OLD CAMBRIDGE DAYS

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The town of Cambridge is a rather insignificant little place situated some fifty miles to the north of London. Its only title to distinction is, and always has been, that it happens to be the site of a university. Why this should be so is a complete mystery; the historians have never been able to throw any light on it and probably never will. But how the university actually came about—that is known with some precision. The earliest universities of Christian Europe were all born in the same way: a great scholar settled in a town and attracted some students to his lectures; these disciples then stayed with him and, in their turn, began teaching the young men who continued to arrive; and so, without any forethought, the thing was done. Thus the University of Bologna, the first of the great European foundations, came into being simply because some of the local monks began giving public lectures on Roman law: this was about 1150. Today, in one of the principal squares of Bologna, there is a monument to those men who were in effect the first university lecturers of medieval Europe. (Throughout the centuries, many of Italy’s greatest advocates have received their training in the Bologna law school.) The universities of Paris and of Oxford were both founded about fifty years later, in exactly the same way.

Now the universities of the second wave of foundations arose in a quite different manner. When learned men have been in one another’s company for a sufficient length of time they usually begin to quarrel. From the highest motives, naturally: doctrinal questions, difficult philosophical points, and the like. In those early universities, the disputes sometimes grew so acute that the entire body of scholars split into two factions; and then the dissenting party would go away and found a rival institution elsewhere, just as a swarm of bees will leave the parent hive. Several French and Italian universities were founded in this

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L. Roth, born in London in 1904, was educated at Clare College, Cambridge, England. He graduated in 1926, a “Wrangler” in the Mathematical Tripos, and was appointed at the Imperial College of Science and Technology at London University. With few interruptions, he worked there until 1967, first as Lecturer and then as Reader in Pure Mathematics.

A pupil of H. F. Baker, he was awarded a Rockefeller Research Fellowship at Rome University in 1929, where he established lasting and fruitful relationships with Italian geometers. In fact, his original work is almost entirely devoted to algebraic geometry following the methods of the Italian school. His main contributions concern birationality and unirationality questions on algebraic manifolds, as well as Abelian, pseudo-Abelian, and group varieties, and are of lasting significance. A complete list of his publications, more than 90 in number, which will be found at the end of an obituary by E. G. Togliatti appeared in Boll. Un. Mat. It., (4) 3, 1970, pp. 326–332.

Leonard Roth was a very good lecturer and had a deep and widespread knowledge of the arts, music, and literature. His many-sided gifts and his charm are only partially apparent from the following paper found among other MSS left by him. It was probably not written for publication. Those who had the privilege of knowing him will always recall his more intimate endowments: his unusual kindness, unpretentiousness, and deeply-felt humanity. B. SEGRE.
way, by swarms from Paris and Bologna respectively. And Cambridge was founded, about the year 1200, by a swarm from Oxford.

Incidentally, this last migration had a curious aftermath. Until fairly recently, anyone who proceeded to a master's degree at Oxford had to sign a document affirming that he would never in any circumstances lecture to the little town of Stamford (Stamford, by the way, is on the road from London to York). The reason for this procedure was that some time later, a second migration left Oxford and set up a new center of learning at Stamford. However, the project came to nothing, and the dissentients soon returned home to Oxford. But the university authorities were so alarmed by these two flights that they took steps to prevent any more.

And so, by the end of the thirteenth century, we find two university institutions thriving in England, and situated—by Continental standards at any rate—almost within stone's throw of each other. However, although so near on the map, the climatic conditions which they enjoyed—or endured, as the case may be—were very different indeed. Oxford has a mild winter and is rather enervating on the whole. But Cambridge is much worse off; and in the early Middle Ages, when it lay on the edge of a huge undrained fen, the weather there must have been truly horrible. Even today, with its cold and damp winter, aggravated by a wind blowing across the European plain, it is nothing to joke about.

Now very soon after Oxford and Cambridge were founded they were confronted with serious problems of discipline. The presence of a large body of young men, in every respect alien to the local townsfolk, led to frequent disorders, the so-called "town and gown" riots, which often culminated in a murder. The situation was particularly galling to the magistrates because of the immunity frequently enjoyed by priests and clerks. This still persists in some institutions: thus, in Bologna university, if one student commits an offense against another which is normally punishable by law, the police are powerless to intervene. The authorities of both English universities sought a solution to the problem by putting their students into halls of residence, which later became known as colleges. The Head of the House (as he is still officially designated at Oxford) or Master of the College, to give him his usual modern title, was endowed with considerable powers over the inmates. His second in command was known as the Father of the College; he was in loco parentis—and he is the ancestor of the modern Dean. We shall soon see what his role was in the Cambridge scholastic world.

We may remark in passing that scarcely any other university saw fit to follow the example set by Oxford and Cambridge; this is exceedingly odd since one and all were beset by the same disciplinary problem. At both the English universities the college system gradually brought about fundamental changes in the administration. As the centuries passed, the central authority lost more and more to the individual colleges, until the latter became very nearly autonomous bodies, each of them responsible for the supervision and teaching of their own
students. This shift in power was to have significant consequences for the study of mathematics in Cambridge, which is our main theme here.

There was, however, one important right which the University still retained: that of holding the degree examination. This took the form of a disputation of strict syllogistic character, in which no other kind of reasoning was permitted; it was a three-cornered affair—examiner versus candidate, with the Dean acting as buffer between the contestants. Whenever his "son" was held up for a syllogism, the Dean would slip one in; if the candidate was unable to Cargo the argument, the Dean would come to his assistance. One beautiful feature of these medieval examinations was that no candidate ever failed; he might do very well, in which case he received the distinction summa cum laude, or his performance might be execrable. But in any case, he was awarded his degree; that is what Deans were for in those days.

Echoes of this ancient ceremony are still to be heard in the Cambridge examination system of today. Because the parties to the dispute used to sit on three-legged stools, the Cambridge examination is called a Tripos; moreover, in memory of the disputation, everyone who gains a first class in the Mathematical Tripos is designated a Wrangler.

In those remote times there was no hint that mathematics was destined to play the predominant part in Cambridge university studies which it was later to be assigned. The syllabus followed the lines of the normal medieval curriculum; geometry (but not mathematics), logic, philosophy, music, with specialist courses for students of law, divinity, or medicine. We have to wait until the mid-seventeenth century for the emergence of mathematics as a major discipline, that is to say, for the period in which Isaac Barrow occupied the mathematical chair.

How important is Barrow in the history of seventeenth-century science? The answer given to this question depends largely on one's nationality. The fact is, most of the European nations have their own horse in the Calculus Stakes: thus the French have Fermat and the Italians have Torricelli; if the Russians have not yet entered a candidate, it is only a question of time before they do. Any Englishman would, I fancy, put his money on Barrow; and there are sound, not patriotic, reasons for such a choice. It seems, on reading Barrow's lectures, that he has come nearer to the general notions of derivative and integral than have any of his predecessors. Now Newton was Barrow's pupil, and he absorbed Barrow's ideas.

Newton is, of course, the greatest of all Cambridge professors; he also happens to be the greatest disaster that ever befell not merely Cambridge mathematics in particular but British mathematical science as a whole. This is a fact which the historians do not care to dwell upon; if they mention it at all, they rarely give it due emphasis.

When Newton succeeded Barrow in the Cambridge chair, he had already lost interest in mathematical studies and had turned his intellect into theological and speculative channels. Moreover, he became caught up in public affairs as
well. When William and Mary came to the throne in 1688, the British coinage was in a terribly debased condition, and it was essential to commerce that this should be remedied as quickly as possible. For some years the government shrank before the magnitude and peril of the task; in the end, however, the re-coinage was decided upon: Newton was called to London as Master of the Mint, and under his supervision the operation was performed with complete success.

These aspects of Newton's career—his indifference to mathematics and his absenteeism—may be termed his negative contributions to the ruin of British science. His positive contribution was far more serious: I refer to his quarrel with Leibniz concerning the origins of the infinitesimal calculus. Throughout his life, Newton shrank from every kind of controversy: by a strange irony his very reticence landed him in the bitterest dispute in mathematical history. The main facts are so well known that I need only allude to them briefly. For some time, there had been subterranean rumblings in the hitherto cordial relations between Newton and Leibniz. In 1715, however, matters came to a head; there then appeared a lengthy historical review of the work done in this field by the two rivals. The article was anonymous, but it is now generally believed to have been written entirely by Newton himself. After surveying the whole situation apparently with complete objectivity, it ends by summing up carefully but decisively against Leibniz, virtually accusing him of having stolen Newton's ideas. At this the Continental mathematicians immediately leapt to Leibniz's defense; at the same time the British mathematicians ranged themselves behind Newton. And there, for over a century, they stayed; their patriotism and solidarity were manifested in a boycott of European mathematics—and, most regrettably, during the very period when the modern science was in its full flood of development. So the Great Sulk went on; and the work of the Bernoullis, of Euler, Lagrange, Laplace, Gauss, and Cauchy remained for Britons a dead letter.

But this is not the whole story by any means; even when the period of official isolation (so to speak) was over, its disastrous consequences continued to make themselves felt. Although during the nineteenth century British applied mathematics made spectacular strides, pure mathematics was more or less neglected and, with the exception of Arthur Cayley, Great Britain produced no pure mathematician of the highest rank. It was not until the beginning of the twentieth century that research in pure mathematics, worthy of international repute, began once more to be produced in any considerable quantity. We shall have more to say of this shortly; in the meantime we continue with our story of Cambridge.

At both Oxford and Cambridge, the eighteenth century was a period of stagnation or even decay. In his autobiography Gibbon has left us a picture of one old university that will very well serve for both: idle students and still more reprehensible teachers, professors who never lectured, and some who never resided. Cambridge mathematical studies had gone the same way as the other
disciplines. The University degree examinations were still held in the medieval fashion, but these disputations had by now sunk to a mere farce.

Towards the end of the century, however, the first signs of a Cambridge revival appeared. A mathematical examination with some pretensions to seriousness came into being; although at first purely optional, it gradually ousted the old degree examination, so that in the early nineteenth century it became the sole test for the B.A. It was then that the examination acquired the name of the Mathematical Tripos. The results of the examination were published in order of merit; the first man on the list was called the Senior Wrangler; after him came the other Wranglers—these were the candidates who had been deemed worthy of a first class. Next in order came the Senior Optimes—these formed the second class. Finally, there were the third class men who were called, somewhat euphemistically, Junior Optimes; one suspects that by modern standards a fair number of these would have been refused a degree altogether. On Degree Day, when the successful candidates were presented to the Vice-Chancellor in the Senate House, a curious ceremony was observed. As the last Junior Optime—the bottom man on the list—came forward to be presented, from the public gallery there was lowered an enormous wooden spoon, which he received as a consolation prize. The expression “to get the wooden spoon” has since become proverbial.

As we have said, under the new arrangements every Cambridge man who wished to graduate had to go through the mathematical examination. At this stage of events, the ease with which one could scrape a third class showed a way out of the difficulty; even so, many men of ability were compelled to waste their time, and some had to leave the University without a degree. Even after another examination, the Classical Tripos, was instituted in 1822, the degree examination remained the same. And soon after that its standard was to be stiffened to a remarkable extent. That made things much worse for the nonmathematicians.

Quite early in the nineteenth century a handful of Cambridge men began to realize that it was high time to come out of the Great Sulk. In 1821 they formed what they called the Cambridge Analytical Society, whose aim was to familiarize British mathematicians with the work of the great Europeans. One of the leaders of this movement was Charles Babbage, who has since become famous as the father of the electronic computer. The task assumed by these young men was formidable indeed: for there is only one more conservative institution in the world than Cambridge, and that is Oxford. But it may be asserted that after ten years or so of propaganda their work began to show fruit. We thus enter the modern period of Cambridge mathematical studies, and with it the utterly fantastic story of the Mathematical Tripos in its golden days.

As we have already recounted, the Tripos was from its inception a competitive examination. By some process which has never been satisfactorily described, the fresh enthusiasm for mathematics which now burst upon the University transformed this examination into a high-speed marathon whose like has never been
seen before or since. It became far and away the most difficult mathematical test that the world has ever known, one to which no university of the present day can show any parallel. This is undoubtedly a sweeping statement; but the evidence for it is clear and overwhelming. The nineteenth century is, of course, the great period of Cambridge mathematical physics; it includes Ferrers, Green, Stokes, Kelvin, Clerk Maxwell, G. H. Darwin, Rayleigh, Larmor, J. J. Thomson—to mention only the top flight. Now all these men went through the Tripos mill; and it strained their abilities to the utmost.

At that time the teaching was entirely in the hands of the individual colleges, and much of it was grievously inadequate. But in any case, it could never have served the needs of the Tripos examination in its new form. Other methods of instruction were sought—and found. Any man coming to Cambridge and wishing to take a high place in the Tripos, at once put himself in the hands of a professional coach. The training was intensive and unremitting; it lasted for ten terms, with all the intervening long vacations as extra study periods. The examination was then taken early in the January of the undergraduate's fourth year of residence.

Each of the coaches divided his pupils into a number of small groups, who met him once or twice a week. At the meeting he would hand them back their solutions to the previous week's problems, and circulate in class his own set of solutions. While this was going on he would expound the new theorem or subject for study at the blackboard—which meant that a trainee had for part of the time at least to subdivide his attention between manuscript and lecture.

This relentless driving had two purposes before it: to train the candidate to the point where he could write out, with lightning speed and no hesitation, the proof of any theorem required by the syllabus; and where he could write out, at lightning speed and with almost no hesitation, the solution to any problem the examiners might set. Mere mathematical ability was not enough: rapidity of thought had to be added to it. Let me give some examples. Cayley, who afterwards occupied the Cambridge chair of pure mathematics for many years, took the Tripos in 1842. At that time a feature of the examination was a two-hour paper consisting entirely of problems; it was not expected that the candidates would do all the questions—they would naturally have a choice. On the evening after the problem paper, a friend called on Cayley to see how things were going; and, in the manner of friends, he brought bad news. "I've just seen Smith," he announced (Smith was a rival), "and do you know, he did all the questions within two hours." "Oh," remarked Cayley, "well, I cleaned up that paper in forty-five minutes."

Lord Kelvin, when he was just plain William Thomson of Peterhouse, was easily the best mathematician of his year, and was widely tipped for the Senior Wranglership. In fact, on the day that the results of the Tripos were published, he said to his college servant, "Oh, just go down to the Senate House, will you, and see who is Second Wrangler?" Soon after the man returned, and announced: "You, sir." Evidently there had been someone in the examination hall who could write, if not think, faster than Kelvin.
As may be imagined, coaching for the Tripos was a highly specialized profession; at any given time there were only two or three men of outstanding capacity for this odd perversion of learning. The most famous of all coaches was unquestionably E. J. Routh. Routh was a very considerable mathematician in his own right, and his books on dynamics are still standard works of reference. For a number of years he had a virtual monopoly on Senior Wranglerships, and many of the highest places were invariably taken by his pupils. Another celebrated coach was R. R. Webb of St. John's College. It may surprise most people to learn that the great mathematician W. H. Young also had a hand in the business: one hardly associates such goings-on with a man of his calibre. But it should be borne in mind that the business was very profitable.

In my student days at Cambridge, I attended some advanced lectures in differential geometry which were given by an elderly don named R. A. Herman. He was the last of the great coaches, a survival from a past epoch. Herman was a Fellow of Trinity College, of which Hardy and Littlewood also were members, and he had taught them both in his time, though their success could hardly be attributed to any of their instructors. All three of them had been Senior Wranglers. Nobody could recall when Herman had taken the Tripos—he was by then a legendary figure—but Hardy had triumphed in the year 1898 and Littlewood in 1908, the very last year of the old regime: the following year the new regulations came into force and with them the order of seniority disappeared. Herman had been a pioneer geometer in his day; it was he who introduced into England the use of the moving trihedron in differential geometry, and Hardy himself has put on record his gratitude to Herman for his inspiring lectures on this subject. But when I sat under him some thirty years later, the inspiration had all flickered out; he reminded one of an extinct volcano. Just occasionally, in a chance word or gesture, one could glimpse the man there had been.

So far we have dealt with the Tripos in its more superficial aspects. What was the examination itself like in those times? At the beginning, and indeed until the Analytical Society had brought about a change, the whole system remained tied to Newton. Out of blind loyalty to their Master, the examiners insisted as far as possible on maintaining a form and a substance of which he might have approved. Thus in problems concerning planetary motion or gravitational attraction, candidates were obliged to use the methods of classical geometry which Newton had employed in the *Principia* and which his own discoveries in the calculus had already rendered obsolete even before he composed the work. And in questions dealing with the calculus, the candidates had to adopt the bad notation of fluxions and fluants due to Newton, instead of the good notation of Leibniz which had long gained universal acceptance elsewhere.

After the reformers had had their way, such antiquated notions were discarded. All the same, the Tripos examination remained predominantly a test in applied mathematics. There was an excellent reason for this bias: the ex-
aminers themselves knew hardly any pure mathematics anyway. And the system was obviously self-perpetuating: with each generation the mantle descended from mathematical physicist to mathematical physicist. However, it should not for a moment be imagined that the examination was particularly concerned with applied mathematics in any serious sense of the term: the typical Tripos question, which has been parodied over and over again, was an unreal, often fantastically unreal, abstraction from the physical problem which had suggested it, whose sole object was to render it tractable to the candidates.

Moreover, the Tripos remained a highly conservative institution; with the passage of the years, the distinguished band of mathematicians, whom we have already named, continued to make fundamental contributions to the science; but the Tripos syllabus, generally speaking, kept a respectable distance behind them. Thus Bertrand Russell, who took the examination about 1890, has commented upon both the academic character of the courses and the time-lag in the curriculum. In his day, the Tripos examiners had not yet caught up with Maxwell's equations, which had been given to the world a generation earlier.

Clearly, then, another reformer was called for, one who would take the work on from the point where Babbage and his friends had left it. This might conceivably have been Cayley, who occupied the chair of pure mathematics from 1863 until his death in 1895. But Cayley, despite his eminence in the field of pure mathematics, was a professor of the old school, who looked upon the academic world of his time, saw that it was very bad, but continued to go his own way. Although Cayley lectured regularly, as he was strictly bound to do, upon various branches of pure mathematics, his audience was practically nonexistent. Often it consisted of no more than one pupil. But that pupil was Andrew Russell Forsyth.

This extraordinary man, whom I had the privilege to know in his capacity of Professor Emeritus at the Imperial College of Science, died in extreme old age in 1942; but his fame lives on. Everybody knows him as the author of the most successful book on differential equations that has ever appeared in any language; although it was first published as long ago as 1885, it is still being reprinted. I would venture the opinion that this work has done more than anything else to retard the true development of the subject; for over two generations it has continued to put wrong ideas into people's heads concerning the nature and scope of the theory and, thanks to the author's forceful and authoritative style, in this it has been overwhelmingly successful.

The truth is that Forsyth had the misfortune to be born a hundred years too late; in his mathematical outlook and technique, he was a man of the eighteenth century. His major work on the theory of differential equations, a colossal achievement in six volumes, is still today the only treatise in its class which is by a single hand; but a mere glance at the list of contents suffices to
reveal that, on the whole, Forsyth looks backward to Lagrange rather than forward to Cauchy. However, some knowledge of the rudiments of analysis was essential to an understanding of the work; and as the Cambridge men of his generation had none, the author, who had now succeeded to Cayley's chair, set himself the task of educating them. And this is where he comes into our story.

In 1893 there appeared the first edition of Forsyth's *Theory of Functions of a Complex Variable*: another production which cannot be described as anything less than colossal—even a German professor might have quailed before such a project. The book includes fairly complete accounts of the relevant work of Cauchy, Abel, Riemann, Weierstrass, Appell, and carries on the survey right up to the then contemporary researches of Klein and Poincaré. The style of the book is magisterial, Johnsonian; the author's powers of assimilation are well-nigh incredible—and yet, strange to say, despite his intentions and his absorption of the material, he never comes within reach of comprehending what modern analysis is really about: indeed whole tracts of the book read as though they had been written by Euler.

Nevertheless, for all its shortcomings, this was the work which brought modern pure mathematics into Cambridge. The young men at once began to imbibe it; and not the young men alone. My own copy of the book once belonged to R. R. Webb, the coach whom I have mentioned above, and from his pencilled notes in the margins it seems pretty clear that he was learning his function theory the hard way, much as any beginner would. The very fact that the book was written in the wrong spirit probably contributed to its great initial success. As Littlewood once put it, "Forsyth was not very good at delta and epsilon"; but neither was the public for whom he wrote: so author and readers met on common ground. In any case, it served as a stepping-stone to the real thing, which at that date was to be found only in French or German. Hardy has recorded that he himself first saw the light when he read the volumes of Jordan's *Cours d'Analyse*; and many other young men of his generation must have done likewise.

Within the space of ten years, Forsyth's treatise had achieved its aim. But it also accomplished something which its author had certainly never intended. For Cambridge now found itself equipped with a corps of modern pure mathematicians whose nominal leader was a living fossil firmly fixed in the Sadlerian chair. This grotesque situation seemed to all intents and purposes a permanent one: Forsyth's international reputation was enormous and in any case there was no possibility of removing him; it appeared as though he were there for life. But now fate took a hand in the game. In the year 1909 Forsyth, in the company of other scientists and their families, was travelling to a meeting of the British Association to be held in Canada. Among the party were the eminent physicist C. V. Boys and his wife Marion. Forsyth was then an apparently confirmed bachelor of 51; but he and Marion Boys fell in love with
one another. The end of it was that she decided to leave her husband; and
this meant that Forsyth was compelled to resign his professorship, for in the
Cambridge of those days there was no place for even the suggestion of divorce.
It is pleasant to add that everyone concerned in this affair lived happily ever
after; for it was generally conceded by all his acquaintances that the bereaved
husband bore his loss with remarkable fortitude. (The former Mrs. Boys was
a powerful personality.)

Forsyth survived his wife by many years; in fact he contrived to outlive
everything—that was his tragedy. He had to retire from his chair at the
Imperial College because he had reached the extreme age limit, although he
commanded enough energy to have carried on for at least another five years.
He set himself to learn Arabic and Persian; he wrote several enormous volumes
on what were ostensibly branches of modern mathematics, all treated from
the eighteenth century point of view; the Cambridge University Press, which
made a fortune out of his earlier publications, must have lost a good deal of
it on these. And all the time he was filling reams of paper with formulae and
calculations; I happen to possess some manuscripts of his Cambridge lectures
and also of some work on which he was engaged a year or two before his death:
the differences between them, from the standpoint of calligraphy, are almost
negligible.

I am pleased to relate that I have been able to pay one small tribute to
this remarkable son of Cambridge. Some years ago, when I was asked to re-
write the article on Cayley for the *Encyclopaedia Britannica*, I took the
opportunity to slip him in; and there, for some time to come, I hope he will
stay.

But to continue with our main narrative. Although it almost goes without
saying that Forsyth had himself been a Senior Wrangler (he had studied with
Routh and Webb) and, moreover, was temperamentally inclined towards the
Tripos kind of mathematics, yet he was one of the chief promoters of Tripos
reform. At this point we may conveniently put a question which must have
occurred to the reader very much earlier: how did such a fantastic sort of
academic contest ever take root in the university? I think that the blame for
it must be laid upon the system of college autonomy which we have previously
described. In the absence of any strong central authority, the examination
had fallen into the hands of private individuals, owing no responsibility to
anyone; and until the university was able to regain some of its lost powers,
there was little chance of breaking their hold over it. This reversion to the
medieval form of government took place at about the turn of the century;
the time was now ripe for reform.

As I have said, the new regulations for the Tripos came into force in 1909,
the very year in which Forsyth, unbeknown to himself, was preparing to quit
Cambridge. When I took the examination, nearly twenty years later, the
net result of all the changes could be summarised as follows. In the first place,
the published order of merit had gone, and with it the rat race. Secondly, there was now a fair balance between pure and applied mathematics in both syllabus and examination questions. In the third place, a more advanced section of the old Tripos, which had been taken at a later stage by men who were seeking a fellowship, was incorporated in the undergraduate examination and entitled Schedule B, to distinguish it from the "elementary" part, known as Schedule A. (Incidentally, the University has since returned to the old practice: Schedule B, rechristened Part III, is now generally taken after a fourth year of residence.)

What was the new examination like? No doubt, if any of the high Wranglers from the past century could have returned to scrutinise our papers, they would have pronounced them pretty easy. To most of us, however, they appeared quite otherwise, particularly at first sight, in the examination room. To begin with, although the notorious Tripos trickery had now given place to a more mature outlook, there was still sufficient need for ingenuity to make the whole proceeding a highly risky business. The typical Schedule A question was a three-decker: first the candidate would be asked to prove a theorem; then would come a problem based more or less on this theorem; and thirdly, another problem even less based than the first. In fact, despite all appearances to the contrary, this last might break fresh ground: that was the sting in the tail. Everybody knew that only complete answers to questions really counted, and that the postscript usually mattered more than the rest. Hence a certain general foreboding. A candidate, even a well-prepared one, might go into the examination on the Monday morning and find himself unable to do a single complete question; if, unduly depressed by this failure, he had the same experience on Monday afternoon, then it was all over save the post-mortem.

The kind of question one had to face may be illustrated by the following example—possibly fictitious, for I do not remember seeing it anywhere. In the first part the candidate is asked to obtain the general solution to Laplace's equation in three dimensions, in terms of Legendre functions. Next, he is required to apply his results to the problem of a conducting sphere in a given field. In the third part the sphere is replaced by an ellipsoid which is nearly spherical. Now the fun begins: the answer to this part of the question is stated—not as a guide to the solver, but in order to make the question more difficult. For as the desired result is merely an approximation, anybody could produce an answer of sorts: the real difficulty is to arrive at the formula stated by the examiners. In questions of this type one might polish off the first two parts in no time at all, only to waste up to an hour on the third. And that way madness lies.

Schedule B was a quite different affair, not without its own peculiar troubles. Whereas Schedule A was taken by everyone, and on it one's class was usually decided, Schedule B was optional, that is to say, if any undergraduate possessed the necessary tenaciousness, cunning, and indiscipline, he
might be able to persuade his tutor to let him out of it. Assuming this was impracticable, he found himself confronted by a dilemma. Schedule B was based on the advanced courses in pure and applied mathematics; the questions set in it were of the longish essay type, each taking about an hour to write out. Once again, only complete answers to the questions were really of much use. Now the courses were numerous and comprehensive, while the total number of questions set was comparatively small. If a candidate chose to take a few courses only, he might find on the fatal day that he was unable to answer a single question on a particular paper. I vividly recall one session of our examination at which, ten minutes after the papers had been given out, a candidate rose in his place and walked slowly out of the hall. This act did nothing to cheer those of us who remained.

One might grasp the other horn of the dilemma by electing to take a great number of courses so as to insure against this disaster. But then there arose another serious difficulty: how could one commit all this material to memory? Here again, a fragmentary knowledge of the syllabus was only of doubtful value; for by sheer bad luck the questions set might weave in and out of the candidate's recollections and so lead inevitably to incomplete answers.

Luck certainly played a considerable part in the examination. I myself had some of each sort, though admittedly more good than bad. I took a term's course of lectures given by Littlewood on the foundations of function theory—this course, or a modified form of it, is still being reprinted as a paperback. But the printed version can give no idea of how delightful the lectures actually were: for Littlewood is one of the wittiest mathematicians that Cambridge, or indeed any other university, has ever produced. When, however, we came to the examination I found to my dismay that he had set about the most difficult question in the course. This was it: "Prove that, if $a$ and $b$ are any two given numbers, then one of the following possibilities must hold: either $a$ is less than $b$, or $a$ is equal to $b$, or $a$ is greater than $b."$ Perhaps this result may seem obvious to some of my readers; it certainly seemed obvious to me at the time—in the sense in which it appears obvious to them. But I distinctly recalled that, during the lectures, Littlewood had made a fearful mess of the demonstration; it had taken him the best part of an hour to write up on the blackboard. And I had the feeling that, on the present occasion, he would settle for nothing less. So, apart from the jokes, that course had to be written off as a dead loss.

One day my director of studies said to me: "I see that Mr. Pars" (they were all Misters in those days—no Cambridge man would have been seen dead with a Ph.D.) "is giving a course on general dynamics. I think you might attend." Actually there was no subject I cared less about; but this was a command, and so I attended. Now L. A. Pars was one of those lightning performers in whom Cambridge has always specialised. It is true that he wrote every single word upon the blackboard, but at such a pace that it was next to impos-
sible to keep up with him: to understand what he was doing was quite out of the question. After two terms of this sort of treatment I had a wad of notes as bulky as Whittaker's treatise, on which the lectures were mainly based. An important difference between my account of the subject and Whittaker's was that he knew what he was writing about. As events showed, however, this was irrelevant; for in the examination I encountered two questions on the course which, although ostensibly devoted to problems of dynamics, were really the purest of pure mathematics. I managed to do them both.

This little incident brings us to the grievance which many students nourished against the Tripos system as a whole. In a reasonably balanced examination for the degree a candidate whose interests lay in pure mathematics would have been required to take all the pure mathematics courses together with a selection of the applied; and similarly for a specialist in applied mathematics. But the Cambridge plan insisted on the double dose; and for all but the highly gifted minority this laid an almost intolerable burden upon the conscientious student, to say nothing of the fact that it took no account of the use to which he would turn his knowledge in after life. Experience shows that a taste for one main branch or other of the subject is as a rule acquired fairly early, and that a change is seldom made after. Consequently most of us felt in our bones that we were wasting an awful lot of our time.

And so we arrive at the examination itself. This too could have been arranged better, one thought. Schedule A consisted of six papers: Monday, Tuesday, Wednesday, from 9 A.M. to noon, and then from 1:30 to 4:30 P.M. On the following Monday, Tuesday and Wednesday we sat for Schedule B, which likewise consisted of six papers, each of three hours duration. What went on during those thirty-six hours I cannot now recall in any detail; all I can say is that it was a kind of continuous nightmare. But there was an opening episode which still sticks in my memory. Our examination was held in the great hall of King's College, under the shadow of the famous Gothic chapel; so those candidates who were Scholars of King's had only to cross the college court. I happened to be acquainted with one of them—he was an eccentric individual hailing from Lancashire, and he was notorious for the fact that during the vacations he used to earn a living by playing the organ at the cinema in his home town. At that time vacation work was almost unheard of in England, and his conduct was generally regarded as very queer and perhaps slightly scandalous. As I have said, he had only to cross the court to reach the examination hall. He was wearing his gown, as was necessary at lectures and examinations, but he hadn't bothered to change his slippers. So the officials on duty declined to admit him because he was improperly dressed (I happen to know this as I was sitting near the door and overheard the conversation). Whether he subsequently returned, correctly shod for the occasion, or whether he remained shut out forever I really cannot say; for only a few minutes later I had many other things to worry about.
I remember clearly enough the closing scene of our Tripos. As soon as the last papers were handed in, two fellow-sufferers—myself and a friend—rushed from the hall and walked as fast as we knew how the three miles down to the river where the first of the May races were due to begin. There, in the midst of a crowd of undergraduates and their guests, we soon banished the Tripos from our thoughts. I secretly vowed never to take another examination in my life; and this vow, I am glad to say, I have kept.

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Here my own reminiscences of the Tripos come to an end; but of course the story goes on. Various generations of Cambridge men have each shaped the examination according to their light, but the work is never complete and probably never will be. Other intending reformers of the Tripos are even now waiting in the wings; indeed some among them would reform it altogether. Such a notion, startling as it may appear, is by no means novel; it was held more than forty years ago, by Hardy himself, who had backed the 1909 reform as only a first stage of the program. Hardy firmly believed that the Tripos was an unmitigated evil, for which one must blame the inferior performance of British pure mathematicians vis-à-vis their European colleagues. So, away with the examination.

Now the weakness of this argument resides in its lack of supporting evidence. It would be very difficult to unearth any specific cases of careers which have undoubtedly been ruined or even seriously damaged by the Cambridge mathematical system: on the other hand, the supporters of the status quo can for their part point to a long line of distinguished mathematical physicists, some of whom we have already mentioned, who achieved success either because or in spite of it: looking at their record one could scarcely suppose that they would have done more or better work had they been spared the ordeal of the examination; and, in the past, what an ordeal it was!

Abolitionists are such charming people; their motives are so patently pure, and only rarely do they foresee the full consequence of their projected panaceas. All his life long Hardy moved in the highest academic circles and tutored the most talented of young men. Had he troubled to consult any lecturer from a provincial university, or even (it may be) a don from a Cambridge college less exalted than Trinity, he could easily have learned a simple but significant truth: if students know beforehand that a particular subject is not to be examined upon, they will, almost to a man—or a woman—altogether decline to study it. Even Forsyth could have told Hardy that much: for he had been a professor at London University.