SOME DEBTS I OWE

BY

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Abstract. The primary objects of this paper are:

(1) to acknowledge my debts to a number of important mathematicians who have passed away, and

(2) to describe some of the beginnings of several themes in my research.

1. Introduction. It is impossible for me to express adequately my gratitude to all who were involved with the 42^{nd} Seminaire Lotharingien held in Maratea, Italy during September 1–5, 1998. I am especially indebted to Dominique Foata who really made the trip possible for me and managed so many aspects of the conference so well. Also I extend special thanks to Peter Paule who originally suggested the role I would play. The surprise of being asked to participate in a conference to commemorate your 60^{th} birthday is quite overwhelming. It is impossible to feel worthy of such an event. However, I have always told others that such events were important for the cohesiveness of the mathematical community, and so it would hardly be consistent for me to back out when this event was proposed. After some thought, it occurred to me that, I might use my participation to honor some of those who are no longer with us and who influenced me and many others. I will follow that same general outline in this paper.

After a belated tribute to Marco Schützenberger, I will describe the beginnings of the paths that eventually led to: (1) Ramanujan's Lost Notebook, (2) Bailey Chains, (3) Determinant Evaluations, and (4) Partition Analysis. I will conclude with a look at one of my current paths which I have called the Liouville Mystery.

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2. Marco Schützenberger.

Schützenberger was a grand master of mathematics and a powerful and vivid presence. I had the good fortune to meet him at a number of European conferences, mostly at Oberwolfach. He had great style and great insight.

Let me elaborate with slight variations on two of the stories that began Dick Askey's tribute to Marco.

Dick started off by describing his first meeting with Marco. I was there with a different viewpoint. I knew both of them fairly well prior to this meeting. Each was a strong personality with passionately held views resulting from years of devoted research and study. I knew Dick also to be a committed non-smoker, and I knew of Marco's chain smoking. I also knew that I had arranged for Dick's invitation to the meeting: I believed that one of the world's premier workers in special functions would have valuable interactions with the world of enumerative combinatorics. So I watched this meeting with some anxiety. Within minutes of the beginning of the conversation Marco introduced a discussion of X (a famous European mathematician) whom he described as "the cross I bear!" Unknown to Marco, Dick had had a very serious confrontation with X over fundamental principles dear to his heart. From that moment on, my anxiety vanished. I am convinced that (if asked) Dick would have happily lit Marco's cigarettes for the rest of the afternoon. They agreed on much more than X, but X was the perfect starting point.

In Dick's second story concerning a flawed combinatorial proof of a famous result, I remember the conversation at the end a bit differently. Dick, Marco and I were seated near each other. At the conclusion of the talk Marco asked what we thought. I replied that I would reserve judgement. He responded: "You reserve judgement on THAT? You make me doubt that the result is even true. When I return to Paris I will check it on the computer."

Most meaningful were the many ways Marco attempted both to educate and encourage many people (including me) over many years. I recall his gracious letter enthusiastically welcoming my edited publication of MacMahon's collected papers. I remember how he began a conversation with me on my long memoir devoted to a topic I had named "partition ideals." He said "You do not write well." He went on to explain exactly what he meant, and by the end of the evening had convinced me of the flaws in my exposition.

I vividly remember a letter from him which he sent to me at a low point in my career. I know I carefully saved the letter. In fact, I so carefully saved it that I have hunted for it for two years without success. So I will paraphrase it from memory. The meaning is preserved but his eloquence is gone. It went something like this:

Do not be too concerned about your current disappointment. The best that any mathematician can really hope for is to prove some first class theorems *and have them understood and appreciated by a few good mathematicians. Everything else is politics.*

His advice in this letter is something I have thought about at numerous times and in numerous settings.

We will all miss his wisdom.

3. The Path to Ramanujan's Lost Notebook.

One of the most remarkable events in my career was the discovery of Ramanujan's Lost Notebook in the Wren Library of Trinity College, Cambridge. I have provided lengthy accounts of that event elsewhere [18], [21; pp. 5–6]. Prior to 1976, the only information anyone had about Ramanujan's work in 1919–1920 (the last year of his life) was contained in a letter on a new topic, mock theta functions, which Ramanujan sent to Hardy early in 1920. The letter was published as the last two pages in Ramanujan's Collected Papers [50; pp. 354–355]. Suffice it to say that what I needed at the time of the discovery was a deep knowledge of Ramanujan's mock theta functions. It was the extensive list of formulas for these functions in the Lost Notebook that made clear to me that I had found the lost discoveries made by Ramanujan during the last year of his life.

As incredible as it may seem, I learned all about the mock theta functions in graduate school at the University of Pennsylvania. My thesis advisor was Hans Rademacher, the famous German number theorist, who had emigrated to the U. S. in the 1930's. His work on partitions and modular forms had led him to Ramanujan, and he had asked his student, Leila Dragonette, to study the third order mock theta functions for her Ph.D. thesis [41]. Rademacher told me how pleased he was with her work, but he felt that it could be greatly improved. So he set me to work to improve it [6].

Simultaneously, Nathan Fine gave a course entitled: Basic Hypergeometric Functions. I signed up for it mistakenly thinking that "basic" meant "beginning" or "elementary." I soon found out that "basic" meant "q"; however, Fine was sufficiently mesmerizing that I never regretted my error [25].

Rademacher's assigned project for me consisted of: (1) determining the behavior of the third order mock theta functions under the transformations of the modular group, and (2) applying the celebrated circle method to obtain asymptotic series for the power series coefficient of the third order mock theta functions. He believed that the combination of these two projects would yield improvements on Dragonette's results, and this turned out to be the case [6]. This project was immensely interesting to me, but it did not provide me with a real feel for the inner workings of the mock theta functions. This was to be provided by Fine's course. His course was based on a manuscript he had been perfecting for a decade; it eventually became a book [42]. He covered the first chapters of his book. In this material, he discussed the third order mock theta functions at length. Realizing that Rademacher had assigned me a thesis project on mock theta functions, Fine asked me to present an account of the fifth order mock theta functions from Watsons second paper [57]. So I plunged in. Of this paper, Watson had once said: "The basic hypergeometric series which has been used hitherto is of no avail for these functions, and other means must be sought to establish Ramanujan's relations which connect functions of order 5. After spending a fortnight on fruitless attempts, I proceeded to attack the problem by the most elementary methods available, namely applications of Euler's formulae mingled with rearrangements of repeated series; and within the day I had proved not only the five relations set out by Ramanujan but also five other relations whose existence he had merely stated. My proofs of these relations are all so long that I took the trouble to analyse one of the longest in the hope of being able to say that it involved "thirty-nine steps"; it was, however, disappointing to a student of John Buchan to find that a moderately liberal count revealed only twenty-four." I suspect I was one of the first to read this paper carefully. I was able to extend Watson's methods in [3], [4] and [5]. The upshot of this exercise was that I emerged from graduate school with an intimate familiarity with mock theta functions on an almost individual basis.

A little over a decade later in 1976, I was invited to participate in the conference, Combinatoire et Represéntation du Groupe Symétrique, a Table Ronde organized by Dominique Foata at the University of Strasbourg. The then current manifestation of airline ticket fare irrationality was that if you stayed in Europe for at least three weeks, your fare was miniscule. Because of this financial incentive, the University of Wisconsin (where I was visiting for the academic year thanks to Richard Askey) allowed me to undertake a 3 week European itinerary which included several days in Cambridge. I went there as the guest (and at the suggestion) of Lucy Slater. She had told me that many of Watson's papers (G. N. Watson died in 1965) had been deposited in the Trinity College Library. So I went with minimal expectations to examine Watson' papers. In one box was a manuscript of nearly one hundred pages written in Ramanujan's inimitable hand. Perusing it I saw many of the formulas from Watson's second paper as well as other formulas which Watson had suggested couldn't exist. The manuscript had few words mostly formulas. However when I saw series like

$$1+\frac{q}{1+q}+\frac{q^4}{(1+q)(1+q^2)}+\cdots$$

or

$$q+q^3(1+q)+q^6(1+q)(1+q^2)+\cdots,$$

I recognized immediately my old friends the fifth order mock theta functions.

Of course, the Lost Notebook [51] was a gold mine. I have spent a significant portion of the last two decades studying this marvellous collection of formulas that Ramanujan stated without proof.

The Lost Notebook has many amazing formulae; so I will conclude with two formulas that quite surprised me [22]. Consider Euler's function

$$S(q) = \prod_{n=1}^{\infty}(1+q^n) = \prod_{n=1}^{\infty}\frac{1}{1-q^{2n-1}}$$

and define

$$R(q) = 1 + \sum_{n=1}^{\infty}\frac{q^{n(n+1)/2}}{(1+q)(1+q^2)\cdots(1+q^n)}\ ,$$

and

$$D(q) = -\frac{1}{2} + \sum_{n=1}^{\infty}\frac{q^n}{1-q^n}\ .$$

Then Ramanujan [51; p. 14] asserts:

$$\sum_{n=0}^{\infty}(S(q)-(1+q)(1+q^2)\cdots(1+q^n)) = S(q)D(q) + \frac{1}{2}R(q)$$

and

$$\sum_{n=0}^{\infty}\left(S(q) - \frac{1}{(1-q)(1-q^3)\cdots(1-q^{2n+1})}\right) = S(q)D(q^2) + \frac{1}{2}R(q)\,.$$

This study subsequently led to the discovery by Freeman Dyson, Dean Hickerson and me [33] that most of the power series coefficients of $R(q)$ are zero; however every integer appears infinitely often as a coefficient.

4. The q-Series Path to Bailey Chains.

In this topic, my experiences in graduate school played a very important role as well. Here too both Rademacher and Fine had important things to say.

Rademacher loved Schur's paper [53] which contains Schur's independent discovery of the Rogers-Ramanujan identities, and Rademacher used Schur's unique statement [53; p. 303 translated]:

Theorem. *We define the infinite determinant*

$$D(x_1,x_2,x_3,\dots) = \begin{vmatrix} 1 & x_1 & 0 & 0 & \dots \\ -1 & 1 & x_2 & 0 & \dots \\ 0 & -1 & 1 & x_3 & \dots \\ 0 & 0 & -1 & 1 & \dots \\ \vdots & \vdots & \vdots & \vdots & \end{vmatrix} \tag{4.1}$$

and we let

$$D_\mu(q) = D(q^\mu, q^{\mu+1}, q^{\mu+2}, \dots), \tag{4.2}$$

then for $|q| < 1$

$$D_1(q) = \prod_{n=1}^{\infty} \frac{1}{(1-q^{5n-4})(1-q^{5n-1})}$$

and

$$D_2(q) = \prod_{n=1}^{\infty} \frac{1}{(1-q^{5n-3})(1-q^{5n-2})}. \tag{4.3}$$

Still following Schur, we let

$$D(x_1, x_2, \dots, x_m) = \begin{vmatrix} 1 & x_1 & 0 & \dots\dots\dots & & 0 \\ -1 & 1 & x_2 & \dots\dots\dots & & 0 \\ 0 & -1 & 1 & \dots\dots\dots & & 0 \\ \vdots & \vdots & \vdots & & & \vdots \\ 0 & 0 & 0 & -1 & 1 & x_m \\ 0 & 0 & 0 & 0 & -1 & 1 \end{vmatrix} \tag{4.4}$$

The proof of the standard form of the Rogers-Ramanujan identities then proceeds as follows. If

$$\Delta(z, q) = D(z, zq, zq^2, zq^3, \dots), \tag{4.5}$$

then expansion of $\Delta(z, q)$ along its top row yields

$$\Delta(z, q) = \Delta(zq, q) + z\Delta(zq^2, q), \tag{4.6}$$

and substituting a power series expansion for $\Delta(z, q)$ into (4.6) and comparing coefficients of z^n, we find directly that

$$\Delta(z, q) = 1 + \sum_{n=1}^{\infty} \frac{q^{n^2-n} z^n}{(1-q)(1-q^2)\cdots(1-q^n)}, \tag{4.7}$$

which means

$$D_\mu(q) = \Delta(q^\mu, q) = 1 + \sum_{n=1}^{\infty} \frac{q^{n^2+(\mu-1)n}}{(1-q)(1-q^2)\dots(1-q^n)}. \tag{4.8}$$

Finally it is possible to deduce (4.3) from letting $n \to \infty$ in the following polynomial identities

$$D(q, q^2, \ldots, q^n) = \sum_{\lambda=-\infty}^{\infty} (-1)^\lambda q^{\lambda(5\lambda+1)/2} \begin{bmatrix} n+1 \\ \lfloor \frac{n+1-5\lambda}{2} \rfloor \end{bmatrix} \tag{4.9}$$

and

$$D(q^2, q^3, \ldots, q^n) = \sum_{\lambda=-\infty}^{\infty} (-1)^\lambda q^{\lambda(5\lambda-3)/2} \begin{bmatrix} n+1 \\ \lfloor \frac{n+1-5\lambda}{2} \rfloor + 1 \end{bmatrix} \tag{4.10}$$

A careful study of the several aspects of Schur's work suggests the beginning both of much further development of q-difference equations [7] (of which (4.6) is an example) and of a study of polynomial identities (such as (4.9) and (4.10)) which lead to the theory of Bailey chains [20].

Indeed the observation that $\Delta(z, q)$ yields (4.6) by expansion along the top row immediately suggests that other q-difference equations such as Atle Selberg's [54] generalization of (4.6) can be translated back into infinite determinants. From there the determinants can be truncated (just as (4.4) truncates (4.1)). Then if one is lucky one can expand the truncations along their last column (as Schur did for (4.4)), and, with some luck, it will be possible to read off what partitions are being generated. This is, in fact, the genesis of [7], and the work there led to the sequence of papers [8] [11] [12] [14] culminating in [17]. In recent years, these studies have led to collaboration with J. Olsson and C. Bessenrodt [24], [30], [34].

As noted in [20; p. 279], (4.9) and (4.10) are instances of Bailey pairs. However the distance between Schur's paper and Bailey pairs in general is great. Indeed it was the study of q-series alluded to in the last paragraph that eventually led to my collaboration with Richard Askey [27]. In trying to link that paper with the work of Bailey [38], [39], I was led to [20].

The "aha!" moment for Bailey chains came in the summer of 1982 in Toronto. I had agreed to give a paper in a special session on the work of Gabor Szegö run by Richard Askey. I settled on trying to say something about the Rogers-Szegö polynomials. At the last minute, as I tried to fit these recalcitrant objects into the format of earlier work, I saw the power of Bailey's lemma [20; p. 270] unfolding in front of my eyes. This was quickly written up in [20].

The basic idea is a sequence of pairs of rational functions $(\alpha_n, \beta_n)_{n \geqq 0}$ which is called a Bailey pair provided that for each $n \geqq 0$

$$\beta_n = \sum_{j=0}^{n} \frac{\alpha_j}{\prod_{h=1}^{n-j}(1-q^h) \prod_{k=1}^{n+j}(1-aq^k)} .$$

The Bailey Lemma [20], [39] may succinctly be stated as follows:

Bailey's Lemma. If $(\alpha_n, \beta_n)_{n \geqq 0}$ is a Bailey pair, then so is (α'_n, β'_n) where

$$\alpha'_n = \frac{(\rho_1; q)_n (\rho_2; q)_n \left(\frac{aq}{\rho_1 \rho_2}\right)^n \alpha_n}{\left(\frac{aq}{\rho_1}; q\right)_n \left(\frac{aq}{\rho_2}; q\right)_n},$$

and

$$\beta'_n = \sum_{j=0}^{n} \frac{(\rho_1; q)_j (\rho_2; q)_j \left(\frac{aq}{\rho_1 \rho_2}; q\right)_{n-j} \left(\frac{aq}{\rho_1 \rho_2}\right)^j \beta_j}{(q; q)_{n-j} \left(\frac{aq}{\rho_1}; q\right)_n \left(\frac{aq}{\rho_2}; q\right)_n}.$$

Bailey never wrote this result down in this form, and consequently he missed the power of this result to produce infinite chains (Bailey Chains) of Bailey pairs. The applications of [20] continue to this day. One of the subsequent highlights was the use of Bailey chains by F. Dyson, D. Hickerson and me [33] to prove that

$$1 + \sum_{n=1}^{\infty} \frac{q^{n(n+1)/2}}{(1+q)(1+q^2)\cdots(1+q^n)}$$
$$= \sum_{\substack{n \geqq 0 \\ |j| \leqq n}} (-1)^{n+j} q^{n(3n+1)/2 - j^2} (1 - q^{2n+1}).$$

Suppose $\Delta_e(n)$ (resp. $\Delta_0(n)$) is the number of partitions of n into distinct parts with even (resp. odd) rank. The rank is the largest part minus the number of parts.

Identity (4.1) implies that

$$\Delta_e(n) - \Delta_0(n)$$

is almost always 0 but that it also takes any integral value infinitely many times.

This is the result alluded to at the end of Section 3.

5. Determinant Evaluations.

The problems that led to my study of determinant evaluation have already appeared in the previous section. Namely, when learning about Schur's work from Rademacher, I noticed that setting $q = 1$ in (4.9) yielded a new representation of the Fibonacci numbers, namely

$$F_{n+1} = \sum_{\lambda=-\infty}^{\infty} (-1)^{\lambda} \binom{n}{\left\lfloor \frac{n-5\lambda}{2} \right\rfloor}. \tag{5.1}$$

Furthermore this formula implies immediately that if p is a prime congruent to 1 modulo 5 (so necessarily $p = 10j + 1$), then

$$F_{p+1} \equiv (-1)^{2j} \equiv 1 \pmod{p}. \tag{5.2}$$

This is the only proof of (5.2) I know which does not rely on the Binet formula [55; p. 15] for F_n.

I did consider very early [13; Received by the editors, March 16, 1966] a full generalization of (4.9) [13; p. 302]

$$\Delta_{k,n} = \sum_{\lambda=-\infty}^{\infty} (-1)^{\lambda} q^{\lambda((2k+1)\lambda+1)/2} \begin{bmatrix} n \\ \left\lfloor \frac{n-(2k+1)\lambda}{2} \right\rfloor \end{bmatrix}. \tag{5.3}$$

However, it took a number of years before I stumbled on the successive ranks theorem [15], [16], and a number of years after that before these polynomials became important in statistical mechanics [21; Ch. 8], [28].

However, I did discover a number of interesting properties of

$$F_{k,n} = \sum_{\lambda=-\infty}^{\infty} (-1)^{\lambda} \binom{n}{\left\lfloor \frac{n-(2k+1)\lambda}{2} \right\rfloor}. \tag{5.4}$$

These were recorded in a paper [10] whose results are somewhat overshadowed by the fact that the name "Einstein" replaced "Eisenstein" throughout, and my middle initial "E" was replaced by "H." I can only say in my defense that these mistakes were not in my original manuscript, and I was not sent any sort of page or galley proofs to correct.

In any event, I showed in [10] that the roots of the auxiliary polynomial for the minimal recurrence for $F_{k,n}$ do, in fact, define the maximal real subfield of $Q(e^{2\pi i/(2k+1)})$. Among my unpublished discoveries was the fact that

$$\delta_k = \det(F_{k,i+j-1})_{1 \leqq i,j \leqq k} = \det\left(\binom{i+j-1}{\left\lfloor \frac{i+j-1}{2} \right\rfloor}\right)_{1 \leqq i,j \leqq k} \tag{5.5}$$

satisfied

$$\delta_k = (-1)^{\lfloor k/2 \rfloor} \tag{5.6}$$

I found this assertion nearly impossible until I happened upon the following two evaluations in Muir's famous book [49; pp. 435–436]

$$\det\left(\binom{2i+2j-1}{i+j-2}\right)_{1 \leqq i,j \leqq n} = 1, \tag{5.7}$$

and

$$\det\left(\frac{1}{2j+2i-1}\binom{2i+2j-1}{i+j-2}\right)_{1\leqq i,j\leqq n} = 1\,. \tag{5.8}$$

While (5.7) and (5.8) only involve the odd central binomial coefficients whereas (5.5) concerns all central binomial coefficients, it was clear that Muir's method was precisely what the doctor ordered to establish (5.6).

Namely, Muir multiplies the determinant in (5.7) on the left by

$$\det\left(\frac{(-1)^{i+j-1}(2i-1)}{(2j-1)}\binom{i+j-1}{2j-2}\right)_{1\leqq i,j\leqq n} \tag{5.9}$$

to produce a lower triangular determinant with 1's on the main diagonal. To treat (5.8) he multiplies that determinant on the left by

$$\det\left((-1)^{i+j}\binom{i+j-2}{2j-2}\right)_{1\leqq i,j\leqq n}\,. \tag{5.10}$$

In each instance, Muir states the underlying binomial coefficient identity necessary to prove that the result of multiplication is an upper triangular determinant with ones on the main diagonal. In fact, each identity is an instance of the Pfaff-Saalschütz ${}_3F_2$ summation [35; p. 9], a fact not realized by Muir nor by me at the time.

Following up on this idea was quite easy. One defines a lower triangular determinant $\det(c_{ij})_{1\leqq i,j\leqq k}$ with 1's on the main diagonal. Then the undetermined c_{ij} can easily be found to force

$$\det(c_{ij})_{1\leqq i,j\leq k}\cdot\delta_k = \epsilon_k \tag{5.11}$$

where ϵ_k is upper triangular. The $i-1$ entries $c_{i,j}$, $1 \leqq j \leqq i-1$, must fulfill the $i-1$ linear equation

$$\sum_{k=1}^{i-1} c_{i,k}\binom{k+j-1}{\left\lfloor\frac{k+j-1}{2}\right\rfloor} + \binom{i+j-1}{\left\lfloor\frac{i+j-1}{2}\right\rfloor} = 0\,. \tag{5.12}$$

From (5.12) we can empirically produce as many of the c_{ij} as we want. It is then an easy matter to guess that if

$$a_{ij} = \frac{(-1)^{i+j-1}(2j-1)}{(2i-1)}\binom{i+j-1}{2i-2} \tag{5.13}$$

and

$$b_{ij} = (-1)^{i+j} \binom{i+j-1}{2i-1}, \tag{5.14}$$

then

$$c_{i,j} = \begin{cases} a_{h,m} & \text{if } i = 2h-1,\ j = 2m-1 \\ 0 & \text{if } i = 2h-1,\ j = 2m \\ b_{h,m} & \text{if } i = 2h,\ j = 2m \\ -2b_{h,m} & \text{if } i = 2h,\ j = 2m-1 \end{cases} \tag{5.15}$$

Two further binomial coefficient identities are required and again each is an instance of the Pfaff-Saalschütz summation.

There were several ideas I took away from this. First, something like this method ought to work on any determinant of binomial coefficients. This faith led to all of my work on plane partitions [19], [23], [24], [31]. Second the complexity of the rules defining c_{ij} in (5.15) suggests that there will be a mixture of summation theorems required to finish off a given result.

In recent years, C. Krattenthaler has built a number of powerful methods for determinant evaluation (see for example [43]). These methods go well beyond the technique described here.

6. Partition Analysis and P. A. MacMahon.

Elsewhere in this volume, Peter Paule, Axel Riese, and I present an account of one aspect of our work to implement the method of partition analysis in Mathematica.

So I shall abbreviate this section down to a short acknowledgement of P. A. MacMahon's influence on much of my work. Thanks to a 1971 invitation by Gian-Carlo Rota to edit MacMahon's Collected Papers [47], [48]; I became aware of many rich and little explored areas of combinatorics and partitions.

MacMahon's account of partition analysis makes clear that it is a powerful method [46; Section VIII]. However, his failure to refine it adequately to evaluate the generating functions for plane partitions caused the method to fall into disuse. In the following 75 years only Richard Stanley [54] made significant use of it (in his proof of the Anand-Dumir-Gupta conjecture [1]).

Once it was realized that the method is in fact algorithmic, the task of implementing it in computer algebra became an important project.

7. The Liouville Mystery.

The last topic in this collection of vignettes is a subject that has been on my mind for the last four years. In the summer of 1994, David Crippa, Klaus Simon and I collaborated on the application of q-series to certain problems in random graphs [32]. Among the results we required was the following, old, often rediscovered chestnut

$$\sum_{n=1}^{\infty} \frac{(-1)^n q^{n(n+1)/2}}{(1-q)(1-q^2)\cdots(1-q^{n-1})(1-q^n)^2} = \sum_{m,n\geqq 1} q^{mn}\,. \tag{7.1}$$

Somewhat related were some papers by W. N. Bailey [36], [37] which had been inspired by Bell's proof [40] of "Liouville's Last Theorem." Indeed, Bell's paper begins:

"In the usual notation,

$$N = N[n = wx + xy + yz + zu, ; w, x, z, u > 0,\ y \geqq 0]$$

denotes the number of sets (w, x, y, z, u) of integers, subject to the conditions indicated, satisfying the stated equations in which n is a arbitrary constant integer > 0. Then

$$N = D_2(n) - n\, D_0(n)\,. \tag{7.2}$$

This curious result is the only one of the numerous theorems on quadratic forms stated by Liouville for which (apparently) no proof has been published."

Bell devoted a significant portion of his career (cf. [51]) to an explication of Liouville's work in number theory. To gain some idea of the mystery and controversy surrounding Liouville's original work on this topic, let us refer to Lützen's biography [45; pp. 228 and 229]. "Liouville had begun publishing on quadratic forms in 1856. In 1860, he inserted more than a dozen notes on this question in his *Journal*, and in 1861, he ran amuck, publishing more than 30 notes of one or two pages each, all with the same structure: a theorem stating that numbers of a particular form, $a + b\mu$ (a, b are specified numbers, μ a variable), can be written in a given number of ways, by way of a particular quadratic form, for example, $Ax^2 + By^2 + Cz^2 + Dt^2$ ($A,\ B,\ C,\ D$ are specified numbers) The theorems were not proved, but merely illustrated with a particular value of μ. Thus, not only did Liouville keep the proofs of his theorems about number-theoretical functions to himself, he also hid how they could be applied to quadratic forms. . . .

"If Liouville hid his methods, like renaissance mathematicians, in order to impress his colleagues with his results, he did not entirely succeed. This can be seen in a letter from Hermite to Catalan, probably written shortly after 1865, when Catalan had moved to Liège:

> For a long time, I have shared your sentiments of regret concerning Liouville's last arithmetical publications. The secret behind his numerous theorems has not been long in becoming known (P. Pépin has proved them). He would have gained much by showing his principles and his methods at once instead of keeping them to himself; his meager and monotonous verifications make one smile a little."

While Pepin, Humbert and other solved some of Liouville's riddles, Bell became famous as their master. For this he received the Bôcher Prize in 1924 [52; p. 201].

To my great surprise, Liouville's Last Theorem and related results involving rather messy convolutions of sums of powers of divisors, are in fact closely related to (7.1).

To understand the relationship, we define

$$\mathcal{L}_k(q) = \sum_{n_1, n_2, \ldots, n_{k+1} \geqq 1} q^{n_1 n_2 + n_2 n_3 + n_3 n_4 + \cdots + n_k n_{k+1}}, \tag{7.3}$$

and

$$M_k(q) = \sum_{n=1}^{\infty} \frac{(-1)^n q^{n(n+1)/2}}{(1-q)(1-q^2)\cdots(1-q^{n-1})(1-q^n)^{k+1}}. \tag{7.4}$$

Then (7.1) asserts

$$M_1(q) = \mathcal{L}_1(q). \tag{7.5}$$

Using the results of [32], it is easy to prove

$$M_2(q) = \mathcal{L}_1(q) + \mathcal{L}_2(q) + \mathcal{L}_3(q), \tag{7.6}$$

and with a lot of effort and several results like Liouville's Last Theorem one can prove [26]

$$M_3(q) = \mathcal{L}_1(q) + 2\mathcal{L}_2(q) + 3\mathcal{L}_3(q) + 2\mathcal{L}_4(q) + \mathcal{L}_5(q). \tag{7.7}$$

Given that the coefficients of the $\mathcal{L}_i(q)$ in (7.5)–(7.7) form the table

$$\begin{matrix} 1 \\ 1\ 1\ 1 \\ 1\ 2\ 3\ 2\ 1, \end{matrix}$$

one expects that we are considering the famous table of trinomial coefficients. If this is true, the next line should be

$$1\ 3\ 6\ 7\ 6\ 3\ 1$$

But

$$\begin{aligned} M_4(q) = {} & \mathcal{L}_1(q) + 3\mathcal{L}_2(q) + 6\mathcal{L}_3(q) + 7\mathcal{L}_4(q) \\ & + 6\mathcal{L}_5(q) + 3\mathcal{L}_6(q) + \mathcal{L}_7(q) \\ & + (q^7 + 2q^8 + 6q^9 + 11q^{10} + 22q^{11} + 33q^{12} \\ & + 57q^{13} + 83q^{14} + \cdots) \end{aligned} \tag{7.8}$$

Thus the beautiful pattern of (7.5)–(7.7) almost holds up, but not quite. And so, Liouville's Last Theorem becomes linked with a new mystery: What is going on in (7.8), and what is the correct continuation of the pattern beginning with (7.5)–(7.7)?

8. Conclusion.

It is impossible within the confines of a survey paper to provide anything like an accounting of my many debts. To all who helped organize the Maratea conference and to all who participated you have my deep thanks.

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