A lot has been written about Ramanujan's unconventional story and how his talent found an expression through G H Hardy who invited Ramanujan to England, and collaborated with him. The third leg of the triangle at that time was Littlewood. This review by Littlewood of Ramanujan's collected papers published after the latter's death is unusual. Littlewood takes occasion to make enlightening remarks about what he thought of specific talents of Ramanujan and how some of these were nurtured by Hardy. Littlewood mentions some insightful comments in his unusual review after prefacing them by "To do it justice I must infringe a little the rules about collaboration." This is a very revealing review of historical value as well.

B Sury

COLLECTED PAPERS OF SRINIVASA RAMANUJAN

Edited By G H Hardy, P V Seshu Atgar and B M Wilson
Pp. xxxvi+355, 30s net. 1927. (Cam. Univ. Press.)

Ramanujan was born in India in December 1887, came to Trinity College, Cambridge, in April 1914, was ill from May 1917 onwards, returned to India in February 1919, and died in April 1920. He was a Fellow of Trinity and a Fellow of the Royal Society.

Ramanujan had no university education, and worked unaided in India until he was twenty-seven. When he was sixteen he came by chance on a copy of Carr's *Synopsis of Mathematics*; and this book, now sure of an immortality its author can hardly have dreamt of, woke him quite suddenly to full activity. A study of its contents is indispensable to any considered judgment. It gives a very full account of the purely formal side of the integral calculus, containing, for example, Parseval's formula, Fourier's repeated integral and other "inversion formulae", and a number of formulae of the type recognizable by the expert under the general description "$f(\alpha) = f(\beta)$ if $\alpha\beta = \pi^2$". There is also a section on the transformation of power series

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into continued fractions. Ramanujan somehow acquired also an effectively complete knowledge of the formal side of the theory of elliptic functions (not in Carr). The matter is obscure, but this, together with what is to be found in, say, Chrystal's *Algebra*, seems to have been his complete equipment in analysis and theory of numbers. It is at least certain that he knew nothing of existing methods of working with divergent series, nothing of quadratic residuacity, nothing of work on the distribution of primes (he may have known Euler's formula $\prod(1-p^{-s})^{-1} = \sum n^{-s}$, but not any account of the ζ-function). Above all, he was totally ignorant of Cauchy's theorem and complex function theory. (This may seem difficult to reconcile with his complete knowledge of elliptic functions. A sufficient, and I think a necessary, explanation would be that Greenhill's very odd and individual *Elliptic Functions* was his text-book.)

The work he published during his India period did not represent his best ideas, which he was probably unable to expound to the satisfaction of editors. At the beginning of 1914, however, a letter from Ramanujan to Mr. Hardy (then, at Trinity, Cambridge) gave unmistakable evidence of his powers, and he was brought to Trinity, where he had three years of health and activity. (Some characteristic work, however, belongs to his two years of illness.)

I do not intend to discuss here in detail the work for which Ramanujan was solely responsible (a very interesting estimate is given by Prof. Hardy, p.xxxiv). If we leave out of account for the moment a famous paper written in collaboration with Hardy, his definite contributions to mathematics, substantial and original as they are, must, I think, take second place in general interest to the romance of his life and mathematical career, his unusual psychology, and above all to the fascinating problem of how great a mathematician he might have become in more fortunate circumstances. In saying this, of course, I am adopting the highest possible standard, but no other is appropriate.

Ramanujan's great gift is a "formal" one; he dealt in "formulae". To be quite clear what is meant, I give two examples (the second is at random, the first is one of supreme beauty):

$$p(4) + p(9)x + p(14)x^2 + \cdots = 5\frac{\{(1-x^5)(1-x^{10})(1-x^{15})\cdots\}^5}{\{(1-x)(1-x^2)(1-x^3)\cdots\}^6} \ ,$$

where $p(n)$ is the number of partitions of n;

$$\int_0^\infty \frac{\cos \pi x}{\{\Gamma(a+x)\Gamma(a-x)\}^2} dx = \frac{1}{4\Gamma(2a-1)\{\Gamma(a)\}^2} \ (a > \tfrac{1}{2}).$$

But the great day of formulae seems to be over. No one, if we are again to take the highest standpoint, seems able to discover a radically new type, though Ramanujan comes near it in his work on partition series; it is futile to multiply examples in the spheres of Cauchy's theorem and elliptic function theory, and some general theory dominates, if in a less degree, every other field. A hundred years or so ago his powers would have had ample scope. Discoveries alter the general mathematical atmosphere and have very remote effects, and we are not prone to attach great weight to rediscoveries, however independent they seem. How much are we to allow for this; how great a mathematician might Ramanujan have been 100 or 150 years ago; what would have happened if he had come into touch with Euler at the right moment? How much does lack of education matter? Was it formulae or nothing, or did he develop in the direction he did only because of Carr's book—after all, he learned later to do new things well, and at an age mature for an Indian? Such are the questions Ramanujan raises; and everyone has now the material to judge them. The letters and the lists of results announced without proof are the most valuable evidence available in the present volume; they suggest, indeed, that the note-books would give an even more definite picture of the essential Ramanujan, and it is very much to be hoped that the editors' project of publishing them in *extenso* will eventually be carried out.

Carr's book quite plainly gave Ramanujan both a general direction and the germs of many of his most elaborate developments. But even with these partly derivative results one is impressed by his extraordinary profusion, variety, and power. There is hardly a field of formulae, except that of classical number-theory, that he has not enriched, and in which he has not revealed unsuspected possibilities. The beauty and singularity of his results is entirely uncanny. Are they odder than one would expect things selected for oddity to be? The moral seems to be that we never expect enough; the reader at any rate experiences perpetual shocks of delighted surprise. And if he will sit down to an unproved result taken at random, he will find, if he can prove it at all, that there is at lowest some "point", some odd or unexpected twist. Prof. Watson and Mr. Preece have begun the heroic task of working through the unproved statements; some of their solutions have appeared recently in the *Journal of the London Mathematical Society*, and these strongly encourage the opinion that a complete analysis of the note-books will prove very well worth while.

There can, however, be little doubt that the results showing the most striking originality and the deepest insight are those on the distribution of primes (see pp. xxii-xxv, xxvii, 351, 352). The problems here are not in origin formal at all; they concern approximative formulae for such things as the number of primes, or of integers expressible as a sum of two squares, less than a large number x; and the determination of the orders of the errors is a major part of the theory. The subject has a subtle function-theory side; it was inevitable that Ramanujan should fail here, and that his methods should lead him astray; he predicts the approximative formulae, but is quite wrong about the orders of the errors. These problems tax the last resources of analysis, took over a hundred years to solve, and were not solved at all before 1890; Ramanujan could not possibly have achieved complete success. What he did was to perceive that an attack on the problems could at least be begun on the formal side, and to reach a point at which the main results became plausible. The formulae do not in the least lie on the surface, and his achievement, taken as a whole, is most extraordinary.

If Carr's book gave him direction, it had at least nothing to do with his *methods*, the most important of which were completely original. His intuition worked in analogies, sometimes remote, and to an astonishing extent by empirical induction from particular numerical cases. Being without Cauchy's theorem, he naturally dealt much in transformations and inversions of order of double integrals. But his most important weapon seems to have been a highly elaborate technique of transformation by means of divergent series and integrals. (Though methods of this kind are of course known, it seems certain that his discovery was quite independent.) He had no strict logical justification for his operations. He was not interested in rigour, which for that matter is not of first-rate importance in analysis beyond the undergraduate state, and can be supplied, given a real idea, by any competent professional. 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. It is a minor indication of his quality that he can never have *missed* Cauchy's theorem. With it he could have arrived more rapidly and conveniently at certain of his results, but his own methods enabled him to survey the field with an equal comprehensiveness and as sure a grasp.

I must say something finally of the paper on partitions (pp. 276-309) written jointly with Hardy. The number $p(n)$ of the partitions of n increases rapidly with n, thus:

$$p(200) = 3972999029388.$$

The authors show that $p(n)$ is the integer nearest

$$\frac{1}{2\sqrt{2}} \sum_{q=1}^{2} \sqrt{q} A_q(n) \psi_q(n), \tag{1}$$

where $A_q(n) = \sum \omega_{p,q} e^{-2np\pi i/q}$, the sum being over p's prime to q and less than it, $\omega_{p,q}$ is a certain $24q$th root of unity, ν is of the order of $\sqrt{n}$, and

$$\psi_q(n) = \frac{d}{dn}(\exp\{C\sqrt{(n-\tfrac{1}{24})}/q\}),\ C = \pi\sqrt{\tfrac{2}{3}}.$$

We may take $\nu = 4$ when $n = 100$. For $n = 200$ we may take $\nu = 5$; five terms of the series (1) predict the correct value of $p(200)$. We may always take $\nu = a\sqrt{n}$ (or rather its integral part), where a is any positive constant we please, provided n exceeds a value $n_0(a)$ depending only on a.

The reader does not need to be told that this is a very astonishing theorem, and he will readily believe that the methods by which it was established involve a new and important principle, which has been found very fruitful in other fields. The story of the theorem is a romantic one. (To do it justice I must infringe a little the rules about collaboration. I therefore add that

Prof. Hardy confirms and permits my statements of bare fact.) One of Ramanujan's Indian conjectures was that the first term of (1) was a very good approximation to $p(n)$; this was established without great difficulty. At this stage the $n - \frac{1}{24}$ was represented by a plain n—the distinction is irrelevant. From this point the real attack begins. The next step in development, not a very great one, was to treat (1) as an "asymptotic" series, of which a fixed number of terms (*e.g.* $\nu = 4$) were to be taken, the error being of the order of the next term. But from now to the very end Ramanujan always insisted that much more was true than had been established: "there must be a formula with error $O(1)$." This was his most important contribution; it was both absolutely essential and most extraordinary. A severe numerical test was now made, which elicited the astonishing facts about $p(100)$ and $p(200)$. Then ν was made a function of n; this *was* a very great step, and involved new and deep function-theory methods that Ramanujan obviously could not have discovered by himself. The complete theorem thus emerged. But the solution of the final difficulty was probably impossible without one more contribution from Ramanujan, this time a perfectly characteristic one. As if its analytical difficulties were not enough, the theorem was entrenched also behind almost impregnable defences of a purely formal kind. The form of the function $\psi_q(n)$ is a kind of indivisible unit; among many asymptotically equivalent forms it is essential to select exactly the right one. Unless this is done at the outset, and the $-\frac{1}{24}$ (to say nothing of the $\frac{d}{dn}$) is an extraordinary stroke of formal genius, the complete result can never come into the picture at all. There is, indeed, a touch of real mystery. If only we *knew* there was a formula with error $O(1)$, we might be forced, by slow stages, to the correct form of ψ_q. But why was Ramanujan so certain there *was* one? *Theoretical* insight, to be the explanation, had to be of an order hardly to be credited. Yet it is hard to see what numerical instances could have been available to suggest so strong a result. And unless the form of ψ_q was known already, no numerical evidence could suggest anything of the kind—there seems no escape, at least, from the conclusion that the discovery of the correct form was a single stroke of insight. We owe the theorem to a singularly happy collaboration of two men, of quite unlike gifts, in which each contributed the best, most characteristic, and most fortunate work that was in him. Ramanujan's genius did have this one opportunity worthy of it.

The volume contains a biography by the second of the editors, and the obituary notice by Prof. Hardy. These give quite a vivid picture of Ramanujan's interesting and attractive personality. The mathematical editors have done their work most admirably. It is very unobtrusive; the reader is told what he wants to know at exactly the right moment, and more thought and bibliographical research must have gone into it than he is likely to suspect.

J. E. LITTLEWOOD

Nature, stretching Horace Davenport out, had forgotten to stretch him sideways, and one could have pictured Euclid, had they met, nudging a friend and saying: "Don't look now, but this chap coming along illustrates exactly what I was telling you about a straight line having length without breadth"

– P.G.Wodehouse.

"Every mathematician worthy of the name has experienced ... the state of lucid exaltation in which one thought succeeds another as if miraculously... this feeling may last for hours at a time, even for days. Once you have experienced it, you are eager to repeat it but unable to do it at will, unless perhaps by dogged work"

–Andre Weil.

"I have often pondered over the roles of knowledge or experience, on the one hand, and imagination or intuition, on the other, in the process of discovery. I believe that there is a certain fundamental conflict between the two, and knowledge, by advocating caution, tends to inhibit the flight of imagination. Therefore, a certain naivete, unburdened by conventional wisdom, can sometimes be a positive asset"

–Harish-Chandra.

"A person who can, within a year, solve x^2 - 92 y^2 = 1 is a mathematician"

–Brahmagupta