of them the same as I had examined in March, for I had plucked these at the time, but there was not one that did not possess its pollinia, and, as on the previous occasion, none of them showed any appearance of having had pollen-grains in contact with their stigmas.

Yet the Tapada, during the spring, produces these orchids by the thousand, vigorous, healthy, sweet-smelling plants, and in sunny days its air is perfectly alive with insect life, as I have said, of every kind.

HENRY O. FORBES

Old Hall, Highgate, N.

New Meteor Radiant

THE "two remarkable meteors" closely following each other, observed by Mr. Hope on May 13 (NATURE, vol. xvi. p. 43), proceeded most probably from a shower in the small southern constellation Crater. Your correspondent describes the point of first appearance as a *little* south of Arcturus. If this means about 7° or 8° below that star, then the observed courses accord well with the centre indicated, which is apparently quite a new radiant. From the Italian catalogue of 7,512 meteor paths (observed by Schiaparelli, Denza, and others in 1872) which I have lately been reducing, I found this shower at R.A. 170°, Dec. 10° S. (near $\epsilon - \theta$ Crateris) for May 3—15 from nine meteors.

Ashley Down, Bristol, May 27 W. F. DENNING

OUR ASTRONOMICAL COLUMN

ANTHELM'S STAR OF 1670.—The small star which is very close to the position of this object, as determined from the observations contained in Lemonnier's "Histoire Celeste," deserves to be closely watched, as there is more than a suspicion of its variation within narrow limits. Thus in August, 1872, it was exactly equal to a star which follows it 12·5s. in R.A., 4′·9 to the north; while in November, 1874, it was as certainly fainter by half a magnitude. This small star precedes the variable S. Vulpeculæ 43·6s., and is north of it 2′ 11″; two other small stars follow less than 2′ from the parallel, 22·4s. and 30·6s. respectively.

Prof. Schönfeld found the place of Anthelm's star from the observations of Hevelius and Picard,

R.A. 19h. 41m. 37s. N.P.D. 63° 2′·3 for 1855·0.

The place of the suspicious star alluded to above is, for the same epoch,

R.A. 19h. 41m. 43·5. N.P.D. 63° 2′ 7″.

It was meridionally observed at Greenwich in 1872.

D'ARREST'S COMET.—As yet we hear nothing of observations of the short-period comet of D'Arrest, for which M. Leveau has worked so laboriously, with the view to facilitate its discovery at the present return. The intensity of light was at a maximum about May 22, but diminishes gradually during the summer. Nevertheless, early in August it is still of the same amount as when Prof. Schmidt discontinued his observations at Athens in December, 1870, at which time he stated he could have followed it longer but for the want of an ephemeris. Subjoined are the comet's calculated positions during the absence of moonlight in the present month, for Paris noon:—

	Right Ascension. h. m. s.	North Polar Distance. ° ′	Distance from the Earth.
June 7	1 18 58	85 26·2	1·613
,, 9	1 25 14	85 11·8	—
,, 11	1 31 27	84 58·2	1·611
,, 13	1 37 35	84 45·2	—
,, 15	1 43 40	84 33·0	1·609
,, 17	1 49 41	84 21·5	—
,, 19	1 55 38	84 10·7	1·607

Though the comet will not arrive at its least distance from the earth during the present visit (1·396) until October 20, it passed its perihelion on May 10. The period of revolution is now 2,434 days, or 35½ days longer than at its last appearance in 1870.

THE D'ANGOS COMET OF 1784.—Perhaps no person who has been occupied in astronomical observation and calculation has obtained for himself, rightly or wrongly, a more unenviable notoriety than the Chevalier D'Angos, who, in the latter part of the eighteenth century, was possessed of a small observatory in the island of Malta. From the unusual character of some of his statements his name came to be associated with anything in the way of observation that appeared to be apocryphal, and we find not only Zach was in the habit of terming doubtful assertions "*Angosiades*," but even Pastorff, who himself put upon record more than one suspicious statement, appeared to consider that he was establishing the good faith of an observation of a comet in transit across the sun's disc by declaring that it was not an observation made "à la D'Angos." We pass over on this occasion the reported observations by D'Angos in 1784 and 1798 of a comet or planet upon the sun, with the view to presenting the reader with a brief outline of the actual state of a case that has been open to still greater suspicion, viz., his observation and calculation of what appears in some of our catalogues as the second comet of 1784; and we may be pardoned for bringing together here particulars which though probably known to those who have access to a good astronomical library, are not so likely to be within the cognisance of those who cannot command such a collection. And further, it is almost essential to bring the main points in the case into one view, to enable the reader to judge for himself whether D'Angos is deserving of the opprobrium which has been cast upon him or not.

Writing from Malta on April 15, 1784, D'Angos apprised Messier at Paris that he had discovered a comet in Vulpecula on April 11, and he inclosed two approximate positions observed on the mornings of these days. He stated that the comet was very small, without tail, and with only "a slight appearance of nebulosity." Messier

did not receive this letter until May 14, when he sought in vain for the comet. Pingré who wrote before any suspicion had been raised with respect to D'Angos, attributed this to its having in the interval receded to too great a distance from the earth, or having attained too great south declination. It appears that Messier did not receive any further observations from Malta, but D'Angos some time afterwards communicated to him elements of the orbit, calculated by himself, and it was to be presumed with the aid of further positions. The observatory at Malta was burnt at a subsequent period, and the whole of the papers, &c., of D'Angos were stated to have fallen a prey to the flames, so that it was supposed in France that the observations were irrecoverably lost. Burckhardt had endeavoured by successive hypotheses to extract some idea of the nature of the orbit from the two rough observations which he had received, and as his results differed widely from those of D'Angos, and even the elements of the latter did not represent these observations, Delambre, at the instance of Burckhardt, wrote for further particulars. In reply, D'Angos stated that he had only saved from the fire his meteorological journal, in which, under date April 22, was mentioned an observation of the zodiacal light, without any reference to the comet, whence he concluded that on this date the latter was no longer visible.

This assertion will appear a most extraordinary one when it is stated that so far from the observations being lost, they had appeared in a memoir drawn up by D'Angos himself, in a periodical conducted by Bernoulli and Hindenberg, entitled—*Leipziger Magazin für reine und angewandte Mathematik*, Leipzig, 1786, where they were discovered by Olbers, as he mentions in a letter to Encke, inviting his discussion of them. Positions of the comet in longitude and latitude are there given for fourteen nights between April 10 and May 1, and they are followed by the elements of the orbit, which D'Angos says he had calculated from them.

Zach in 1812 had suspected that the observations of the second comet of 1784 were imaginary, and had suggested that the orbit should be omitted from the catalogues, but he adds as he had only great probabilities and moral, not mathematical, proofs to support his view, he did not insist upon it. To provoke an explanation, however, he states he had enveloped "ce mystère d'iniquité" in a problem in vol. iii. of his *Correspondance Astronomique*, where he printed a series of positions of a body, which he invited his readers to explain, and which puzzled Olbers and Bessel who failed, like others, to discover Zach's meaning. Burckhardt also on receiving intimation from Olbers of his having brought to light what purported to be the observations of D'Angos, remarked upon the importance attaching to the circumstance, since it might lead to proof that they had been fabricated.

It remains to describe in a future note or notes, the results of Encke's investigation and of later inquiries relative to the comet of D'Angos.

PROF. SYLVESTER ON TEACHING AND "RESEARCHING"

IN the address of Prof. Sylvester at the Johns Hopkins University, to which we have already referred, he spoke as follows on the above subject:—

Let me take this opportunity of making my profession of faith on a subject much mooted at the present day, as to whether the highest grade of university appointments should be conferred with or without the condition of teaching annexed.

I hesitate not to say that, in my opinion, the two functions of teaching and working in science should never be divorced. I believe that none are so well fitted to impart knowledge (if they will but recognise as existing, and take the necessary pains to acquire, the art of presentation) as those who are engaged in reviewing its methods and extending its boundaries—and I am sure that there is no stimulus so advantageous to the original investigator as that which springs from contact with other minds and the necessity for going afresh to the foundations of his knowledge, which the work of teaching imposes upon him. I look forward to the courses of lectures that I hope to deliver in succession within the walls of this university as marking the inauguration of a new era of productivity in my own scientific existence; nor need I consider any subject too low (as it is sometimes foolishly termed) for me to teach, when I remember to have seen the minutes of the conversation held between the delegates of the Convention, at the time of the French Revolution, and the illustrious Lagrange, the son of the pastry-cook of Turin, possibly the progenitor of the Marquis Lagrange, of turf celebrity (Citoyen Lagrange, as he is styled in the record), who, when asked what subject he would be willing to profess for the benefit of the community, answered meekly, "I will lecture on Arithmetic."

At this moment I happen to be engaged in a research of fascinating interest to myself, and which, if the day only responds to the promise of its dawn, will meet, I believe, a sympathetic response from the Professors of our divine Algebraical art wherever scattered through the world.

These are things called Algebraical Forms. Prof. Cayley calls them Quantics. These are not, properly speaking, Geometrical Forms, although capable, to some extent, of being embodied in them, but rather schemes of processes, or of operations for forming, for calling into existence, as it were, algebraic quantities.

To every such Quantic is associated an infinite variety of other forms that may be regarded as engendered from and floating, like an atmosphere, around it—but infinite in number as are these derived existences, these emanations from the parent form, it is found that they admit of being obtained by composition, by mixture, so to say, of a certain limited number of fundamental forms, standard rays, as they might be termed in the Algebraic Spectrum of the Quantic to which they belong. And, as it is a leading pursuit of the Physicists of the present day to ascertain the fixed lines in the spectrum of every chemical substance, so it is the aim and object of a great school of mathematicians to make out the fundamental derived forms, the Covariants and Invariants, as they are called, of these Quantics.

This is the kind of investigation in which I have, for the last month or two been immersed, and which I entertain great hopes of bringing to a successful issue. Why do I mention it here? It is to illustrate my opinion as to the invaluable aid of teaching to the teacher, in throwing him back upon his own thoughts and leading him to evolve new results from ideas that would have otherwise remained passive or dormant in his mind.

But for the persistence of a student of this University in urging upon me his desire to study with me the modern Algebra I should never have been led into this investigation; and the new facts and principles which I have discovered in regard to it (important facts, I believe,) would, so far as I am concerned, have remained still hidden in the womb of time. In vain I represented to this inquisitive student that he would do better to take up some other subject lying less off the beaten track of study, such as the higher parts of the Calculus or Elliptic Functions, or the theory of Substitutions, or I wot not what besides. He stuck with perfect respectfulness, but with invincible pertinacity, to his point. He would have the New Algebra (Heaven knows where he had heard about it, for it is almost unknown in this continent), that or nothing. I was obliged to yield, and what was the consequence?