A Mathematical Education

It is *my* education. It illustrates conditions before 1907, but has some oddities of its own.

I am sure that I do not suffer from the weakness of false modesty, and to begin with I do not mind saying that I was precocious : as a matter of fact precocity in a mathematician has no particular significance one way or the other, and there are plenty of examples both ways ; I happen to belong to the precocious class.

Born June 9, 1885, I was in South Africa from 1892 to 1900 ; I left the Cape University at the age of 14, and after 2 or 3 months went to England to go to St. Paul's School, where I was taught for 3 years by F. S. Macaulay. My knowledge then was slight by modern standards ; the first 6 books of Euclid, a little algebra, trigonometry up to solution of triangles. During my 3 years at St. Paul's I worked intensively ; seriously overworked indeed, partly because it was a period of severe mental depression.

The tradition of teaching (derived ultimately from Cambridge) was to study 'lower' methods intensively before going on to 'higher' ones ; thus analytical methods in geometry were taken late, and calculus very late. And each book was more or less finished before we went on to the next. The accepted sequence of books was : Smith's *Algebra* ; Loney's *Trigonometry* ; *Geometrical Conics* (in a very stiff book of Macaulay's own : metrical properties of the parabola, for instance, gave scope for infinite virtuosity) ; Loney's *Statics and Dynamics*, without calculus ; C. Smith's *Analytical Conics* ; Edward's *Differential Calculus* ; Williamson's *Integral Calculus* ; Besant's *Hydrostatics*. These were annotated by Macaulay and provided with revision papers at intervals. Beyond this point the order

could be varied to suit individual tastes. My sequence, I think, was: Casey's *Sequel to Euclid*; Chrystal's *Algebra II*; Salmon's *Conics*; Hobson's *Trigonometry* (2nd edition, 1897); Routh's *Dynamics of a Particle* (a book of more than 400 pages and containing some remarkably highbrow excursions towards the end); Routh's *Rigid Dynamics*; Spherical Trigonometry (in every possible detail); Murray's *Differential Equations*; Smith's *Solid Geometry*; Burnside and Panton's *Theory of Equations*; Minchin's *Statics* (omitting elasticity, but including attractions, with spherical harmonics, and—of course—an exhaustive treatment of the attractions of ellipsoids).

I had read nearly all of this before the Entrance Scholarship Examination of December 1902. (I was expected to do well, but I found the papers difficult and got only a Minor Scholarship at Trinity.[1] I had had a severe attack of influenza some weeks before, and though I did not feel mentally unfit I certainly must have been.) We were not overtaught and there were no oral lessons, and while anyone *could* go to Macaulay in a difficulty it was on the whole not done. We went up, of course, with paper work at intervals, at first from examples marked by him in the current book, later from our own selections. (There was a weekly problem paper from Wolstenholme's collection, set at one time by him, later by the head boy, who was myself in my last year; if we all failed at a problem it became Macaulay's duty to perform at sight at the blackboard.) The class were encouraged to go to seniors for help, I should say to the great benefit of all concerned. Work directly for the Scholarship Examination was confined to a revision in the preceding term. (His academic successes, however, were notorious. In his 25 years at the School there were

[1] I remember being horribly put off in the first paper by sitting opposite a man who was rapidly reeling off the questions: I changed my seat for later papers. It must have been Mercer (who was a graduate of Manchester University and was making a fresh start at Cambridge, a not uncommon practice at the time).

I remember also that Cambridge inspired in me an awe equalled by nothing I have felt since.

41 scholarships (34 in Cambridge) and 11 exhibitions; and in the 20 years available these provided 4 Senior Wranglers, 1 2nd, and 1 4th. My own period was a peak. G. N. Watson, a year my junior at the school and at Cambridge, was also a Senior Wrangler; incidentally he was fully as precocious as myself. G. R. Blanco-White, a year my senior, was 2nd Wrangler.) Dr. Maxwell Garnett's description of the education as having a University atmosphere is a fair one. Self-reliance being the expected thing we mostly acquired it, and as Macaulay himself did creative work (he became an F.R.S. in 1928) we caught something of the feeling that mathematics was a natural activity.

There was nothing much wrong with my education so far and what was wrong was inherent in the system. Ideally I should have learnt analysis from a French Cour d'Analyse instead of from Chrystal and Hobson, but this would have been utterly unconventional. I did not see myself as a pure mathematician (still less as an analyst) until after my Tripos Part I, but I had enough instinctive interest in rigour to make me master the chapters of Chrystal on limits and convergence. The work is rigorous (within reasonable limits), and I really did understand, for instance, uniform convergence, but it is appallingly heavy going. (The 2nd edition of Hobson (1897) was a strange mixture, as Macaulay observed in a marginal note, of careful rigour and astonishing howlers [1], but I had done the 'convergence' sections in Chrystal.)

From this point (the Scholarship Examination), however,

* [1] E.g. the fallacious proof that two power series agreeing in value have identical coefficients. On pp. 243-4, again, there are remarkable passages. 'If the limit of S_n is infinite, or if it is finite but not definite, the series is not convergent.' 'To show that [the general principle of convergence] is sufficient, denote by R_n the infinite series $a_{n+1}+a_{n+2}+\ldots$, the remainder after n terms, then by making r [in $\sum_{n+1}^{n+r}$] infinite, we see that $|R_n|<\epsilon$ if $n\geqq m$, hence S has a value between $S_n\pm\epsilon$ [and ϵ is arbitrarily small]; also S_n being the sum of a number of finite quantities is finite, hence S is finite. Thus $S_{n+r}-S_n$ can be made as small as we please by

I wasted my time, except for rare interludes, for 2½ years (8 months at school, 2 academic years at Cambridge). First the 8 months at school. Rightly enough to begin with, I read Smith's *Solid Geometry* : this did not take long, though I recall that while I followed it easily enough I failed to digest it for examination purposes and did very badly in the questions in the final school examination. The best things to do in applied mathematics would have been the ' water, gas and electricity ' subjects. There was probably no suitable text-book on electricity, but Lamb's *Hydrodynamics* was available. The prolonged study of dynamics for some reason (my own fault) stopped short at only the elements of moving axes. In pure mathematics the ideal would again have been more *Cours d'Analyse*. Instead of such things I spent a long time reading Tait's book on the futile subject of Quaternions. Then occurred one of the interludes : I read Harkness and Morley's *Introduction to the Theory of Analytic Functions* (1898). The correct thing to say would be something about the opening up of infinite horizons and a new spirit of approach to mathematics. The cold facts were quite different. I was indeed greatly struck by individual things [1] and a number of them stuck with me for a long time [2]. But no infinite horizons. I am, as a

making n large enough, therefore lim S_n=lim S_{n+r}, hence the value of S is definite, being independent of the form of n.' [Trivial alterations to abbreviate; punctuation as in the original.]* Hobson was a professional analyst when he wrote this : it is a case, certainly very extreme, of blind spots and blindly following tradition when writing a text-book—one cannot be reopening questions all the time. I once caught myself in lecture reproducing a very bad test for differentiating under the integral sign, oblivious of the good one I should be using if I were writing a paper.

[1] Much as everyone is struck on first meeting definite integrals by contour integration ; this was in fact one of the things.

[2] This led to an incident in my first term at Cambridge. Our lecturer on analysis was rather a martinet. On one occasion I knew exactly what was coming, having read it in H. and M. : I wrote it all down at speed and looked elaborately out of the window. ' Are you not taking this down, Sir ? ' ' I've got it down.' He visibly hesitated whether to ask to see it (I was not then known to be any good), but in the end said ' I beg your pardon.' The class thought I had scored, but for myself I felt that he had, by making me feel a boor.

matter of fact, sceptical about such 'introductions': they can seem admirable if you know the subject already, but I don't think they thrill the beginner, at least legitimately; nothing but the hard technical story is any real good. Incidentally the book is a bit woolly in its account of real numbers; this was perhaps hardly avoidable in 1898; nothing my generation ever came across (at any rate in English) had the sharp bracing precision the student gets today.

On coming up to Cambridge (October 1903) I coached for 2 years (20 months [1]) for Part I of the Tripos with R. A. Herman, contemporary and friend of my father, and the last of the great coaches.[2] The period is gloomy to look back on. If I am to record new things I acquired which were in any sense worth acquiring, they were moving axes in dynamics, hydro-dynamics, and differential geometry (beyond what was in Smith). Also small additions to what I knew already in spherical harmonics and complex variable analysis. Electricity was completely scrappy and I never saw Maxwell's equations.[3] Enthusiasm was touched just twice, by a stimulating course in the first term by A. N. Whitehead on the foundations of mechanics, and by an admirable one on differential geometry given by Herman in his capacity of College lecturer, of which more later. To be in the running for Senior Wrangler one had to spend two-thirds of the time practising how to solve difficult problems against time. I remember that I had then no serious use for lectures, except Herman's; my note-books show that I attended only about half the time, and in such cases I never looked at the notes again.

[1] One long vacation only.

[2] The reform of 1910 extinguished almost at once the general practice of coaching.

[3] It is fair to say that in 2 years I could not use all the available courses (College or coaching). For completeness I should add that I wasted time on optics and astronomy (*not* worth knowing) and then practically disregarded them.

It used to be claimed that the discipline in ' manipulative skill ' bore later fruit in original work. I should deny this almost absolutely—such skill is very short-winded. My actual experience has been that after a few years nothing remained to show for it all except the knack, which has lasted, of throwing off a set of (modern) Tripos questions both suitable and with the silly little touch of distinction we still feel is called for ; this never bothers me as it does my juniors. (I said ' almost ' absolutely ; there could be rare exceptions. If Herman had been put on to some of the more elusive elementary inequalities at the right moment I can imagine his anticipating some of the latest and slickest proofs, perhaps even making new discoveries.)

The old Tripos and its vices are dead horses ; I will not flog them. I do not claim to have suffered high-souled frustration. I took things as they came ; the game we were playing came easily to me, and I even felt a sort of satisfaction in successful craftsmanship.

My detailed career for the 20 months was as follows. I overworked in my 1st Michaelmas term. On the other hand, I did all but no work in the Lent term (part cause training for the Lent races) ; in consequence I took the Trinity March Scholarship examination [1], for which anyhow it was impossible to prepare, feeling at the top of my form, and reversed my failure in the Entrance by coming out top of the list. In June I took 2nd year Mays and came out top (Mercer not sitting). I got full marks in the Analysis paper, my first contact with a startled Hardy, who had just come on the Trinity staff (and was privately coaching Mercer). In my 2nd year the only academic event was the Tripos (which I took while still 19) : I was bracketed Senior Wrangler with Mercer.

The Mays Analysis paper reminds me of a fatuous experi-

[1] For Senior Scholarships, and open to all (including Entrance Scholars) not already Senior Scholars: it is now abolished.

ment, and I will digress. I lived in Bideford (Devon) and decided to spend part of the Easter vacation buried at Hartland Quay (in superb scenery and the spot in England most distant from a railway station). The idea was to give up smoking, concentrate on work in the mornings and late afternoons, and 'relax' on poetry and philosophy (Principia Ethica) in the evenings, fortified by strong coffee. (Incidentally my generation worked mainly at night, and 1 o'clock was early to go to bed: there was also a monstrous belief that 8 hours was the minimum a mathematician should work a day; the really virtuous man, by cutting down his sleep, should achieve 10.) My window opened on the sea, which I used as a waste-paper-basket, and on arrival I ceremonially threw my pipes and tobacco into it. Next day I relapsed. The work I got through was very slight, but it consisted in reading the parts of Whittaker's *Modern Analysis* I did not already know, and revising, and this is why analysis was at my fingers' ends in the Mays. The experiment taught me something of the truth that for serious work one does best with a background of familiar routine, and that in the intervals for relaxation one should *be* relaxed. Much could be said on this theme, but this is not the moment for it: I will say, however, that for me the thing to avoid, for doing creative work, is above all Cambridge life, with the constant bright conversation of the clever, the wrong sort of mental stimulus, all the goods in the front window.

Something about the M.T.I. examination itself. It consisted of 7 papers ('1st 4 days') on comparatively elementary subjects, the riders, however, being quite stiff, followed a week later by another 7 ('2nd 4 days'). A pass on the 1st 4 days qualified for a degree, but the 2nd 4 days carried double the marks, and since it was impossible to revise everything the leading candidates concentrated on the 2nd 4 days[1], in which, moreover, it was generally

[1] I did one very bad paper in the first four days, all optics and astronomy.

possible to find enough questions without preparing all subjects. The leaders generally came out pretty level on the 1st 4 days, and the things they used to do I now find almost incredible. It is a lost world, and except for odd accidents I cannot remotely guess what questions I did, but I inherited the mark sheets of my year (1905) from one of the examiners. The marks of the leaders in the 1st 4 days, ignoring the problem paper, were 1350, 1330, 1280, 1230 (the Senior Wranglers 3rd and 4th), followed by 8 more, of whom the last got 990. Full marks were 1930, and the papers were of 10 questions, for the most part with stiff riders. On the problem paper Mercer got 270 out of 760 *for* 18 *questions* (I got only 180). In one paper I got 177 out of 230 for the riders, and I can remember something of this. One question was pure book-work about Carnot's cycle, of which I had not heard. Another was about a condenser, of which I also had not heard, but I reconstructed the question from the answer to the rider. My recollection, however, is that I did *all* that paper apart from Carnot: the marks I dropped must have been for inaccuracy and my notoriously slovenly 'style' [1]. In the 2nd 4 days (ignoring the problem paper) Mercer and I each got about 2050 out of 4500 (each about 330 out of 1340 in the 18 question problem paper). What staggers me most here was a paper (mixed pure and applied) in which I got practically full marks for book-work (290 out of 310, apparently I avoided 'slovenliness' here) *plus* 250 out of 590 for the riders.

The marks of the candidates have a frequency graph not at all Gaussian; it is horizontal from the highest point

[1] I do not take off marks in examinations for slovenliness as such (and always protest against examiners' bleatings that 'the numerical work was slovenly and inaccurate'). Muddled writing in considered work is of course a heinous crime, but at speed and at examination level it is trivial. Much nonsense used to be talked about this. It amuses me to recall the man, famous for clear thinking emerging in faultless copper-plate even in examinations, and held up to us as a model. In his later career he wrote more bad, muddled, and completely wrong mathematics than anyone before or since.

onward. Explanations suggest themselves, but oddly enough the graph of a recent Mays examination is roughly Gaussian.

There is only one other question I am sure of having done, and for the following reason. I began on a question on elementary theory of numbers, in which I felt safe in my school days. It did not come out, nor did it on a later attack. I had occasion to fetch more paper ; when passing a desk my eye lit on a heavy mark against the question. The candidate was not one of the leading people, and I half-unconsciously inferred that I was making unnecessarily heavy weather ; the question then came out fairly easily. The perfectly highminded man would no doubt have abstained from further attack ; I wish I had done so, but the offence does not lie very heavily on my conscience.

The M.T. II (1906). This dealt in quite genuine mathematics (and except that the corresponding Part III is now taken in the 3rd instead of a normal 4th year the examination has been much the same ever since the '80's). I wasted a good deal of time, unluckily in some ways, but partly in the ordinary course of trial and error. Pursuing differential geometry, I embarked on Darboux's *Théorie des Surfaces*, and read 3 of the 4 volumes (i.e. I read 1500 pages). It is a beautiful work, but my initial enthusiasm flagged : it was not my subject. In the examination there were several questions on it ; I did them all ; but I could have done them the year before, from Herman's lectures. The rest of my studies were in analysis of a sort. One thing Cambridge made almost inevitable for an analyst ; intensive study of Legendre functions, and all that. Such 'dictionary' subjects are utterly unsuitable for a good man. It was, however, the more inevitable in that a lecture was provided (by E. W. Hobson ; he later wrote a standard text-book) : I was the sole member of the class. It amuses me to recall that I could get up this kind of thing completely by heart : several questions were set and I wiped the floor with all of

them. I attended, also as sole member of the class, a lecture by E. W. Barnes on his current work on double Γ and ζ functions. There was a highly individual lecture by H. F. Baker, on selected points from widely scattered branches of analysis ; this was stimulating but not intended to be pedagogic. I presented the more elementary of the two parts constituting ' Elliptic Functions ' in the Schedule and ' attended ' the course on this by A. Berry. The lectures, however, were at 9 a.m., and I managed to get there only about half the time (working as I did till 2 or 3 in the morning) ; I never read up the notes, nor did I follow the obvious course of reading a text-book (though we depended far more on text-books and less on lectures than now), and I abandoned the subject for examination purposes. The fact is that I had as yet no sort of idea of what was good for me, and, again, I read no complex function theory proper. Having somehow acquired a working knowledge of Analysis I never read seriously any of the *Cours d'Analyse.* Others have put on record how Jordan first opened their eyes to what real mathematics was ; this I missed. But I was also very casual, Picard would certainly have been very good for me. My memory of all this is very hazy and I must have read things I have forgotten. A few weeks before the examination, in the Easter term, I first came across the early volumes of the Borel series, and it was these, in cold fact, that first gave me an authentic thrill : series of positive terms, divergent series, and the volume on integral functions. The first 2 were irrelevant for the examination ; the last I presented officially, but I lost the book, could not conveniently get another copy, and did not prepare it. But now I knew the kind of thing I wanted.

For special reasons I can identify the details about one paper (Friday, June 1, 1906, 9-12), and it interests me, in the light of my later activities, to see what I did *not* then know. There were 6 questions :

1. Elaborate Legendre functions.

2. Multiplication of series.
3. Discontinuous Riemann integrable function.
4. Reversion of power series (with a specific radius of convergence to be established).
5. Conformal representation of an oval on a half-plane.
6. Elliptic functions (ϑ-functions from the part of the subject I was not presenting).

In this the only question I was supposed to know about was number 1 (where of course I got full marks at high speed). By the rules of the game this entitled me to fullest possible marks for the whole paper; but in such cases one was naturally a little nervous about whether the examiners would notice, and this is why I remember everything. I knew something about number 4 unofficially, but not in the precise form given, so my answer was imperfect. About all the rest my ignorance was actual as well as official (though 5 years earlier, at school, I had known number 2).

I was told that I had done very well in the whole examination. I had, however, not yet technically qualified for a degree. In those days a Tripos taken in one's 2nd year did not count at all, and while M.T. II qualified for a degree when taken in one's 4th year it did not do so in one's 3rd. There have always been provisions (Special Graces of the Senate) for dealing with anomalies, however, and I heard about this only by chance.

There were 9 classes, I(1) to III(3). The standard was sometimes preposterous, and the examination went out in 1910 in a blaze of glory. An unusually large and strong field included 6 people afterwards well-known as Professors of mathematics, or an equivalent; class I division 1 was empty. It was empty also the previous year, and had only 1 member the year before that.

To finish with lectures. In my 4th year there were probably few left for me to go to. A. R. Forsyth (Sadleirian Professor) gave a course on differential equations; this did not appeal to me. What I did go to were courses by White-

head on foundations of geometry and on foundations of mathematics, given for the first time. There were 3 or 4 of us in the class and we found them very exciting. (Whitehead had recently been made a Senior College Lecturer at Trinity, with the duty of giving lectures out of the ordinary run. His stock College lectures, except for the one on the principles of mechanics, were solid and unexciting affairs on applied mathematics : a mathematician can have the duty of being dull—Eddington lecturing on Spherical Trigonometry.) I cannot remember going to any other lectures.

My research began, naturally, in the Long Vacation of my 3rd year, 1906. My director of studies (and tutor) E. W. Barnes suggested the subject of integral functions of order 0. The first idea was to find asymptotic formulae for functions with simple given zeros like $a_n=e^n$; the analytic methods he had been using with success for non-zero order were not working. Incidentally this brought me into touch with another famous and important Borel volume, Lindelöf's *Calcul des Résidus*. There were the best of reasons for the method's not working, as appeared later, but the general suggestion was an excellent one ; I rather luckily struck oil at once by switching to more ' elementary ' methods, and after that never looked back. The conjecture soon suggested itself that a function of order 0 would, on some large circles, have the property

$$m(r)>(M(r))^{1-\epsilon},$$

where $M(r)$ and $m(r)$ are the maximum and minimum moduli. By my elementary methods, at least, this is quite tough, and it took me probably a couple of months. (The corresponding result for non-zero order is that for order less than $\frac{1}{2}$ $m(r)$ is as large as a positive power of $M(r)$ (on some circles). This I could prove only with $\frac{1}{4}$ for $\frac{1}{2}$, and the full result was proved later by A. Wiman by more ' function-theory ' methods. A. S. Besicovitch, however, has recently revived the elementary method to prove further extensions.) I sent

a longish paper (about functions of order 0) to the London Mathematical Society (Jan. 1, 1907). I should omit a good deal today, but it was not too obscurely written, and the $m>M^{1-\epsilon}$ result is quite respectable. It also contains what I believe is the first instance of a certain 'averaging argument'. (One wants to prove that a function $f(x)$ exceeds a suitable lower bound m at *some* point of a range $0\leqslant x\leqslant 1$, say. Each individual point is intractable but the way out sometimes exists that the average of $f(x)$ over (0, 1) can be shown to exceed an m; then *some* x, though unidentifiable, must make $f(x)>m$.) The referees disagreed, one being violently unfavourable (by the time I learned in later life who he was I had disinterestedly come to think him a bit of an ass). Hardy was appointed as 3rd referee and the paper was duly published. I have not since had trouble with papers, with the single exception that the Cambridge Philosophical Society once rejected (quite wrongly) one written in collaboration with Hardy.

Barnes was now encouraged to suggest a new problem: 'prove the Riemann Hypothesis'. As a matter of fact this heroic suggestion was not without result; but I must begin by sketching the background of $\zeta(s)$ and prime numbers in 1907, especially so far as I was myself concerned. I had met $\zeta(s)$ in Lindelöf, but there is nothing there about primes, nor had I the faintest idea there was any connexion; for me the R.H. was famous, but only as a problem in integral functions; and all this took place in the Long Vacation when I had no access to literature, had I suspected there was any. (As for people better instructed, only some had heard of Hadamard's paper, and fewer still knew of de la Vallée Poussin's in a Belgian journal. In any case, the work was considered very sophisticated and outside the main stream of mathematics. The famous paper of Riemann is included in his collected works; this states the R.H., and the extraordinary, but unproved, 'explicit formula' for $\pi(x)$; the 'Prime Number Theorem' is not mentioned, though it is

doubtless an easy guess granted the explicit formula. As for Hardy in particular, he told me later that he 'knew' the P.N.T. had been proved, but he thought by Riemann. All this was transformed at a stroke by the appearance of Landau's book in 1909.)

I remembered the Euler formula $\zeta(s)=\Pi(1-p^{-s})^{-1}$; it was introduced to us at school, as a *joke* (rightly enough, and in excellent taste). (Oddly enough it is not in Chrystal's *Algebra*; but in the 'convergence' chapter there is an example, with references: $\Sigma f(p)$ is convergent if $\Sigma f(n)/\log n$ is. The n's are however misprinted as p's. Against the resulting false statement I find a note made by me in 1902, query $f(p)=1/p$: I was sure in 1902 that $\Sigma 1/(p \log p)$ converges—it is actually not too big a jump from the Euler product.) In the light of Euler's formula it is natural to study $P(s)=\Sigma p^{-s}$. I soon saw that if the P.N.T. were true 'with error about $\sqrt{x}$' the R.H. would follow. Now at that time, and for anyone unacquainted with the literature, there was no reason to expect any devilment in the primes. And the $\sqrt{x}$ seems entirely natural, for the reason that a proper factor of n cannot exceed $\sqrt{n}$. So I started off in great excitement and confidence, and only after a week or so of agony came to realize the true state of things. There was, however, a consolation prize. It occurred to me to try the reverse argument: I assumed R.H., operated (in the line of least resistance) with the integral function

$$\Pi\left\{\left(1+\frac{z}{p}\right)e^{-\frac{z}{p}}\right\}$$

and successfully deduced the P.N.T. This was just in time for my 1st Fellowship dissertation (September 1907); I suppressed it the following year.

I have a clear recollection of my youthful views about the P.N.T., and they illustrate the uncertainty of judgment and taste in a beginner in a field with no familiar land-marks. I was thrilled *myself*; but didn't feel at all sure how the

result would appeal to others, and if someone had said, 'not bad, but of course very special, not "proper" mathematics', I should have meekly acquiesced. Hardy (a junior Fellowship Elector at the time) told me much later that he had 'courageously' said at the time that it was the best thing in the dissertation, though without realizing it was submitted as original. The dissertation as a whole was well received, and though I was passed over for a man at his last shot there was a gentleman's agreement that I should be elected next time.

From October 1907 to June 1910 I was Richardson lecturer at Manchester University. At £250 this was better than the usual £150 or £120, and I was advised to take it, but it was a great mistake. I could have stayed in Cambridge as a Research Scholar, and was soon offered the Allen Scholarship (incidentally tenable with a Fellowship if one got that later), but refused it to stay at Manchester. I did not gain financially, but felt I needed a change from Cambridge. If an austere desire for working at full stretch was also a motive it was fulfilled. My work was as follows. 3 hours lecturing to a 'failed Matriculation' class (the University earned fees by this); 3 hours to superior Intermediate; 3 (possibly 2) hours to pupil teachers on 'Principles of Mathematics' (an 'Education' stunt, naturally a complete failure), 2 hours class-work with 3rd year Honours class, and 3 hours full-dress lectures to them. Most of the unoccupied time during the mornings was spent in a sort of 'class-work': 12 to 20 students sat doing examples, to be helped out when they got stuck; it is an admirable system (for the students). Beyond this there was much paper work from the large elementary classes. In any case, and whatever the details, what happened was 4 hours work of one sort or another on M., W., F. mornings, 3 hours Tu., Th. mornings; after lunch paper work and some lecture preparation done in a private room at the University

and lasting from 2.30 to 4.0 or 4.30. (Elementary lectures we learned, of course, to deliver with a minimum of preparation, on occasion extempore.) Saturdays were free. But while for most of the staff the day's work ended by 4.30, I had high pressure work on top of the low pressure mountain. The 3rd year Honours class at that time got what was in spirit the most liberal mathematical education in the country. Unhampered by the official examinations, which were made to yield the results known to be right, we aimed at doing a few selected things really properly, dealt with some utilitarian stuff in class-work, and did not try to cover everything. The Pure side of this was my responsibility, and I had a completely free hand. One of my selections was Differential Geometry. This gave comparatively little trouble, since I stuck slavishly to my notes of Herman, except for a necessary dilution. (Many years later I mentioned this to Hardy, who confessed in return that when he found being Professor of Geometry at Oxford involved giving actual lectures in Geometry he did exactly the same thing.) For the rest my lectures were in analysis. These called for as much preparation as any I have given since, and I had to prepare them in the evenings. Hardy's *Pure Mathematics* and Bromwich's *Infinite Series* were not available the 1st year. I must have found Jordan no use for what I wanted ; Goursat's *Cours* I could have used to some extent, were it not for the almost incredible fact that I was unaware of its existence. It is hard to realize now the difficulty of planning a logical order that would not unexpectedly let one down (and my admiration for Bromwich's performance was unbounded). I aimed only at teaching a working efficiency (no elegance, but full rigour—and we dealt even in repeated infinite integrals), but it was exceedingly hard going. The lectures were fairly successful, and temporarily seduced Sydney Chapman into becoming an analyst. (I added to my difficulties by being one of the most feckless young men I know of ; my lecture notes were

not unnaturally scrawled, but they were on odd sheets and too chaotic to be used another year.) It remains to add to this story that the two long terms were 10-week, the only remission being that the Long Vacation began reasonably early in June. Work at this pressure (apart from my special difficulties) was the accepted thing, and research was supposed to be done in one's leisure : I remember one Easter vacation, when I was worn out and could not force myself to work, suffering pangs of conscience over my laziness. ' Young men of today don't know what work is.' I should add that H. Lamb (doubling the parts of Pure and Applied Professors) did his full share of the work, and showed me many kindnesses.

I joined the Trinity staff in 1910 (succeeding Whitehead). This coincided with new mathematical interests. Landau's book on analytical number theory made exciting reading, and stimulated me to some ideas on the ζ-function, but I need not say anything about this. I have, however, some vivid, and to me amusing, recollections of the discovery of the proof of the ' Abel-Tauber Theorem ' (' if $\lim_{x\to 1} \sum a_n x^n = s$ and $a_n = O(1/n)$ then $\sum a_n$ converges to s '). This happened at Bideford in the Easter vacation of 1911. The problem had quite certainly been suggested by Hardy, but I was unaware that he had proved the (weaker) ' Cesàro-Tauber '. This is very strange. In the first place he had told me about it ; but I suppose at a time when I had not begun to think actively in that field. On the other hand, I had at that time in high degree the flair of the young for tracking down any previous experience that might bear on the problem in hand ; this must have been out of action. But however strange, it was providential. The main theorem depends on 2 separate ideas, and one of them is the connexion between 3 (or more) successive derivates (if $f = o(1)$ and $f'' = O(1)$ then $f' = o(1)$ [1]). I began on the Cesàro-Tauber and in the course

[1] See p. 36, and footnote 1.

of finding a proof was led to the derivates theorem : but for this the derivates theorem would never have emerged out of the rut of the established proof (which differed a good deal), and without it I should never have got the main theorem. (The derivates theorem was actually known, but buried in a paper by Hadamard on waves.) It is of course good policy, and I have often practised it, to begin without going too much into the existing literature.

The derivates theorem enables one to reject certain parts of the thing one wants to tend to 0. One day I was playing round with this, and a ghost of an idea entered my mind of making r, the number of differentiations, *large*. At that moment the spring cleaning that was in progress reached the room I was working in, and there was nothing for it but to go walking for 2 hours, in pouring rain. The problem seethed violently in my mind : the material was disordered and cluttered up with irrelevant complications cleared away in the final version, and the ' idea ' was vague and elusive. Finally I stopped, in the rain, gazing blankly for minutes on end over a little bridge into a stream (near Kenwith wood), and presently a flooding certainty came into my mind that the thing was done. The 40 minutes before I got back and could verify were none the less tense.

On looking back this time seems to me to mark my arrival at a reasonably assured judgment and taste, the end of my ' education '. I soon began my 35-year collaboration with Hardy.