THE INDIAN MATHEMATICIAN RAMANUJAN*

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I have set myself a task in these lectures which is genuinely difficult and which, if I were determined to begin by making every excuse for failure, I might represent as almost impossible. I have to form myself, as I have never really formed before, and to try to help you to form, some sort of reasoned estimate of the most romantic figure in the recent history of mathematics; a man whose career seems full of paradoxes and contradictions, who defies almost all the canons by which we are accustomed to judge one another, and about whom all of us will probably agree in one judgment only, that he was in some sense a very great mathematician.

The difficulties in judging Ramanujan are obvious and formidable enough. Ramanujan was an Indian, and I suppose that it is always a little difficult for an Englishman and an Indian to understand one another properly. He was, at the best, a half-educated Indian; he never had the advantages, such as they are, of an orthodox Indian training; he never was able to pass the "First Arts Examination" of an Indian university, and never could rise even to be a "Failed B.A." He worked, for most of his life, in practically complete ignorance of modern European mathematics, and died when he was a little over 30 and when his mathematical education had in some ways hardly begun. He published abundantly—his published papers make a volume of nearly 400 pages—but he also left a mass of unpublished work which had never been analysed properly until the last few years. This work includes a great deal that is new, but much more that is rediscovery, and often imperfect rediscovery; and it is sometimes still impossible to distinguish between what he must have rediscovered and what he may somehow have learnt. I cannot imagine anybody saying with any confidence, even now, just how great a mathematician he was and still less how great a mathematician he might have been.

These are genuine difficulties, but I think that we shall find some of them less formidable than they look, and the difficulty which is the greatest for me has nothing to do with the obvious paradoxes of Ramanujan's career. The real difficulty for me is that Ramanujan was, in a way, my discovery. I did not invent him—like other great men, he invented himself—but I was the first really competent person who had the chance to see some of his work, and I can still remember with satisfaction that I could recognise at once what a treasure I had found. And I suppose that I still know more of Ramanujan than any one else, and am still the first authority on this particular subject. There are other people in England, Professor Watson in particular, and Professor Mordell, who know parts of his work very much better than I do, but neither Watson nor Mordell knew Ramanujan himself as I did. I saw him and talked with him almost every

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day for several years, and above all I actually collaborated with him. I owe more to him than to any one else in the world with one exception, and my association with him is the one romantic incident in my life. The difficulty for me then is not that I do not know enough about him, but that I know and feel too much and that I simply cannot be impartial.

I rely, for the facts of Ramanujan's life, on Seshu Aiyar and Ramachaundra Rao, whose memoir of Ramanujan is printed, along with my own, in his *Collected Papers*. He was born in 1887 in a Brahmin family at Erode near Kumbakonam, a fair-sized town in the Tanjore district of the Presidency of Madras. His father was a clerk in a cloth-merchant's office in Kumbakonam, and all his relatives, though of high caste, were very poor.

He was sent at 7 to the High School of Kumbakonam, and remained there nine years. His exceptional abilities had begun to show themselves before he was 10, and by the time that he was 12 or 13 he was recognised as a quite abnormal boy. His biographers tell some curious stories of his early years. They say for example that, soon after he had begun the study of trigonometry, he discovered for himself "Euler's theorems for the sine and cosine" (by which I understand the relations between the circular and exponential functions), and was very disappointed when he found later, apparently from the second volume of Loney's *Trigonometry*, that they were known already. Until he was 16 he had never seen a mathematical book of any higher class. *Whittaker's Modern Analysis* had not yet spread so far, and Bromwich's *Infinite Series* did not exist. There can be no doubt that either of these books would have made a tremendous difference to him if they could have come his way. It was a book of a very different kind, Carr's *Synopsis*, which first aroused Ramanujan's full powers.

Carr's book (*A synopsis of elementary results in pure and applied mathematics*, by George Shoobridge Carr, formerly Scholar of Gonville and Caius College, Cambridge, published in two volumes in 1880 and 1886) is almost unprocurable now. There is a copy in the Cambridge University Library, and there happened to be one in the library of the Government College of Kumbakonam, which was borrowed for Ramanujan by a friend. The book is not in any sense a great one, but Ramanujan has made it famous, and there is no doubt that it influenced him profoundly and that his acquaintance with it marked the real starting point of his career. Such a book must have had its qualities, and Carr's, if not a book of any high distinction, is no mere third-rate textbook, but a book written with some real scholarship and enthusiasm and with a style and individuality of its own. Carr himself was a private coach in London, who came to Cambridge as an undergraduate when he was nearly 40, and was 12th Senior Optime in the Mathematical Tripos of 1880 (the same year in which he published the first volume of his book). He is now completely forgotten, even in his own college, except in so far as Ramanujan has kept his name alive; but he must have been in some ways rather a remarkable man.

I suppose that the book is substantially a summary of Carr's coaching notes. If you were a pupil of Carr, you worked through the appropriate sections of the

Synopsis. It covers roughly the subjects of Schedule A of the present Tripos (as these subjects were understood in Cambridge in 1880), and is effectively the "synopsis" it professes to be. It contains the enunciations of 6165 theorems, systematically and quite scientifically arranged, with proofs which are often little more than cross-references and are decidedly the least interesting part of the book. All this is exaggerated in Ramanujan's famous note-books (which contain practically no proofs at all), and any student of the note-books can see that Ramanujan's ideal of presentation had been copied from Carr's.

Carr has sections on the obvious subjects, algebra, trigonometry, calculus and analytical geometry, but some sections are developed disproportionally, and particularly the formal side of the integral calculus. This seems to have been Carr's pet subject, and the treatment of it is very full and in its way definitely good. There is no theory of functions; and I very much doubt whether Ramanujan, to the end of his life, ever understood at all clearly what an analytic function is. What is more surprising, in view of Carr's own tastes and Ramanujan's later work, is that there is no elliptic functions. However Ramanujan may have acquired his very peculiar knowledge of this theory, it was not from Carr.

On the whole, considered as an inspiration for a boy of such abnormal gifts, Carr was not too bad, and Ramanujan responded amazingly.

"Through the new world thus opened to him," say his Indian biographers,* "Ramanujan went ranging with delight. It was this book which awakened his genius. He set himself to establish the formulae given therein. As he was without the aid of other books, each solution was a piece of research so far as he was concerned . . . Ramanujan used to say that the goddess of Namakkal inspired him with the formulae in dreams. It is a remarkable fact that frequently, on rising from bed, he would note down results and rapidly verify them, though he was not always able to supply a rigorous proof. . . ."

I have quoted the last sentences deliberately, not because I attach any importance to them—I am no more interested in the goddess of Namakkal than you are—but because we are now approaching the difficult and tragic part of Ramanujan's career, and we must try to understand what we can of his psychology and of the atmosphere surrounding him in his early years.

I am sure that Ramanujan was no mystic and that religion, except in a strictly material sense, played no important part in his life. He was an orthodox high-caste Hindu, and always adhered (indeed with a severity most unusual in Indian residents in England) to all the observances of his caste. He had promised his parents to do so, and he kept his promises to the letter. He was a vegetarian in the strictest sense—this proved a terrible difficulty later when he fell ill—and all the time he was in Cambridge he cooked all his food himself, and never cooked it without first changing into pyjamas.

Now the two memoirs of Ramanujan printed in the *Papers* (and both written

* Quotations (except those from my own memoir of Ramanujan) are from Seshu Aiyar and Ramachaundra Rao.

by men who, in their different ways, knew him very well) contradict one another flatly about his religion. Seshu Aiyar and Ramachaundra Rao say

"Ramanujan had definite religious views. He had a special veneration for the Namakkal goddess. . . . He believed in the existence of a Supreme Being and in the attainment of Godhead by men. . . . He had settled convictions about the problem of life and after . . . ";

while I say

" . . . his religion was a matter of observance and not of intellectual conviction, and I remember well his telling me (much to my surprise) that all religions seemed to him more or less equally true . . . ".

Which of us is right? For my part I have no doubt at all; I am quite certain that I am.

Classical scholars have, I believe, a general principle, *difficilior lectio potior*—the more difficult reading is to be preferred—in textual criticism. If the Archbishop of Canterbury tells one man that he* believes in God, and another that he does not, then it is probably the second assertion which is true, since otherwise it is very difficult to understand why he should have made it, while there are many excellent reasons for his making the first whether it be true or false. Similarly, if a strict Brahmin like Ramanujan told me, as he certainly did, that he had no definite beliefs, then it is 100 to 1 that he meant what he said.

This was no sufficient reason why Ramanujan should outrage the feelings of his parents or his Indian friends. He was not a reasoned infidel, but an "agnostic" in its strict sense, who saw no particular good, and no particular harm, in Hinduism or in any other religion. Hinduism is, far more for example than Christianity, a religion of observance, in which belief counts for extremely little in any case, and, if Ramanujan's friends assumed that he accepted the conventional doctrines of such a religion, and he did not disillusion them, he was practising a quite harmless, and probably necessary, economy of truth.

This question of Ramanujan's religion is not itself important, but it is not altogether irrelevant, because there is one thing which I am really anxious to insist upon as strongly as I can. There is quite enough about Ramanujan that is difficult to understand, and we have no need to go out of our way to manufacture mystery. For myself, I liked and admired him enough to wish to be a rationalist about him; and I want to make it quite clear to you that Ramanujan, when he was living in Cambridge in good health and comfortable surroundings, was, in spite of his oddities, as reasonable, as sane, and in his way as shrewd a person as anyone here. The last thing which I want you to do is to throw up your hands and exclaim "here is something unintelligible, some mysterious manifestation of the immemorial wisdom of the East!" I do not believe in the immemorial wisdom of the East, and the picture I want to present to you is that of a man who had his peculiarities like other distinguished men, but a man in whose

* The Archbishop.

society one could take pleasure, with whom one could take tea and discuss politics or mathematics; the picture in short, not of a wonder from the East, or an inspired idiot, or a psychological freak, but of a rational human being who happened to be a great mathematician.

Until he was about 17, all went well with Ramanujan.

"In December 1903 he passed the Matriculation Examination of the University of Madras, and in the January of the succeeding year he joined the Junior First in Arts class of the Government College, Kumbakonam, and won the Subrahmanyam scholarship, which is generally awarded for proficiency in English and Mathematics . . . ",

but after this there came a series of tragic checks.

"By this time, he was so absorbed in the study of Mathematics that in all lecture hours—whether devoted to English, History, or Physiology—he used to engage himself in some mathematical investigation, unmindful of what was happening in the class. This excessive devotion to mathematics and his consequent neglect of the other subjects resulted in his failure to secure promotion to the senior class and in the consequent discontinuance of the scholarship. Partly owing to disappointment and partly owing to the influence of a friend, he ran away northward into the Telugu country, but returned to Kumbakonam after some wandering and rejoined the college. As owing to his absence he failed to make sufficient attendances to obtain his term certificate in 1905, he entered Pachaiyappa's College, Madras, in 1906, but falling ill returned to Kumbakonam. He appeared as a private student for the F. A. examination of December 1907 and failed . . .".

Ramanujan does not seem to have had any definite occupation, except mathematics, until 1912. In 1909 he married, and it became necessary for him to have some regular employment, but he had great difficulty in finding any because of his unfortunate college career. About 1910 he began to find more influential Indian friends, Ramaswami Aiyar and his two biographers, but all their efforts to find a tolerable position for him failed, and in 1912 he became a clerk in the office of the Port Trust of Madras, at a salary of about £30 a year. He was then nearly 25. The years between 18 and 25 are the critical years in a mathematician's career, and the damage had been done. Ramanujan's genius never had again its chance of full development.

There is not much to say about the rest of Ramanujan's life. His first substantial paper had been published in 1911, and in 1912 his exceptional powers began to be understood. It is significant that, though Indians could befriend him, it was only the English who could get anything effective done. Sir Francis Spring and Sir Gilbert Walker obtained a special scholarship for him, £60 a year, sufficient for a married Indian to live in tolerable comfort. At the beginning of 1913 he wrote to me, and Professor Neville and I, after many difficulties, got him to England in 1914. Here he had three years of uninterrupted activity, the results of which you can read in his *Papers*. He fell ill in the summer of 1917, and never really recovered, though he continued to work, rather spasmodically, but with no real sign of degeneration, until his death in 1920. He became a Fellow of the Royal Society early in 1918, and a Fellow of Trinity College, Cambridge, later in the same year (and was the first Indian elected to either society). His last mathematical letter on "Mock-Theta functions", the subject of Professor Watson's presidential address to the London Mathematical Society last year, was written about two months before he died.

The real tragedy about Ramanujan was not his early death. It is of course a disaster that any great man should die young, but a mathematician is often comparatively old at 30, and his death may be less of a catastrophe than it seems. Abel died at 26 and, although he would no doubt have added a great deal more to mathematics, he could hardly have become a greater man. The tragedy of Ramanujan was not that he died young, but that, during his five unfortunate years, his genius was misdirected, side-tracked, and to a certain extent distorted.

I have been looking again through what I wrote about Ramanujan 16 years ago, and, although I know his work a good deal better now than I did then, and can think about him more dispassionately, I do not find a great deal which I should particularly want to alter. But there is just one sentence which now seems to me indefensible. I wrote

"Opinions may differ about the importance of Ramanujan's work, the kind of standard by which it should be judged, and the influence which it is likely to have on the mathematics of the future. It has not the simplicity and the inevitableness of the very greatest work; it would be greater if it were less strange. One gift it shows which no one can deny, profound and invincible originality. He would probably have been a greater mathematician if he could have been caught and tamed a little in his youth; he would have discovered more that was new, and that, no doubt, of greater importance. On the other hand he would have been less of a Ramanujan, and more of a European professor, and the loss might have been greater than the gain . . ."

and I stand by that except for the last sentence, which is quite ridiculous sentimentalism. There was no gain at all when the College at Kumbakonam rejected the one great man they had ever possessed, and the loss was irreparable; it is the worst instance that I know of the damage that can be done by an inefficient and inelastic educational system. So little was wanted, £60 a year for five years, occasional contact with almost anyone who had real knowledge and a little imagination, for the world to have gained another of its greatest mathematicians.

Ramanujan's letters to me, which are reprinted in full in the *Papers*, contain the bare statements of about 120 theorems, mostly formal identities extracted from his note-books. I quote fifteen which are fairly representative. They include two theorems, (14) and (15), which are as interesting as any but of which one is false and the other, as stated, misleading. The rest have all been verified since by somebody; in particular Rogers and Watson found the proofs of the extremely difficult theorems (10)–(12).

(1) $$1-\frac{3!}{(1!2!)^3}x^2+\frac{6!}{(2!4!)^3}x^4-\cdots$$
$$=\left(1+\frac{x}{(1!)^3}+\frac{x^2}{(2!)^3}+\cdots\right)\left(1-\frac{x}{(1!)^3}+\frac{x^2}{(2!)^3}-\cdots\right).$$

(2) $$1-5\left(\frac{1}{2}\right)^3+9\left(\frac{1.3}{2.4}\right)^3-13\left(\frac{1.3.5}{2.4.6}\right)^3+\cdots=\frac{2}{\pi}.$$

(3) $$1+9\left(\frac{1}{4}\right)^4+17\left(\frac{1.5}{4.8}\right)^4+25\left(\frac{1.5.9}{4.8.12}\right)^4+\cdots=\frac{2^{3/2}}{\pi^{1/2}\{\Gamma(\frac{3}{4})\}^2}.$$

(4) $$1-5\left(\frac{1}{2}\right)^5+9\left(\frac{1.3}{2.4}\right)^5-13\left(\frac{1.3.5}{2.4.6}\right)^5+\cdots=\frac{2}{\{\Gamma(\frac{3}{4})\}^4}.$$

(5) $$\int_0^\infty \frac{1+\left(\frac{x}{b+1}\right)^2}{1+\left(\frac{x}{a}\right)^2}\cdot\frac{1+\left(\frac{x}{b+2}\right)^2}{1+\left(\frac{x}{a+1}\right)^2}\cdots dx=\tfrac{1}{2}\pi^{1/2}\frac{\Gamma(a+\frac{1}{2})\Gamma(b+1)\Gamma(b-a+\frac{1}{2})}{\Gamma(a)\Gamma(b+\frac{1}{2})\Gamma(b-a+1)}.$$

(6) $$\int_0^\infty\frac{dx}{(1+x^2)(1+r^2x^2)(1+r^4x^2)\cdots}=\frac{\pi}{2(1+r+r^3+r^6+r^{10}+\cdots)}.$$

(7) If $\alpha\beta=\pi^2$, then
$$\alpha^{-1/4}\left(1+4\alpha\int_0^\infty\frac{xe^{-\alpha x^2}}{e^{2\pi x}-1}dx\right)=\beta^{-1/4}\left(1+4\beta\int_0^\infty\frac{xe^{-\beta x^2}}{e^{2\pi x}-1}dx\right).$$

(8) $$\int_0^a e^{-x^2}dx=\tfrac{1}{2}\pi^{1/2}-\frac{e^{-a^2}}{2a+}\;\frac{1}{a+}\;\frac{2}{2a+}\;\frac{3}{a+}\;\frac{4}{2a+\cdots}.$$

(9) $$4\int_0^\infty\frac{xe^{-x\sqrt{5}}}{\cosh x}dx=\frac{1}{1+}\;\frac{1^2}{1+}\;\frac{1^2}{1+}\;\frac{2^2}{1+}\;\frac{2^2}{1+}\;\frac{3^2}{1+}\;\frac{3^2}{1+\cdots}.$$

(10) If $$u=\frac{x}{1+}\;\frac{x^5}{1+}\;\frac{x^{10}}{1+}\;\frac{x^{15}}{1+\cdots},\qquad v=\frac{x^{1/5}}{1+}\;\frac{x}{1+}\;\frac{x^2}{1+}\;\frac{x^3}{1+\cdots},$$
then $$v^5=u\frac{1-2u+4u^2-3u^3+u^4}{1+3u+4u^2+2u^3+u^4}.$$

(11) $$\frac{1}{1+}\;\frac{e^{-2\pi}}{1+}\;\frac{e^{-4\pi}}{1+\cdots}=\left\{\sqrt{\left(\frac{5+\sqrt{5}}{2}\right)}-\frac{\sqrt{5}+1}{2}\right\}e^{2\pi/5}.$$

(12) $$\frac{1}{1+}\;\frac{e^{-2\pi\sqrt{5}}}{1+}\;\frac{e^{-4\pi\sqrt{5}}}{1+\cdots}=\left[\frac{\sqrt{5}}{1+\sqrt[5]{\left\{5^{3/4}\left(\frac{\sqrt{5}-1}{2}\right)^{5/2}-1\right\}}}-\frac{\sqrt{5}+1}{2}\right]e^{2\pi/\sqrt{5}}.$$

(13) If $F(k)=1+\left(\frac{1}{2}\right)^2 k+\left(\frac{1.3}{2.4}\right)^2 k^2+\cdots$ and $F(1-k)=\sqrt{(210)}F(k)$, then

$k=(\sqrt{2}-1)^4(2-\sqrt{3})^2(\sqrt{7}-\sqrt{6})^4(8-3\sqrt{7})^2(\sqrt{10}-3)^4(4-\sqrt{15})^4(\sqrt{15}-\sqrt{14})^2(6-\sqrt{35})^2.$

(14) The coefficient of x^n in $(1-2x+2x^4-2x^9+\cdots)^{-1}$ is the integer nearest to

$$\frac{1}{4n}\left(\cosh(\pi\sqrt{n})-\frac{\sinh(\pi\sqrt{n})}{\pi\sqrt{n}}\right).$$

(15) The number of numbers between A and x which are either squares or sums of two squares is

$$K\int_A^x \frac{dt}{\sqrt{(\log t)}}+\theta(x),$$

where $K=0.764\cdots$ and $\theta(x)$is very small compared with the previous integral.

I should like you to begin by trying to reconstruct the immediate reactions of an ordinary professional mathematician who receives a letter like this from an unknown Hindu clerk.

The first question was whether I could recognize anything. I had proved things rather like (7) myself, and seemed vaguely familiar with (8). Actually (8) is classical; it is a formula of Laplace first proved properly by Jacobi; and (9) occurs in a paper published by Rogers in 1907. I thought that, as an expert in definite integrals, I could probably prove (5) and (6), and did so, though with a good deal more trouble than I had expected. On the whole the integral formulas seemed the least impressive.

The series formulas (1)–(4) I found much more intriguing, and it soon became obvious that Ramanujan must possess much more general theorems and was keeping a great deal up his sleeve. The second is a formula of Bauer well known in the theory of Legendre series, but the others are much harder than they look. The theorems required in proving them can all be found now in Bailey's Cambridge Tract on hypergeometric functions.

The formulas (10)–(13) are on a different level and obviously both difficult and deep. An expert in elliptic functions can see at once that (13) is derived somehow from the theory of "complex multiplication", but (10)–(12) defeated me completely; I had never seen anything in the least like them before. A single look at them is enough to show that they could only be written down by a mathematician of the highest class. They must be true because, if they were not true, no one would have had the imagination to invent them. Finally (you must remember that I knew nothing whatever about Ramanujan, and had to think of every possibility), the writer must be completely honest, because great mathematicians are commoner than thieves or humbugs of such incredible skill.

The last two formulas stand apart because they are not right and show Ramanujan's limitations, but that does not prevent them from being additional evidence of his extraordinary powers. The function in (14) is a genuine approximation to the coefficient, though not at all so close as Ramanujan imagined, and Ramanujan's false statement was one of the most fruitful he ever made, since it ended by leading us to all our joint work on partitions. Finally (15), though literally "true", is definitely misleading (and Ramanujan was under a real misapprehension). The integral has no advantage, as an approximation, over the simpler function

$$\frac{Kx}{\sqrt{(\log x)}}, \tag{16}$$

found in 1908 by Landau. Ramanujan was deceived by a false analogy with the problem of the distribution of primes. I must postpone till later what I have to say about Ramanujan's work on this side of the theory of numbers.

It was inevitable that a very large part of Ramanujan's work should prove on examination to have been anticipated. He had been carrying an impossible handicap, a poor and solitary Hindu pitting his brains against the accumulated wisdom of Europe. He had had no real teaching at all; there was no one in India from whom he had anything to learn. He can have seen at the outside three or four books of good quality, all of them English. There had been periods in his life when he had access to the library in Madras, but it was not a very good one; it contained very few French or German books; and in any case Ramanujan did not know a word of either language. I should estimate that about two-thirds of Ramanujan's best Indian work was rediscovery, and comparatively little of it was published in his life-time, though Watson, who has worked systematically through his notebooks, has since disinterred a good deal more.

The great bulk of Ramanujan's published work was done in England.* His mind had hardened to some extent, and he never became at all an "orthodox" mathematician, but he could still learn to do new things, and do them extremely well. It was impossible to teach him systematically, but he gradually absorbed new points of view. In particular he learnt what was meant by proof, and his later papers, while in some ways as odd and individual as ever, read like the works of a well-informed mathematician. His methods and his weapons, however, remained essentially the same. One would have thought that such a formalist as Ramanujan would have revelled in Cauchy's Theorem, but he practically never used it,* and the most astonishing testimony to his formal genius is that he never seemed to feel the want of it in the least.

It is easy to compile an imposing list of theorems which Ramanujan rediscovered. Such a list naturally cannot be quite sharp, since sometimes he found a part only of a theorem, and sometimes, though he found the whole theorem, he was without the proof which is essential if the theorem is to be properly understood. For example, in the analytic theory of numbers he had,

* Perhaps never. There is a reference to "the theory of residues" on p. 129 of the *Papers*, but I believe that I supplied this myself.

in a sense, discovered a great deal, but he was a very long way from understanding the real difficulties of the subject. And there is some of his work, mostly in the theory of elliptic functions, about which some mystery still remains; it is not possible, after all the work of Watson and Mordell, to draw the line between what he may have picked up somehow and what he must have found for himself. I will take only cases in which the evidence seems to me tolerably clear.

Here I must admit that I am to blame, since there is a good deal which we should like to know now and which I could have discovered quite easily. I saw Ramanujan almost every day, and could have cleared up most of the obscurity by a little cross-examination. Ramanujan was quite able and willing to give a straight answer to a question, and not in the least disposed to make a mystery of his achievements. I hardly asked him a single question of this kind; I never even asked him whether (as I think he must have done) he had seen Cayley's or Greenhill's *Elliptic Functions*.

I am sorry about this now, but it does not really matter very much, and it was entirely natural. In the first place, I did not know that Ramanujan was going to die. He was not particularly interested in his own history or psychology; he was a mathematician anxious to get on with the job. And after all I too was a mathematician, and a mathematician meeting Ramanujan had more interesting things to think about than historical research. It seemed ridiculous to worry him about how he had found this or that known theorem, when he was showing me half a dozen new ones almost every day.

I do not think that Ramanujan discovered much in the classical theory of numbers, or indeed that he ever knew a great deal. He had no knowledge at all, at any time, of the general theory of arithmetical forms. I doubt whether he knew the law of quadratic reciprocity before he came here. Diophantine equations should have suited him, but he did comparatively little with them, and what he did do was not his best. Thus he gave solutions of Euler's equation

$$x^3 + y^3 + z^3 = w^3, \tag{17}$$

such as

$$x=3a^2+5ab-5b^2,\ y=4a^2-4ab+6b^2,\ z=5a^2-5ab-3b^2,\ w=6a^2-4ab+4b^2; \tag{18}$$

and

$$x=m^7-3m^4(1+p)+m(2+6p+3p^2),\ y=2m^6-3m^3(1+2p)+1+3p+3p^2,$$
$$z=m^6-1-3p-3p^2,\ w=m^7-3m^4p+m(3p^2-1); \tag{19}$$

but neither of these is the general solution.

He rediscovered the famous theorem of von Staudt about the Bernoullian numbers:

$$(-1)^n B_n = G_n + \frac{1}{2} + \frac{1}{p} + \frac{1}{q} + \cdots \frac{1}{r}, \tag{20}$$

where $p, q, \cdots$ are those odd primes such that $p-1, q-1, \cdots$ are divisors of

$2n$, and G_n is an integer. In what sense he had proved it it is difficult to say, since he found it at a time of his life when he had hardly formed any definite concept of proof. As Littlewood says "the clear-cut idea of what is *meant* by a proof, nowadays so familiar as to be taken for granted, he perhaps did not possess at all; if a significant piece of reasoning occurred somewhere, and the total mixture of evidence and intuition gave him certainty, he looked no further". I shall have something to say later about this question of proof, but I postpone it to another context in which it is much more important. In this case there is nothing in the proof that was not obviously within Ramanujan's powers.

There is a considerable chapter of the theory of numbers, in particular the theory of the representation of integers by sums of squares, which is closely bound up with the theory of elliptic functions. Thus the number of representations of n by two squares is

$$r(n) = 4\{d_1(n) - d_3(n)\}, \tag{21}$$

where $d_1(n)$ is the number of divisors of n of the form $4k+1$ and $d_3(n)$ the number of divisors of the form $4k+3$. Jacobi gave similar formulas for 4, 6 and 8 squares. Ramanujan found all these, and much more of the same kind.

He also found Gauss's theorem that n is the sum of 3 squares except when it is of the form

$$4^a(8k + 7), \tag{22}$$

but I do not attach much importance to this. The theorem is quite easy to guess and difficult to prove. All known proofs depend upon the general theory of ternary forms, of which Ramanujan knew nothing, and I agree with Professor Dickson in thinking it very unlikely that he possessed one. In any case he knew nothing about the number of representations.

Ramanujan, then, before he came to England, had added comparatively little to the theory of numbers; but no one can understand him who does not understand his passion for numbers in themselves. I wrote before

"He could remember the idiosyncrasies of numbers in an almost uncanny way. It was Littlewood who said that every positive integer was one of Ramanujan's personal friends. I remember going to see him once when he was lying ill in Putney. I had ridden in taxi-cab No. 1729, and remarked that the number seemed to me rather a dull one, and that I hoped that it was not an unfavorable omen. 'No,' he replied, 'it is a very interesting number; it is the smallest number expressible as a sum of two cubes in two different ways.'* I asked him, naturally, whether he could tell me the solution of the corresponding problem for fourth powers; and he replied, after a moment's thought, that he knew no obvious example, and supposed that the first such number must be very large." †

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* $1729 = 12^3 + 1^3 = 10^3 + 9^3$.

† The smallest known is Euler's example

$$635318657 = 158^4 + 59^4 = 134^4 + 133^4.$$

In algebra, Ramanujan's main work was concerned with hypergeometric series and continued fractions (I use the word algebra, of course, in its old-fashioned sense). These subjects suited him exactly, and here he was unquestionably one of the great masters. There are three now famous identities, the "Dougall-Ramanujan identity"

$$(23)\qquad \sum_{n=0}^{\infty}(-1)^n(s+2n)\frac{s^{(n)}}{1^{(n)}}\frac{(x+y+z+u+2s+1)^{(n)}}{(x+y+z+u+s)_{(n)}}\prod_{x,y,z,u}\frac{x_{(n)}}{(x+s+1)^{(n)}}$$
$$=\frac{s}{\Gamma(s+1)\Gamma(x+y+z+u+s+1)}\prod_{x,y,z,u}\frac{\Gamma(x+s+1)\Gamma(y+z+u+s+1)}{\Gamma(z+u+s+1)},$$

where

$$a^{(n)} = a(a+1)\cdots(a+n-1),\qquad a_{(n)} = a(a-1)\cdots(a-n+1),$$

and the "Rogers-Ramanujan identities"

$$(24)\qquad \begin{aligned} 1+\frac{q}{1-q}+\frac{q^4}{(1-q)(1-q^2)}+\frac{q^9}{(1-q)(1-q^2)(1-q^3)}+\cdots &= \frac{1}{(1-q)(1-q^6)\cdots(1-q^4)(1-q^9)\cdots},\\ 1+\frac{q^2}{1-q}+\frac{q^6}{(1-q)(1-q^2)}+\frac{q^{12}}{(1-q)(1-q^2)(1-q^3)}+\cdots &= \frac{1}{(1-q^2)(1-q^7)\cdots(1-q^3)(1-q^8)\cdots},\end{aligned}$$

in which he had been anticipated by British mathematicians, and about which I shall speak in other lectures. As regards hypergeometric series one may say, roughly, that he rediscovered the formal theory, set out in Bailey's tract, as it was known up to 1920. There is something about it in Carr, and more in Chrystal's *Algebra*, and no doubt he got his start from that. The four formulas (1)–(4) are highly specialized examples of this work.

His masterpiece in continued fractions was his work on

$$(25)\qquad \frac{1}{1+}\,\frac{x}{1+}\,\frac{x^2}{1+\cdots},$$

which includes the theorems (10)–(12). The theory of this fraction depends upon the Rogers-Ramanujan identities, in which he had been anticipated by Rogers, but he had gone beyond Rogers in other ways and the theorems which I have quoted are his own. He had many other very general and very beautiful formulas, of which formulas like Laguerre's

$$(26)\qquad \frac{(x+1)^n-(x-1)^n}{(x+1)^n+(x-1)^n}=\frac{n}{x+}\,\frac{n^2-1}{3x+}\,\frac{n^2-2^2}{5x+\cdots}$$

are extremely special cases. Watson* has recently published a proof of the most imposing of them.

It is perhaps in his work in these fields that Ramanujan shows at his very best. I wrote before

"It was his insight into algebraical formulae, transformation of infinite series, and so forth, that was most amazing. On this side most certainly I have never met his equal, and I can compare him only with Euler or Jacobi. He worked, far more than the majority of modern mathematicians, by induction from numerical examples; all his congruence properties of partitions, for example, were discovered in this way. But with his memory, his patience, and his power of calculation he combined a power of generalization, a feeling for form, and a capacity for rapid modification of his hypotheses, that were often really startling, and made him, in his own peculiar field, without a rival in his day."

I do not think now that this extremely strong language is extravagant. It is possible that the great days of formulas are finished, and that Ramanujan ought to have been born 100 years ago; but he was by far the greatest formalist of his time. There have been a good many more important, and I suppose one must say greater, mathematicians than Ramanujan during the last 50 years, but not one who could stand up to him on his own ground. Playing the game of which he knew the rules, he could give any mathematician in the world fifteen.

In analysis proper Ramanujan's work is inevitably less impressive, since he knew no theory of functions, and you cannot do real analysis without it, and since the formal side of the integral calculus, which was all that he could learn from Carr or any other book, has been worked over so repeatedly and so intensively. Still, Ramanujan rediscovered an astonishing number of the most beautiful analytic identities. Thus the functional equation for the Riemann Zeta-function

$$\zeta(s) = \sum_{n=1}^{\infty} \frac{1}{n^s},$$

namely

$$\zeta(1-s) = 2(2\pi)^{-s}\cos\tfrac{1}{2}s\pi\;\Gamma(s)\zeta(s), \tag{27}$$

stands (in an almost unrecognizable notation) in the notebooks. So does Poisson's summation formula

$$\alpha^{1/2}\{\tfrac{1}{2}\phi(0) + \phi(\alpha) + \phi(2\alpha) + \cdots\} = \beta^{1/2}\{\tfrac{1}{2}\psi(0) + \psi(\beta) + \psi(2\beta) + \cdots\}, \tag{28}$$

where

$$\psi(x) = \sqrt{\left(\frac{2}{\pi}\right)}\int_0^{\infty}\phi(t)\cos xt\,dt$$

and $\alpha\beta = 2\pi$; and so also does Abel's† functional equation

* G. N. Watson, *Proceedings of the Cambridge Philosophical Society*, vol. 31, 1935, p. 7.

† The equation was rediscovered by Rogers and is attributed to him in the *Papers* (p. 337); but it is to be found in a posthumous fragment of Abel (*Œuvres*, t.2., p. 193).

$$(29) \qquad L(x) + L(y) + L(xy) + L\left(\frac{x(1-y)}{1-xy}\right) + L\left(\frac{y(1-x)}{1-xy}\right) = 3L(1)$$

for

$$L(x) = \frac{x}{1^2} + \frac{x^2}{2^2} + \frac{x^3}{3^2} + \cdots .$$

He had most of the formal ideas which underlie the recent work of Watson and of Titchmarsh and myself on "Fourier kernels" and "reciprocal functions"; and he could of course evaluate any evaluable definite integral. There is one particularly interesting formula, viz.

$$(30) \qquad \int_0^\infty x^{s-1}\{\phi(0) - x\phi(1) + x^2\phi(2) - \cdots\}dx = \frac{\pi\phi(-s)}{\sin s\pi},$$

of which he was especially fond and made continual use. This is really an "interpolation formula", which enables us to say, for example, that, under certain conditions, a function which vanishes for all positive integral values of its argument must vanish identically. I have never seen this formula stated explicitly by any one else, though it is closely connected with the work of Mellin and others.

I have left till last the two most intriguing sides of Ramanujan's early work, his work on elliptic functions and in the analytic theory of numbers. The first is probably too specialized and intricate for anyone but an expert to understand, and I shall say nothing about it now. The second subject is still more difficult (as anyone who has read Landau's book on primes or Ingham's tract will know), but anyone can understand roughly what the problems of the subject are, and any decent mathematician can understand roughly why they defeated Ramanujan. For this was Ramanujan's one real failure; he showed, as always, astonishing imaginative power, but he proved next to nothing, and a great deal even of what he imagined was false.

Here I am obliged to interpolate some remarks on a very difficult subject, *proof* and its importance in mathematics. All physicists, and a good many quite respectable mathematicians, are contemptuous about proof. I have heard Professor Eddington, for example, maintain that proof, as pure mathematicians understand it, is really quite uninteresting and unimportant, and that no one who is really certain that he has found something good should waste his time looking for a proof. It is true that Eddington is inconsistent, and has sometimes even descended to proof himself. It is not enough for him to have direct knowledge that there are exactly

$$136 \cdot 2^{256}$$

protons in the universe; he cannot resist the temptation of proving it; and I cannot help thinking that the proof, whatever it may be worth, gives him a certain amount of intellectual satisfaction. His apology would no doubt be that "proof" means something quite different for him from what it means for a pure mathematician, and in any case we need not take him too literally. But the opinion which I have attributed to him, and with which I am sure that almost all physicists agree at the bottom of their hearts, is one to which a mathematician ought to have some reply.

I am not going to get entangled in the analysis of a particularly prickly concept, but I think that there are a few points about proof where nearly all mathematicians are agreed. In the first place, even if we do not understand exactly what proof is, we can, in ordinary analysis at any rate, recognise a proof when we see one. Secondly, there are two different motives in any presentation of a proof. The first motive is simply to secure conviction. The second is to exhibit the conclusion as the climax of a conventional pattern of propositions, a sequence of propositions whose truth is admitted and which are arranged in accordance with rules. These are the two ideals, and experience shows that, except in the simplest mathematics, we can hardly ever satisfy the first ideal without also satisfying the second. We may be able to recognise directly that 5, or even 17, is prime, but nobody can convince himself that

$$2^{127} - 1$$

is prime except by studying a proof. No one has ever had an imagination so vivid and comprehensive as that.

A mathematician usually discovers a theorem by an effort of intuition; the conclusion strikes him as plausible, and he sets to work to manufacture a proof. Sometimes this is a matter of routine, and any well-trained professional could supply what is wanted, but more often imagination is a very unreliable guide. In particular this is so in the analytic theory of numbers, where even Ramanujan's imagination led him very seriously astray.

There is a striking example, which I have very often quoted, of a false conjecture which seems to have been endorsed even by Gauss and which took about 100 years to refute. The central problem of the analytic theory of numbers is that of the distribution of the primes. The number $\pi(x)$ of primes less than a large number x is approximately

$$\frac{x}{\log x}; \tag{31}$$

this is the "Prime Number Theorem", which had been conjectured for a very long time, but was never established properly until Hadamard and de la Vallée-Poussin proved it in 1896. The approximation errs by defect, and a much better one is

$$\mathrm{Li}\, x = \int_2^x \frac{dt}{\log t}. \tag{32}$$

In some ways a still better one is

$$\text{Li}\,x - \tfrac{1}{2}\text{Li}\,x^{1/2} - \tfrac{1}{3}\text{Li}\,x^{1/3} - \tfrac{1}{5}\text{Li}\,x^{1/5} + \tfrac{1}{6}\text{Li}\,x^{1/6} - \tfrac{1}{7}\text{Li}\,x^{1/7} + \cdots \tag{33}$$

(we need not trouble now about the law of formation of the series). It is extremely natural to infer that

$$\pi(x) < \text{Li}\,x, \tag{34}$$

at any rate for large x, and Gauss and other mathematicians commented on the high probability of this conjecture. The conjecture is not only plausible but is supported by *all* the evidence of the facts. The primes are known up to 10,000,000, and their number at intervals up to 1,000,000,000, and (34) is true for every value of x for which data exist.

In 1912 Littlewood proved that the conjecture is false, and that there are an infinity of values of x for which the sign of inequality in (34) must be reversed. In particular, there is a number X such that (34) is false for some x less than X. Littlewood proved the existence of X, but his method did not give any particular value, and it is only very recently that an admissible value, viz.

$$X = 10^{10^{10^{34}}},$$

was found by Skewes.* I think that this is the largest number which has ever served any definite purpose in mathematics.

The number of protons in the universe is about

$$10^{80}.$$

The number of possible games of chess is much bigger, perhaps

$$10^{10^{50}}$$

(in any case a second order exponential). If the universe were the chessboard, the protons the chessmen, and any interchange in the position of two protons a move, then the number of possible games would be something like the Skewes number. However much the number may be reduced by refinements on Skewes' argument, it does not seem at all likely that we shall ever know a single instance of the truth of Littlewood's theorem.

This is an example in which the truth has defeated not only all the evidence of the facts and of common sense but even a mathematical imagination so powerful and profound as that of Gauss; but of course it is taken from the most difficult parts of the theory. No part of the theory of primes is really easy, but up to a point simple arguments, although they will prove very little, do not actually mislead us. For example, there are simple arguments which might lead any good mathematician to the conclusion†

$$\pi(x) \sim \frac{x}{\log x} \tag{35}$$

* S. Skewes, *Journal of the London Mathematical Society*, vol. 8, 1933, p. 277.

† $f(x) \sim g(x)$ means that the ratio f/g tends to unity.

of the Prime Number Theorem, or, what is the same thing, to the conclusion that

$$(36) \qquad p_n \sim n \log n,$$

where p_n is the n-th prime number.

In the first place, we may start from Euler's identity

$$(37) \qquad \prod_p \frac{1}{1-p^{-s}} = \frac{1}{(1-2^{-s})(1-3^{-s})(1-5^{-s}) \cdots} = \frac{1}{1^s} + \frac{1}{2^s} + \frac{1}{3^s} + \cdots = \sum_n \frac{1}{n^s}.$$

This is true for $s > 1$, but both series and product become infinite for $s = 1$. It is natural to argue that, when $s = 1$, the series and the product should diverge in the same sort of way. Also

$$(38) \qquad \log \prod \frac{1}{1-p^{-s}} = \sum \log \frac{1}{1-p^{-s}} = \sum \frac{1}{p^s} + \sum \left(\frac{1}{2p^{2s}} + \frac{1}{3p^{3s}} + \cdots \right),$$

and the last series remains finite for $s = 1$. It is natural to infer that

$$\sum \frac{1}{p}$$

diverges like

$$\log \left(\sum \frac{1}{n} \right),$$

or, more precisely, that

$$(39) \qquad \sum_{p \leqq x} \frac{1}{p} \sim \log \left(\sum_{n \leqq x} \frac{1}{n} \right) \sim \log \log x$$

for large x. Since also

$$\sum_{n \leqq x} \frac{1}{n \log n} \sim \log \log x,$$

formula (39) indicates that p_n is about $n \log n$.

There is a slightly more sophisticated argument which is really simpler. It is easy to see that the highest power of a prime p which divides $x!$ is

$$\left[\frac{x}{p} \right] + \left[\frac{x}{p^2} \right] + \left[\frac{x}{p^3} \right] + \cdots,$$

where $[y]$ denotes the integral part of y. Hence

$$x! = \prod_{p \leqq x} p^{[x/p]+[x/p^2]+\cdots},$$

$$\log x! = \sum_{p \leqq x}\left(\left[\frac{x}{p}\right] + \left[\frac{x}{p^2}\right] + \cdots\right) \log p. \tag{40}$$

The left-hand side of (40) is practically $x \log x$, by Stirling's Theorem. As regards the right-hand, one may argue; squares, cubes, . . . of primes are comparatively rare, and the terms involving them should be unimportant, and it should also make comparatively little difference if we replace $[x/p]$ by x/p. We thus infer that

$$x\sum_{p \leqq x} \frac{\log p}{p} \sim x \log x, \qquad \sum_{p \leqq x} \frac{\log p}{p} \sim \log x,$$

and this again just fits the view that p_n is approximately $n \log n$.

This is broadly the argument used, naturally in a less naïve form, by Tchebychef, who was the first to make substantial progress in the theory of primes, and I imagine that Ramanujan began by arguing in the same sort of way, though there is nothing in the note-books to show. All that is plain is that Ramanujan found the form of the Prime Number Theorem for himself. This was a considerable achievement; for the men who had found the form of the theorem before him, like Legendre, Gauss, and Dirichlet, had all been very great mathematicians; and Ramanujan found other formulas which lie still further below the surface. Perhaps the best instance is (15). The integral is better replaced by the simpler function (16), but what Ramanujan says is correct as it stands and was proved by Landau in 1909; and there is nothing obvious to suggest its truth.

The fact remains that hardly any of Ramanujan's work in this field had any permanent value. The analytic theory of numbers is one of those exceptional branches of mathematics in which proof really is everything and nothing short of absolute rigour counts. The achievement of the mathematicians who found the Prime Number Theorem was quite a small thing compared with that of those who found the proof. It is not merely that in this theory (as Littlewood's theorem shows) you can never be sure of the facts without the proof, though this is important enough. The whole history of the Prime Number Theorem, and the other big theorems of the subject, shows that you cannot reach any real understanding of the structure and meaning of the theory, or have any sound instincts to guide you in further research, until you have mastered the proofs. It is comparatively easy to make clever guesses; indeed there are theorems, like "Goldbach's Theorem",* which have never been proved and which any fool could have guessed.

The theory of primes depends upon the properties of Riemann's function $\zeta(s)$, considered as an analytic function of the complex variable s, and in particu-

* "Any even number greater than 2 is the sum of two primes."

lar on the distribution of its zeros; and Ramanujan knew nothing at all about the theory of analytic functions. I wrote before

"Ramanujan's theory of primes was vitiated by his ignorance of the theory of functions of a complex variable. It was (so to say) what the theory might be if the Zeta-function had no complex zeros. His method depended upon a wholesale use of divergent series. . . . That his proofs should have been invalid was only to be expected. But the mistakes went deeper than that, and many of the actual results were false. He had obtained the dominant terms of the classical formulae, although by invalid methods; but none of them are such close approximations as he supposed.

"This may be said to have been Ramanujan's one great failure . . .",

and if I had stopped there I should have had nothing to add, but I allowed myself again to be led away by sentimentalism. I went on to argue that "his failure was more wonderful than any of his triumphs", and that is an absurd exaggeration. It is no use trying to pretend that failure is something else. This much perhaps we may say, that his failure is one which, on the balance, should increase and not diminish our admiration for his gifts, since it gives us additional, and surprising, evidence of his imagination and versatility.

But the reputation of a mathematician cannot be made by failures or by rediscoveries; it must rest primarily, and rightly, on actual and original achievement. I have to justify Ramanujan on this ground, and that I hope to do in my later lectures.

NOTE ON DEGREE OF APPROXIMATION TO AN INTEGRAL BY RIEMANN SUMS*

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In a number of different investigations it is desirable to approximate a Riemann integral by the corresponding Riemann sums. The object of the present note is to establish the most easily proved results on the degree of such approximation when equidistant ordinates are used, under various conditions on the function integrated.†

1. *Continuous functions.* We prove the following

THEOREM 1. *Let $f(x)$ be continuous in the interval $0 \leqq x \leqq 1$ and possess there the modulus of continuity $\omega(\delta)$ in the sense that for values x and x' in the interval* $(0, 1)$ *the inequality $|x-x'| \leqq \delta$ implies $|f(x)-f(x')| \leqq \omega(\delta)$. Then we have*

$$\left| \int_0^1 f(x)dx - \frac{1}{n}\sum_{k=1}^{n} f\left(\frac{k}{n}\right) \right| \leqq \omega\left(\frac{1}{n}\right). \tag{1}$$

* Presented to the American Mathematical Society, December 1936.

† Of course Theorems 1 and 2 are not to be regarded as novel. They are contained explicitly or implicitly in various treatments of the definite integral. See for instance Veblen and Lennes, Infinitesimal Analysis, New York, 1907, pp. 157–159. But Theorem 5 is believed to be new, and of some interest.