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SRINIVASA RAMANUJAN MEDAL LECTURE–1979

NUMBER THEORY—MANY CHALLENGES, SOME ACHIEVEMENTS

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MR PRESIDENT, Distinguished Fellows of the Academy, and Friends !

For an Indian mathematician, there cannot be a greater honour than the award of an Academy medal named after the great Srinivasa Ramanujan. Ramanujan's life has provided inspiration to generations of Indian and non-Indian mathematicians since the early part of this century. His example not only gave a tremendous boost to Indian Science, when the country was still far from freedom, but the continued interest in his work all over the mathematical world demonstrates the fact that his impact has not only been deep but also long lasting. That you have considered me worthy of the honour fills me with deep gratitude and great humility. Thank you very much.

I. RAMANUJAN'S FUNCTION $\tau(n)$

I would like to start the talk with Ramanujan's function $\tau(n)$. I was introduced to mathematical research in 1946 by the great Professor S. Chowla through our study of some properties of this function. There has been considerable development in the study of this function in recent years culminating in the proof of a conjecture of Ramanujan by P. Deligne in 1974. This proof earned for Deligne the prestigious *Field Medal* at the *International Congress of Mathematicians*, 1978. At the *International Congress of Mathematicians*, which meets once every four years, one to four Field Medals are awarded to Mathematicians below the age of 40, whose work has been considered most significant. Deligne was awarded this medal in 1978 for his work on Ramanujan's conjecture and related topics.

Lagrange proved in 1770 that every natural number is a sum of four squares. One also knows which numbers are sums of two or three squares. When one knows that every number is a sum of four or more squares, it is natural to ask how many ways such a representation is possible. To be precise, we define $r_s(n)$ to be the number of ways n can be expressed as a sum of s squares and ask for the evaluation of $r_s(n)$ for various $s \geqslant 4$. Ramanujan evaluated $r_{24}(n)$ and found that

$$r_{24}(n) = \frac{16}{691} \sigma^*_{11}(n) + e_{24}(n),$$

where $\sigma_{11}^*(n) = (-1)^n \Sigma_{d/n} (-1)^d d^{11}$ is a known function, while $e_{24}(n)$ is a combination of values of the function $\tau(n)$ introduced by Ramanujan, as follows :

$\tau(n)$ is the coefficient of x^n in

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$$g(x) = x\{(1-x)\ (1-x^2)\ \ldots.\}^{24} = \sum_{1}^{\infty} \tau(n)\ x^n\ ;$$

the best way to interpret the above is: for a given n, take the terms upto $1-x^n$ on the left hand side and then $\tau(n)$ is the coefficient of x^n in the resulting polynomial. The function $\tau(n)$ is known as Ramanujan's Tau function. G. H. Hardy in his tenth lecture on Ramanujan (pp. 161) delivered in 1940 remarked : "*I shall devote this lecture to a more intensive study of some of the properties of Ramanujan's functions $\tau(n)$, which are very remarkable and still imperfectly understood. We may seem to be straying into one of the backwaters of Mathematics, but the genesis of $\tau(n)$ as a coefficient in so fundamental a function compels us to treat it with respect.*" In the light of the work of various mathematicians like Mordell, Hecke, Petterson, Weil, Siegel, Rankin, Serre, Swinnerton-Dyer and Deligne, the function has close connections with theories of modular forms, elliptic curves, quadratic forms and so on and the function has continued to attract the attention of first rate mathematicians. Ramanujan himself was fascinated by the function, made extensive tables and put forth conjectures based on empirical evidence. He proved, among other things, that

$$\tau(7m + k) \equiv 0 \pmod 7) \text{ if } k = 0, 3, 5, 6,$$

$$\tau(23m + k) \equiv 0 (\text{mod } 23) \text{ if } k \text{ has one of 11 values,}$$

$$\tau(n) \equiv n\ \sigma(n)\ (\text{mod } 5)$$

and

$$\tau(n) \equiv \sigma_{11}(n)\ (\text{mod } 691),$$

where $\sigma_k(n) = \Sigma_{d/n}\ d^k$ = sum of kth powers of divisors of n, $\sigma(n) = \sigma_1(n)$.

Inspired by this, various people, including H. Gupta, K. G. Ramanathan, D. B. Lahiri, G. N. Watson, J. R. Wilton and others proved further results of the above type. Working with Professor Chowla in 1946 and 1947, the speaker proved together with Professor Chowla, various congruence properties of $\tau(n)$, typical ones being,

$$\tau(n) \equiv 5\ n^2 \sigma_7\ (n) - 4n\ \sigma_9\ (n)\ (\text{mod } 5^3) \text{ if } (n, 5) = 1,$$

$$\tau(n) \equiv (n^2 + k)\ \sigma_7\ (n)\ (\text{mod } 3^4), \text{ where } k = 0 \text{ or } 9,$$

$$\tau(n) \equiv \sigma_{11}(n)\ (\text{mod } 2^8) \text{ if } n \text{ is odd}$$

and

$$\tau(n) \equiv 0\ (\text{mod } 2^a\ 3^b\ 5^c\ 7^d\ 23^e\ 691^f),*$$

"for almost all" n and given a, b, c, d, e, f.

D. B. Lehiri, D. H. Lehmer, M. H. Ashworth and O. Kolberg have proved similar results for higher powers of some of these numbers; but all the modulii considered are powers of the primes 2, 3, 5, 7, 23 and 691. It has only recently become clear from the work of P. Swinnerton-Dyer, P. Deligne and J. P. Serre that con-

*This result of S. Chowla is stronger than a similar result of Bambah and Chowla (1947 *a, b*) gruences like the above can exist for powers of these primes only. There are indications that there are limitations on the powers involved also (cf. Swinnerton-Dyer, 1972).

While discussing the function $r_{24}(n)$, Hardy remarked that it is dominated by the term $\frac{16}{691}\sigma_{11}^*(n)$. To justify the remark, he showed that $\tau(n) = 0(n^8)$, i.e., $|\tau(n)| \leqslant C n^8$ for some constant $C > 0$. Since $\sigma_{11}^*(n)$ is of order n^{11} for large n, the remark is justified. Ramanujan proved that $\tau(n) = 0(n^7)$ and Hardy showed that $\tau(n) = 0(n^6)$. On the basis of his tables and remarkable intuition, Ramanujan conjectured that $\tau(n) = 0(n^{11/2+\epsilon})$ for every $\epsilon > 0$. This conjecture is known as Ramanujan's conjecture. R. A. Rankin proved in 1939 that $\tau(n) = 0(n^{6-1/5})$, and after A. Weil's results on solutions of Congruences (1948), which are connected with the so-called Kloosterman's sums, one knew that $\tau(n) = 0(n^{6-1/4+\epsilon})$ for all $\epsilon > 0$. It was only in 1974 that P. Deligne finally succeeded in proving the Ramanujan conjecture, and the more general Pettersen and Weil conjectures, combining formidable tools from Algebraic Geometry of Gröthendiek with the classical analysis of Rankin, Hardy and others.

Although, as already stated, S. Chowla has proved that $\tau(n) \equiv 0 \pmod{2^a\, 3^b\, 5^c\, 7^d\, 23^e\, 691^f}$ for almost all n, D. H. Lehmer (1947) showed that $\tau(n) \neq 0$ for $n < 3{,}316{,}799$. He also proved that if $\tau(n) = 0$, n is a prime. The lower bound of zeros of $\tau(n)$ can be increased in view of later results, e.g., it is known that $\tau(n) \neq 0$ if $n < 214, 928, 639, 999$. It is generally believed that $\tau(n)$ is never zero and the belief is commonly called Lehmer's conjecture. However, no one seems to know how to prove this conjecture if it is true.

II. Goldbach Conjecture

Before going on to areas where my colleagues and I have made some contributions in recent years, I would like to talk about an old challenge, where a great deal of progress has been made without, unfortunately, meeting the complete challenge.

In a letter written to Euler in 1742, Goldbach made the conjecture that every even number greater than 2 is a sum of two primes. Although the conjecture is mentioned in almost every textbook and a large amount of numerical evidence in its support has been collected, the conjecture has not been proved or disproved till today. In fact, there was no progress on the problem till 1920, when the first break-through took place. The Norwegian mathematician Viggo Brun developed his famous Sieve at that time and applied it to prove that every large enough even integer can be written as $p_9 + p_9$, where p_r is a number which is the product of at most r primes. The next breakthrough came in 1923, when Hardy and Littlewood showed that on the basis of an unproved hypothesis of the Riemann type, every large odd integer is a sum of three primes, so that every large even integer is a sum of four primes. As is well known, Riemann Hypothesis is believed to be one of the most difficult conjectures in the whole of Mathematics. Therefore, the Hardy-Littlewood result was based on a hypothesis which is likely to be extremely difficult to prove.

The next great advance came in 1930, when Schnirelmann developed his density results to prove the existence of a number k, such that every integer is a sum of at most k primes. Thus, in a sense, Schnirelmann was the first mathematician to prove a Goldbach type result. Thus between 1920 and 1930, approximations to Goldbach problem of three types were introduced. Since 1930, improvements have taken place in all the three directions. Although a large number of people have been involved in these improvements, I shall cite the latest results only in each direction. In 1937, using his modification of the circle method going back to Hardy, Littlewood and Ramanujan, the great I. M. Vinogradov proved the Hardy-Littlewood result without any unproved hypothesis, i.e., he proved unconditionally that every large odd integer is a sum of three primes. In the Brun direction, the latest result was proved by J. Chen in 1973, who proved that every large even integer is a $p + p_2$. In the Schnirelmann direction, the latest result appears to be that of R. C. Vaughan (1977), who has proved that every even number is a sum of atmost 26 primes. Thus in each direction, especially the results of Vinogradov and Chen, we have come almost as close as possible to the Goldbach conjecture without actually solving it.

III. Minkowski's Conjecture

Minkowski proved in 1899 the following result :

Let $L_1 = ax + by$, $L_2 = cx + dy$ be two real linear forms with det $(ad - bc) = \Delta \neq 0$. Then given any real α_1, α_2 there exist integers x, y, such that

$$| (L_1 + \alpha_1) (L_2 + \alpha_2) | \leqslant | \Delta | / 4.$$

Most people believe that Minkowski also thought an analogous result is true for n forms in n variables, and the following is known as Minkowski's conjecture.

Let

$$\begin{aligned} L_1 &= a_{11} x_1 + \dots + a_{1n} x_n \\ &\vdots \\ &\vdots \\ &\vdots \\ L_n &= a_{n1} x_1 + \dots + a_{nn} x_n \end{aligned}$$

be n real linear forms in n variables with det $(a_{ij}) = \Delta \neq 0$. Then given real $\alpha_1, \dots, \alpha_n$, there exist integers $x_1, \dots, x_n$ for which

$$| (L_1 + \alpha_1) \dots (L_n + \alpha_n) | \leqslant | \Delta | / 2^n.$$

One can easily see that such a result, if true, would be best possible. Also the result can be shown to be true for rational a_{ij}.

The Conjecture was proved for $n = 3$ by R. Remak in 1923 and the proof was simplified by H. Davenport in 1939, who gave a very short and elegant version. Other proofs have been given by Birch and Swinnerton-Dyer (1956) and Narzullaev (1975).

For $n = 4$, the conjecture was proved by F. J. Dyson in 1948. The Remak-Davenport-Dyson proof consists of two parts. For convenience, we shall call them Part A and Part B. The Dyson proof of Part B is elementary, but complicated. This has been simplified and extended upto $n = 6$ by A. C. Woods (1965*a*, *b*, 1972). Regarding Part A, the Dyson proof relied heavily on algebraic Topology and he made the remark : "*The proof borrows powerful weapons from the armory of Topology; purely geometrical methods seem quite inadequate.*" Most people seemed to agree with Dyson and there was no success in simplifying the proof. However, in the years 1972–74, B. F. Skubenko in USSR and A. C. Woods and the speaker, working together, independently produced two proofs which can be called simple and elementary. Skubenko also gave a proof for $n = 5$. However, the editor A. V. Malyshev stated in his foreword that "*the exposition as given here is highly incomplete and the paper should be regarded as an initial publication of the proof of this notable result.*" Alan Woods and the speaker, while trying to understand the proof, have recently given a proof on Skubenko lines, which we think is complete and considerably simpler in details. One can thus say with confidence that Minkowski's conjecture has been proved for $n \leqslant 5$. (Incidentally, the proof uses earlier work of A. C. Woods, B. J. Birch and P. Swinnerton-Dyer). The problem is open for $n \geqslant 6$.

When one is not able to prove a desired result, one often looks for weaker result in that direction. The first such success for Minkowski's general conjecture is due to Chebotoroff in 1934 who proved the solvability of the inequality $| \pi (L_i + \alpha_i) | \leqslant | \Delta | / (\sqrt{2})^n$ instead of $| \Delta | / 2^n$. Improvements were made by Mordell, Davenport, Woods and Bombieri who, using earlier ideas of Chebotoroff, Woods, Davenport and Siegel, proved the solvability of $| \pi(L_i + \alpha_i) | \leqslant | \Delta | / (\sqrt{2})^n C_n$, where $C_n \to (3 + 10^{-4}) (2e - 1)$ as $n \to \infty$. Skubenko (1977) has announced that he can prove the solvability of $| \pi(L_1 + \alpha_1) | \leqslant | \Delta | / (\sqrt{2})^n \sqrt{\frac{n}{\log^2 n}}\, e_n$, where $e_n \to 1/e^2$ as $n \to \infty$. The proof is still awaited.

When a problem attracts the attention of the mathematical community and stays unsolved for a long period, very often it generates a great deal of related work and throws up numerous other interesting problems. Minkowski's conjecture also has stimulated the generation of a great deal of exciting Mathematics and work has been done all over the world — in U.K., U.S.A., U.S.S.R., Austria, Germany, Australia, Canada, New Zealand, India, and in other places. In India, my colleagues and students, R. J. Hans-Gill, V. C. Dumir, V. Grover and Madhu Raka have made some nice contributions, many of which are in the course of publication at the moment.

IV. Packings and Coverings

In 1773, Lagrange proved the following result :

Let $f(x, y) = ax^2 + 2bxy + cy^2$ be a positive definite quadratic form with det $f = ac - b^2 = d$. Then there exist integers x, y, not both 0, such that $f(x, y) \leqslant \sqrt{4d/3}$. The result is best possible. Gauss (1831) extended the result to forms in three variables. Hermite (1850) showed that results of Lagrange-Gauss type have analogues for forms in n variables; however, he did not find the best possible results. Best possible results were found for $n = 4, 5$ by Korkine and Zolotoroff in 1870's and for $n = 6, 7, 8$ by Blichfeldt in years 1925, 1926 and 1934. The best results for $n \geqslant 9$ are not known. It was the attempt by Minkowski to prove the Hermite result that led to the development of Geometry of Numbers as an independent branch of Number Theory. Minkowski realised that these results have the following geometrical interpretation.

Let K be a sphere in the n dimensional Euclidean space. Let $\mathcal{F}$ be a family of translates of K. If no two members overlap, we say $\mathcal{F}$ is a packing of equal spheres. One can give a precise meaning to the notion of the proportion of the whole space occupied by these spheres, and call it the density of the packing. It is natural to say that a packing is better if its density is greater. If the centres of the spheres form a regular pattern called a lattice, the packing is called a lattice packing. The best results in quadratic form in n variables of Lagrange-Gauss-Hermite type correspond to the problem of finding the best lattice packings of n dimensional spheres. Minkowski observed that the fact that the density of a packing is at most one, itself gives a proof of Hermite's theorem with better estimates than those of Hermite. Thus the problem of finding the best lattice packings of spheres is of interest both from the arithmetical and geometrical point of view. As stated earlier, these best packings are known only for $n \leqslant 8$; for larger n, very good estimates from above and below have been obtained by C. A. Rogers and W. Schmidt; but the problem is still far from solved. Incidentally, this problem has close connections with some problems of error correcting codes.

Although from the number-theoretic point of view it is only the lattice packings that are of interest, from the geometrical point of view one can ask the natural question, *"Do the best lattice packings stay the best if the condition of the centres forming a lattice is relaxed ?"* In other words, can one find better packings of spheres than the best lattice ones ? Our intuition says it should not be so and most people believe our intuition is correct. The first proof that our intuition is correct for $n = 2$ was given by Thue in 1892. However, the first really complete proofs were produced only in the 1940's by L. Fejes Toth and independently by Mahler and B. Segre. The result was extended to the class of convex sets in the plane independently by C. A. Rogers and L. Fejes Toth in 1950. The problem for three dimensional spheres is still awaiting solution, i.e., one has no proof that no non-lattice packing of spheres is better than the best lattice one. Some progress has been made among others by C. A. Rogers and A. C. Woods; but the challenge is still far from met.

The mathematical theory of Coverings has similarities with packings and analogous questions for coverings have been studied during the last thirty years or so. Let K be an n dimensional body with a volume. Let $\mathcal{F}$ be a family of translates of K that cover the whole space. We say $\mathcal{F}$ is a covering of space by K. If the points of space corresponding to these translations form a lattice, we say $\mathcal{F}$ is a lattice covering by K. One can give a meaning to the concept of the proportion of the covering volume to that covered and call it the density of the covering. Then it is natural to say that if the density of a covering is less than that of another, the former is better than the latter. One is concerned with the questions of best lattice and the best general coverings by a given set K, say a sphere or a class of sets, say convex ones. Lattice coverings have number-theoretic interpretations and lead to results therein. As C. A. Rogers remarked in his book *"earlier results may lie concealed or merely ignored in the vast literature of Mathematics."* The earliest well-known result seems to be that of R. Kershner who proved in 1939 that for circles, no general covering is better than the best lattice one which has density $2\pi/3\sqrt{3}$. A paper by E. Hlawka in 1949 and some work of L. Fejes Toth about the time has led to a lot of interest in the covering problems from 1950 onwards. Regarding the best lattice coverings by spheres in n dimensions, those for $n = 3$ were determined by the speaker in 1954. Simpler proofs were given by E. S. Barnes and L. Few in 1956. The speaker gave in 1954 estimates for $n = 4$ and also made a conjecture. Barnes and Dickson in Australia and Delone, Ryskov and their associates in Russia independently developed theories which eventually led to the proof of the speaker's conjecture in 1964. The problems for $n = 5$ was solved by Ryskov and Baranovsky in 1975 and 1976. The problem is open for $n \geqslant 6$.

Coming to estimates for $\theta(K)$, the density of the best lattice coverings by K, earlier results due to Davenport, Watson, C. A. Rogers, R. P. Bambah and K. F. Roth were of the type $\theta(K) \leqslant C^n$, $C > 1$ for spheres and more general sets of convex bodies. However, in 1959–60 as a result of the work of W. Schmidt and C. A. Rogers, C. A. Rogers obtained the best known estimates $\theta\ (K) \leqslant C\,n\ (\log n)^{\frac{1}{2}\log_2 2\pi e}$ for spheres and $\theta(K) \leqslant n^{\log_2 \log_e n + c}$ for convex bodies. In the other direction, the first lower estimate for spheres was made by the speaker and H. Davenport, who proved in 1952 that $\theta(K) \geqslant 4/3 - \epsilon_n$, where $\epsilon_n \to 0$ as $n \to \infty$. In 1959, Coxeter, Few and Rogers proved $\theta(K) \geqslant \tau_n \sim n/e\sqrt{e}$. The gap between upper and lower estimates has not been reduced since then.

Regarding general coverings, Kershner's result that in the plane no covering circles is better than the best lattice one was extended to the more general system of symmetric convex domains in 1950's by L. Fejes Toth and jointly by C. A. Rogers and the speaker. Other proofs were given in 1960's by R. P. Bambah, C. A. Rogers and H. Zassenhaus and also by the speaker and A. C. Woods. That the result cannot be extended to the more general class of star sets has been shown by the speaker, V. C. Dumir and R. J. Hans-Gill in 1977. For three dimensions, the proof or disproof of the conjecture that no covering is better than the best lattice coverings by spheres seems to be a very hard problem. Partial results of Coxeter, Few and Rogers, as also Bambah and Woods are too far from the goal.

V. Sums of Two Squares

The speaker wishes to end the talk with an intriguing problem where the results one has been able to obtain are very weak compared to the suspected ones. In 1947, S. Chowla and the speaker proved :

Let s_n denote the nth integer, which is a sum of two squares. Then $s_{n+1} - s_n = 0(\ s_n^{1/4})$.

The proof was very elementary and most people believe $s_{n+1} - s_n = 0(s_n^{\epsilon})$ for every $\epsilon > 0$. In spite of the interest of many mathematicians, like Littlewood, Mordell, Erdös and others, no improvement in our weak result is available. C. Hooley (1971) has proved some results for the moments $\sum_{s_{n+1} \leqslant x} (s_{n+1} - s_n)^r, 0 \leqslant r < 5/3$.

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