

available for the *Gazette*, and that can only be done by increasing our membership, so do all you can to induce more and more teachers to join the Association.

One more appeal, and that is to the "highbrows"—do be kind to the writer of articles on elementary work. I am sure that the fear of highbrow criticism has, in the past, prevented some excellent teachers from giving to their fellow teachers ideas that would be valuable in the ordinary class-room.

I do not wish to end on too complacent a note: there is still much to be done to improve our mathematical teaching; but I hope that I have convinced you that there has been great progress in the work of all pupils up to the School Certificate stage—I must not go further than that after what was said by our president of last year—and that to this Association is due very much of the progress that has been made.

Last of all I should like to thank you for the patient way in which you have listened to my odd gleanings and reminiscences.

A. W. SIDDON'S.

P.S. There are two points that I should like to stress. In the early days of the A.I.G.T. there was a committee of the British Association considering the question of Geometry and the A.I.G.T. was much helped by the committee. Again in the early days of the Teaching Committee there was a British Association committee (of which Professor A. R. Forsyth was chairman and Professor John Perry secretary); the work of this committee was very helpful in securing the abolition of Euclid.

1037. Solution to double acrostic, Clearing 1030 (*Gazette*, XIX, December 1935), p. 342.

S	eirtennoe	G	O
I	emonstrate	D	F
D	ek a	F	R
D	rhocente	R	E
O	rhocente	R	E
N	ine point cent	E	E
S	ymmetr	Y	Y

(1). "Reflect."

(2). "Hailed" prohibits the insertion of other points. Unfortunately G. & S. themselves do not use O.

(4). "Converse." And we all know that slipshod reasoning is commonest in connection with converses.

E. H. N.

1038. Un peuple qui n'accorde pas aux mathématiques un rang élevé dans son estime, ne sera jamais en état de remplir les plus hautes tâches civilisatrices et de jouir, par suite, de la considération internationale qui, elle aussi, constitue à la longue, un moyen efficace de conserver notre situation dans le monde et de sauvegarder notre droit à vivre notre propre vie.—From Mittag-Leffler's will, founding the international institute at Stockholm; *Nature*, July 6, 1926, vol. 97, p. 384. [Per Prof. H. G. Forder.]

OLD TRIPOS DAYS AT CAMBRIDGE, AS SEEN FROM ANOTHER VIEWPOINT.

By KARL PEARSON.

NOTHING can convince us more that "things are not what they seem" than a comparison of the descriptions by two men of what they remember of Cambridge in the 'seventies! I should of course recognise my friend Professor Forsyth's picture of the Cambridge of that time and am grateful for his calling to my mind so many memories of the past. But for me he has painted them in far too drab a colour, so that one wonders whether his undergraduate days could have been like mine some of the happiest in his life. The reading of his paper enforces upon me the truth of the principle that environment and personalities are not things in themselves, but are our own "construxts"—each of us giving our own "reality" to such evasive phenomena.

Reading Professor Forsyth's paper, one might suppose that, for the mathematical student of the 'seventies, Cambridge life was a dull grind for more than three years under a non-inspiring "coach"; with a vampire examination hanging over us and sucking out our youthful blood—the terrible strain of the so-called Tripos. Well, that may have been the case for some who worked for place and reputation, but surely the vampire conception of the Tripos arose from some of the men who entered for it, rather than from the examination itself? In my personal view the Tripos of those days was an excellent examination, and this for two reasons: First, because it was *not* specialised, but gave a general review of the principia of many branches of mathematical science, and, secondly, because the weight given by it to "problems", enforced "problems" on the teaching coaches. Every bit of mathematical research is really a "problem", or can be thrown into the form of one, and in post-Cambridge days in Heidelberg and Berlin I found this power of problem-solving gave one advantages in research over German students, who had been taught mathematics in theory, but not by "problems". The problem-experience in Cambridge has been of the greatest service to me in life, and I am grateful indeed for it.

Now let me return to those who constructed a vampire out of the Tripos. Like Forsyth I was one of a class of some six or eight freshmen sitting at Routh's feet in 1875, and I wanted to know something personally of these comrades, so I asked one, in our second or third week, to come to breakfast with me. He replied, very politely refusing, because he was too busy working for the Tripos! That is what I have termed the vampire construct of the Tripos examination. It was very nearly the same with the others. They had come up—in several cases after degrees elsewhere—for the Tripos and the Tripos only, not in order to widen their minds from what Cambridge could furnish in all directions, not to leave Cambridge a little better than they found it. The "coaching system" pro-

vided ample time for other lectures and other reading—advice for the latter was always available—and if you did enough mathematical reading for yourself, appreciated your coach's efforts, and were content to be a wrangler, I don't think the vampire element came into your idea of the Tripos. These facts may show that I differ widely in my "construct" from Professor Forsyth's :

"We were drilled in the gymnastic that led to swift answer according to rule and pattern. In the examination there was no leisure to think : even during our training there had been little leisure for thinking, because we always were being taught ; and independence in reading was almost a misdemeanour in the eyes of some coaches."

Let me question another point in Forsyth's account :

"With the production of the Tripos list the association of coach and pupil ended. They passed out of one another's lives ; and the coach returned to the same round of drill with the pupils who were to go through the final mill" (p. 178).

In my case at any rate this seems to give a very wrong impression of "coach" and "pupil" ! Let me state my experience. Owing to reasons of health I had been withdrawn from school at sixteen, and spent my seventeenth year with a private tutor who had been Senior Wrangler and a tutor of Trinity. He was not a bad teacher, but unable to manage a party of young men, who did not want to learn and whose follies easily developed into vices. Perhaps the most useful knowledge I acquired from him was an appreciation of Dynamics, and from his pupils an acquaintance with dog-breeding which has been useful to me in after-life. At the end of the year I persuaded my father to let me go up to Cambridge and work as a "beast" under Routh. I knew nothing of Routh, but I called upon him in his quarters so graphically described by Forsyth. He was covered with chalk, had his coat off and his shirt sleeves rolled up, with the sponge and duster in his hand, which were so familiar to his pupils. He was in a great hurry between two lectures, but he looked at me and said, "Come to me at seven o'clock to-morrow morning", and we parted.

At seven o'clock I went and found another "beast" there, "Josh" Conway (now Lord Conway of Allington). Routh said to us : "You have a year before entering college, we will devote it to reading subjects not of first-class importance for the Tripos", and he started straight off to lecture on the theory of elasticity. Conway dropped away after a term, and Routh took me alone at seven o'clock in the morning throughout a year, introducing me to Lamé's works and papers by other writers. There was no drill, no attempt to cram. He never referred to the scholarship examination, on which much depended, but left me to work that out for myself. I think Routh had a real affection for me, as I certainly had for him. I was invited several times to his house during my Cambridge career, and never failed afterwards on my rare visits to my Alma Mater to pop in and

shake hands with him at Peterhouse ; and years later when my children went up to Cambridge his widow was kindness itself to them. I think others of Routh's pupils could bring evidence to show that he had in them an interest which extended beyond the Tripos list.

Nor can I agree with Professor Forsyth when he says :

"The imaginary i was suspiciously regarded as an untrustworthy intruder. The complex variable (a phrase that had not then penetrated to Cambridge) was described either as imaginary or impossible" (p. 172).

On the contrary, I heard of the "complex variable" both from Burnside and Frost as well as from Routh himself, who used to give a clear if an elementary account of it. Beyond this, some of us, who in those years read the *Messenger* and *Quarterly Journal of Mathematics*, were confronted with its physical applications and did due honour to its possibilities. I cannot say that anybody directly advised me, but some one or other of my teachers led me, well before my Tripos, to other works of Salmon than his conic sections ; there was $T+T'$ (not T and T' be it noted !), the first volume of Lord Rayleigh's *Sound*, Green's Papers, Clerk Maxwell's great volumes, De St. Venant's edition of Clebsch and his own papers, there was a book on modern geometry by Townsend, Ball on *Screws* (which was undoubtedly recommended by Routh himself), and many more, if I were in my library and could examine the dates of purchase. Besides Lamé's four works and papers by Fresnel, there was Fourier's treatise and undoubtedly other French books. In German, Kirchhoff's *Mechanik*, Heine and Neumann on spherical harmonics and Bessel's functions, Dürge's *Elliptische Functionen*, and a work by Riemann whose title escapes my memory. I do not refer to a list which might be considerably lengthened to show that undergraduate reading extended beyond coach's lectures, but to prove that Cambridge teachers three years before Forsyth went up led their students to read foreign literature. I started at Cambridge with no acquaintance with the standard works of mathematics, and my knowledge at least of their titles must have come from my teachers, Routh, Burnside and Frost. Hence I cannot again agree with Forsyth when he writes :

"But all such ventures were exceptional and rare : there was no advice, no wish, no leisure, to urge us on those paths. The coach was the autocratic director, often the sole director" (p. 175).

I contend that there was ample advice, ample desire, and, above all, ample leisure for those who put aside that notion of the Tripos place being their one object for three years of their life ! Talking about German mathematical works, the door to their study was opened to me by a delightful old German, Steinhilfer, who read Heine's *Reisebilder* with me in my first term. His death soon after left his widow penniless, and this brought me into slight contact

with a big personality, Henry Jackson of Trinity. Another great figure of that time was Minro. I only once came in contact with him. There was in those days a curious habit in Trinity of examining undergraduates of all years for foundation scholarships. Conway and I sat for the examination, but we had to sit for both mathematical and classical papers. One of the latter was a Latin composition paper set by Minro, who was walking up and down the hall. He saw me idle, and he came up and entreated me with almost the tenderness of a father to write a few Latin verses! I would have done anything to comply with his request and please him, but I got only once as a schoolboy been set to make verses and then—I got my uncle to make them for me. That was one experience of my Trinity examination. Another was this, there was a mathematical paper, of which about a third was occupied with questions as to certain mysterious $P_n(x)$'s and $Q_n(x)$'s. These were defined by differential expressions, and the examinee was asked to prove numerous propositions with regard to them. I was much struck by the ingenuity of the examiner who could invent such interesting functions and deduce such remarkable properties of them. The questions fascinated me and I devoted my chief attention to them. Some week or more after I had a letter from Glaisher stating that the examiners rejected me, considering that I had devoted too much time to the higher branches of mathematics, neglecting the fundamental subjects. I thought the examiners must have heard of my reading Lamé's works with Routh; only some time later did I discover that I had been answering bookwork questions on Legendre's functions of which I had never heard previously. I have since held that there were worse examinations in Cambridge in 1875 than the Mathematical Tripos!

I had probably nothing like the same acquaintance with the mathematical leaders in Cambridge of the 'seventies as Professor Forsyth had, for I left Cambridge for Germany shortly after my Tripos and did not return permanently to it. But some little account of my contact with one or two of them may be of interest. Our college lecturer in mathematics was a dear old boy, Percival Frost, but he had to lecture down to the lowest individual in the class, and accordingly his lectures were of no value to Routh's pupils. But the college law was inexorable; every undergraduate *must* go to college lectures. At last a compromise was arranged: I should go to Frost's private house twice a week for an hour. He had a large study; on one side were a good collection of standard mathematical works, on the other at any rate an equally voluminous set of theological writers. "Good morning, Mr. Pearson, what difficulties shall we discuss to-day?" "Good morning, Mr. Frost, can you explain to me this sentence in Tillotson's sermons?" Then followed a theological discussion for at least half an hour. Frost, with a twinkle in his eye: "You are as aggravating as Kingdon Clifford; aye, but he was a mathematician! The best pupil I ever

had. But last Wednesday we were talking about geodesics; it is time to get back to them." Perhaps we did, but oftener we didn't. Spinning billiard balls round silk hats, and the theory of doing so, was his delight, and in vacation time he might be induced to enter the billiard room of an inn and display his prowess. His solid geometry and his wonderful book on curve tracing were excellent treasures in my day. His *Newton* only gave one an appetite which failed to be satisfied in my time at Cambridge. He was also a good hand at backgammon. Once I asked him why he always beat me. He replied: "I always calculate the probability before I make a move. I don't think you do." One day he said to me: "Mr. Pearson, I have a nice mathematical problem for you. When I have shaved, I drop the shaving paper into a certain article, but it never will fall in. Can you account for it?" That was my first problem in aeronautics, and, of course, I failed to solve it; and then the mild twinkle came into his eye, and from his mouth: "Aye, but Clifford would have solved it; he was a mathematician." I cannot bring Frost under Professor Forsyth's description of the "autocratic coach."

With Adams my contact was small, but perhaps worth recording. I think in my second term an Italian, Signor Nathan, came to lecture on Dante in Cambridge. There were then no Italian teachers, no *Modern Languages Tripos*. On the first day there was a fair audience, but after the third lecture only Adams and I were left to keep Nathan in countenance. It was probably unnoticed by Adams; he would be used to small audiences. My next meeting with Adams was of a more painful kind. I held the respect for him current in our time at Cambridge. One day in vacation time I was playing tennis in the grounds of an hotel at St. Ives, Cornwall, when up the drive came an old-fashioned pair followed by a porter carrying one large skin-covered trunk on his back—it was all a little out of date even fifty-eight years ago. But I was thrilled; here *was* a chance! I returned to my game, but I had hardly served when I saw the back of the porter, the hairy trunk and the old-fashioned pair, obviously tired, retreating down the drive! I threw down my racquet, for I knew there were vacant rooms in the hotel, and rushed to the manageress. "What have you done?" I cried; "you have turned away the discoverer of Neptune." "Neptune or no," she replied, "I am not going to have dowdies like those in this hotel!" Such is the fate of genius if it does not put on its best clothes when it enters a big hotel.

Professor Forsyth tells us little about the Smith's Prize Examination, which was in those days very characteristic of the best in Cambridge mathematics. Four professors examined, in my year Stokes, Clerk Maxwell, Cayley and Todhunter, in place of Challis, the Professor of Astronomy, who was ill. We went on each occasion to the examiner's house, did a morning paper, had lunch there, and continued our work on the paper in the afternoon. At Stokes' we had a family lunch, the professor at one end of the table and his

wife at the other, also a daughter and I think a son were present—I may now admit a silent admiration for that very beautiful girl, she frequently came to King's College Chapel Services, and had considerable attraction for the scholars in the stalls. Professor Forstyth may be interested to know that one of the questions put by Stokes was, "Write an essay on the theory of functions", or words to that effect. For Stokes I held a great veneration. His wonderful look of satisfaction when an experiment turned out successfully, and it generally did, is not really describable; it was as if he had made for the first time a most important discovery. A few only of Routh's pupils of my year went, and none but I attended the continuation of his lectures into a second term. "It did not pay for the Tripos." I have only met one lecturer as good as the Stokes of those days, namely, Quineke of Heidelberg. He planned his course so as to fit his topic to his time, and never finished his hour and his lecture in the middle of an experiment. The first words almost he said to me were: "From Cambridge? Stokes is there; the greatest mathematical physicist in England." Years later I was standing by Quineke at the Stokes Jubilee when Lord Kelvin made the remarkable statement that Stokes' room in Pembroke was the first physical laboratory in Europe. Quineke murmured, "Franz Neumann", and I could only nod assent.

At Clerk Maxwell's we did our papers in the dining-room and adjourned for lunch to an upper room, probably the drawing-room, where Clerk Maxwell himself presided. The conversation turned on Darwinian evolution; I can't say now how it came about, but I spoke disrespectfully of Noah's Flood. Clerk Maxwell was instantly aroused to the highest pitch of anger, reproving me for want of faith in the Bible! I had no idea at the time that he had retained the rigid faith of his childhood, and was, if possible, a firmer believer than Gladstone in the accuracy of Genesis. His books, from the little treatise on heat to the revelations contained in his two volumes on electricity and magnetism, were splendid; but he could not lecture. He seemed to have no planned course; and after three weeks of his course on heat, I beat a retreat. It was not solely because "the lectures were no good for the Tripos", but partly because I grew weary of seeing his demonstrator standing, with his thumbs in his waistcoat pockets, almost behind the professor as the latter struggled to boil water. However, his Smith's Prize paper was a treat which we could all enjoy.

The next day we went to Cayley's. His first words were, "Throw off your gowns, gentlemen, you will work more easily without them", and accordingly they were dropped in a heap in a corner of the room, and we set to work unnumbered. Of course I knew nothing of the topics of Cayley's paper. My chance of scoring marks in the Tripos had depended only on my applied mathematics, and my pure mathematics were but sufficient to help in the former branch. But I took things leisurely, as if nothing depended on speed, and worked as one might work in solving crossword puzzles on a train journey.

Cayley did not appear at lunch; sandwiches, biscuits and other light refreshments were brought up on a tray, accompanied by a decanter of excellent port wine; Cayley had not spared his cellar. After sampling a glass, I tried to persuade my co-examinees to do so likewise; two, I think, took a dribble, but the future Smith prizeman, speaking from his conscience, refused—he was true to what he had originally said in our first term. He had come to Cambridge for examination ends; perhaps he thought I was tempting him to drop the prize already well within his grasp. Back we went to our writing, I feeling the better for Cayley's port, and the others satisfied in their consciences that they had done the right thing under examination stress. Cayley evidently did not think good port at all incompatible with the discussion of invariants or higher algebra. A few days later a friend of Cayley's told me that Cayley had remarked that there was only one man who appeared to have thoroughly enjoyed his paper—it was the one man who thoroughly enjoyed his port. Somehow that commendation was more to me than if I had won a Smith's Prize or gratified Routh or my college in being senior.

The last day we went to Todhunter's. I do not think any of us knew more of him than what we had gathered from his textbooks, or, perhaps in a few cases, from his two histories. He held no teaching position. What we did not know was that he was a strict disciplinarian. He came into the room with his papers in his hand. He stood aghast, the papers fell from his hand—mindful of the greater Cayley's permission, we had dropt our gowns in the corner of the room! "Put on your gowns, gentlemen, at once; this is an unheard-of irregularity." Crestfallen, we resumed our academic costume, but with us Cayley was reckoned still higher in the scale of Cambridge worthies than ever before! But Todhunter's paper strangely enough made a turning-point in my career. There was a question in it on either the torsion or flexure of prisms which I answered, having read De St. Venant's memoirs. I thought I had answered it as the author himself had done. But it was not so. Some few years afterwards I was asked by the Syndics of the Press to finish and edit Todhunter's *History of Elasticity*. I accepted, but had not the least idea of how it came about. When I got the manuscript, however, I found a considerable number of pages of my Smith Prize paper had been incorporated into it by Todhunter, with the pencil remark that the proof was better than De St. Venant's. It had evidently been seen by the referees! Thus from that day with its "unheard-of irregularity" started my link with Todhunter, and to me a more vital association—that with the University Press, whose familiar proofs have for some fifty-five years scarcely ever been missing from my writing table!

I have endeavoured to indicate how in the Old Tripos Days at Cambridge it was possible to enjoy thoroughly university life without thinking of the Tripos as a vampire limiting all one's freedom. It was not the fault of the examination that some men became

slaves to it, played no games, read nothing but examination mathematics, came in contact neither with the minds of the better teachers, nor widened their own minds by general reading or interchange of thought with their younger contemporaries in other branches of study. Has the abolition of the Senior Wranglership and the division of the Tripos into specialised sections improved matters? I very much doubt it. I know that, when I had to seek for assistants from Cambridge, I often came across my old friend, who had spent his time there working solely for examination, and, further, I found that owing to his specialisation he could rarely lecture on two heads of miscellaneous mathematics. It was, and I still think is, the fault of the colleges, who look only to marks in examination papers. Even if they do elect a certain number of such single-idea'd scholars, they ought to humanise them, drive out of them the commercial view of the university career as a step to obtaining a livelihood, and force them to take part in the general college and university life. It is needless to say that I am not looking at Professor Forsyth as having been an undergraduate of that type; his account of his own mathematical reading shows that he was not. But I want to protest against his view of the Old Tripos. If it played the vampire to some men, it was partly their fault and partly the fault of the college tutors.

Professor Forsyth may say that I was singularly fortunate. It is true that I belonged to a small community, some thirty-two members in all, of whom a moiety at least were Etonians. So small was our number that the rules of the press-gang had to be applied to the dons in order to man the boat. But our very smallness brought us in far closer community with the dons than was possibly customary in other colleges, and several of them helped much to humanise us cubs. There was Oscar Browning—very good for freshmen in their first or even second terms—he introduced me to Rousseau, Goethe and Italy. There was G. W. Prothero, a man who could hold the balance between the old and the new, between the mediaeval provost and the hustling undergraduate. But the man who most influenced our generation was Henry Bradshaw—best for a man in his third year. Those who had made his friendship would soon lose all conception of a vampire Tripos examination!

Of course, there were other dons of the old school who could not accept the transition that the college was undergoing, and they had to be fought, which was an additional pleasure in life. There were then compulsory divinity lectures dating back to the foundation of the college. After sitting through lectures on the Thirty-Nine Articles and the Prayer Book from a lecturer who did not throw as much life and light even into the latter as the *Interleafed Prayer Book* does, I asked the tutor if I might go to Westcott's lectures instead. After some demur he permitted me, and I sat among the divinity students for a term listening to a most learned but scholastic discourse on whether Christ would have come to earth if Adam had not eaten the apple. Westcott concluded that he would; it was

needful for the perfection of the Godhead. I felt inclined to ask him, "For whose sake?"; but feared he would think it ribald. However, that was enough; I went to the tutor and said, "I am not going to attend any more divinity lectures". He looked agast. "It is an inexorable rule of the college." "Then I am going to another college." "It is unheard of. We shall not give you a *bene decessit*." "Mr. Ferrers of Caius has already agreed to accept me without a *bene decessit*." "This matter must come before the College Council." It did come, and after a hard fight compulsory divinity lectures were abolished. The matter was not ended. The tutor came to my rooms—like Nicodemus, by night—"You have had your way, but it will break the discipline of the college for a scholar to do this. I should like you to sit for an examination in Hooker's *Ecclesiastical Polity*." "That is to hang a man, after capital punishment has been abolished." However, I agreed smilingly to read the judicious Hooker's book if I were not examined on it; it might be interesting. I kept my promise, but I don't think I drew so much amusement and profit from Hooker as from Paley. I only once later came in touch with Westcott. I have said that our community was a small one, and I sympathised a good deal with a man who was the very first student of the "Moral Sciences" in the college. As a freshman, he was rather in trouble; the college tutor had told him that while he had been compelled to get up the conditions of the Mathematical Tripos, he could not be expected to do so for the Moral Sciences Tripos. As a matter of fact, till the irruption of non-Etonians, he had never had to busy himself with anything but the Classical Tripos. Well, the student of the Moral Sciences and I fraternised, especially over Spinoza, and before I finally broke the link with Cambridge I had written in the *Cambridge Review* notices of Pollock's and of Martineau's books on Spinoza, as well as a paper on Maimonides and Spinoza in *Mind*. At a college meeting I again met Westcott. He said to me sorrowfully: "Ah! when I was a young man, I also admired Spinoza." A reply was on my lips, but I refrained. I can only say that till this day I think Spinoza the sole philosopher who provides a conception of the Deity in the least compatible with scientific knowledge.

Of a certainty there were many occasions for action, mostly of a profoundly interesting kind in Cambridge in the Old Tripos Days, even for the mathematician. Compulsory divinity lectures had disappeared, it was time for compulsory chapels to go likewise. I went to the tutor and told him that I objected to compulsory chapels. "Are you a dissenter?" he asked. I could not say that I was, but I appealed to the Test Act. "Well, you are not yet nineteen, and that is a matter for your father to settle." I was sorry to drag my father into the affair, but if one could not settle one's own religion at nineteen years of age, one had to do so. He signed the document of protest much as he would have signed one of his legal opinions or a cheque. He was not a good churchman; he

spent most of his Sundays in what he termed "helping the ass out of the ditch". Armed with the requisite document, I was freed from chapel attendance. On the following Sunday, to the astonishment of the tutor and deans, I appeared in my usual stall in the chapel. The next morning I was summoned to the dean's rooms: "You demanded to be released from attendance at chapel, and you were there yesterday!" "No, Sir, I asked to be released from compulsory attendance at chapel, and I hope to be there when the spirit moves me." Thus I went when I liked after that; nothing further was said to me. But the situation was impossible. Soon after that compulsory chapels were abolished, and I felt I had done my bit towards modernising that great college in my undergraduate days. No doubt some will say that what I attempted was not true progress. That is not my point: I do not wish to pose as a reformer. I want to point out that in the Old Tripos Days at Cambridge it was possible for an individual thoroughly to enjoy his undergraduate career, to read mathematics outside the Tripos range, and a good many other subjects as well, to find "coaches" who led him on and grew to be friends, to find college authorities who on the whole had some sense of humour and were not wholly upset by his want of respect for the "conventions", and lastly, and best of all, to have no vampire Tripos conception hanging over him for more than three years. There was pleasure in the friendships, there was pleasure in the fights, there was pleasure in the coaches' teaching, there was pleasure in searching for new lights as well in mathematics as in philosophy and religion. I think I carried away from Cambridge my full share of the benefits it can bring to any of its sons. Possibly, if I had stayed in Cambridge, the memory of my undergraduate time would have lost some of its sunlight. My "construct" now of "Old Tripos Days at Cambridge" differs widely from Professor Forsyth's. Is it the different natures of the constructors which makes their description of what they experienced so different? Or is it that friend Forsyth is really defending the reforms—for which he was so largely responsible—and which destroyed for ever the venerable Cambridge Mathematical Tripos with its value as a general education? To-day the Cambridge Tripos produces mathematical specialists, but hardly as many men of distinction in all branches of State service.

The Old Schoolhouse, Colharbour,
September 20th, 1935.

K. P.

1039. De Philosophis.

Hic Diogenes olim patria expulsus erat quia nummos adlucinos fecerat; itaque malus philosophus erat. Nam boni philosophi (ut supra demonstravimus — omnes boni scriptores supra demonstrant) nunquam utile quicquam faciunt.—G. M. Lyne, *Baldus*, p. 17. [Per Mr. V. Naylor.]

SOME GEOMETRICAL APPLICATIONS OF VECTORS.

By H. LOB.

THE aim of the following notes is merely to indicate the straightforward way in which vectors lend themselves to geometry. Some of the work is less direct than it might be, but this is partly due to the desire to keep to first principles as much as possible.

The chain of theorems in § 6 has been suggested by a chain constructed many years ago by Mr. H. W. Richmond. In this, points are taken on a unit circle and the second unit circle through each pair of points is drawn; the centres give rise to a chain of unit circles.

1.1. Relation between mutual distances of 4 points in a plane.

Let O, A, B, C be the four given points.

Take $\overline{OA} = \alpha, \overline{OB} = \beta, \overline{OC} = \gamma$.

Then, since the vectors are in one plane,

$$\gamma = l\alpha + m\beta,$$

where l, m are numbers.

Taking scalar products of γ with respect to α, β, γ in turn, we have

$$\left. \begin{aligned} l\alpha^2 + m\alpha \cdot \beta &= \gamma \cdot \alpha \\ l\alpha \cdot \beta + m\beta^2 &= \beta \cdot \gamma \\ l\alpha \cdot \gamma + m\beta \cdot \gamma &= \gamma^2 \end{aligned} \right\}$$

$$\left| \begin{array}{ccc} \alpha^2 & \alpha \cdot \beta & \alpha \cdot \gamma \\ \beta \cdot \alpha & \beta^2 & \beta \cdot \gamma \\ \gamma \cdot \alpha & \gamma \cdot \beta & \gamma^2 \end{array} \right| = 0.$$

The scalar products can all be put in terms of the mutual distances of O, A, B, C .

E.g. if $OA = a, OB = b, OC = c, BC = p, CA = q, AB = r$, we have

$$2\beta \cdot \gamma = 2bc \cos \hat{BOC} \\ = b^2 + c^2 - p^2.$$

1.2. Relation between mutual distances of 5 points in space.

Let O, A, B, C, D be the points.

Take $\overline{OA} = \alpha, \dots, \overline{OD} = \delta$.

Then $\delta = l\alpha + m\beta + n\gamma$.

Hence

$$\left. \begin{aligned} l\alpha^2 + m\alpha \cdot \beta + n\alpha \cdot \gamma &= \alpha \cdot \delta \\ l\alpha \cdot \beta + m\beta^2 + n\beta \cdot \gamma &= \beta \cdot \delta \\ l\alpha \cdot \gamma + m\beta \cdot \gamma + n\gamma^2 &= \gamma \cdot \delta \\ l\alpha \cdot \delta + m\beta \cdot \delta + n\gamma \cdot \delta &= \delta^2 \end{aligned} \right\}$$

Thus

$$\left| \begin{array}{cccc} \alpha^2 & \alpha \cdot \beta & \alpha \cdot \gamma & \alpha \cdot \delta \\ \alpha \cdot \beta & \beta^2 & \beta \cdot \gamma & \beta \cdot \delta \\ \alpha \cdot \gamma & \beta \cdot \gamma & \gamma^2 & \gamma \cdot \delta \\ \alpha \cdot \delta & \beta \cdot \delta & \gamma \cdot \delta & \delta^2 \end{array} \right| = 0,$$