The meaning of "understanding"

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The most effective way to begin a seminar is to use the "cheap trick" known to all senior faculty members. Namely you first go over the names of everyone in the audience and find a way to refer in a favorable manner to the papers of as many people as possible who are in attendance. After that the audience is so pleased with themselves that the speaker can get away with saying almost anything he/she desires without fear of a negative audience reaction.

Not wanting to overlook this excellent piece of advice I have studied the list of people invited to speak at this workshop and I have discovered that almost without exception every one who has been invited shares the distinction that their work has not be properly appreciated. I was struck by the fact that Rodney Baxter deserves the Prime Ministers Award and the Wolf Prize for his many profound discoveries in statistical mechanics; Michio Jimbo deserves the Fields medal for his invention of Quantum Groups; there are many professors in this audience who deserve to be members of the Australian or the US National Academy of Science; there are Readers who should be Professors; there are post doctoral fellows who should have faculty positions, there are graduate students who should be postdoctoral fellows and there are many who deserve grants from the ARC, NSF or CNRS which have not been awarded. I have written many letters and nominations over the years in an attempt to rectify these many injustices but my advice has almost never been followed.

After praising the papers of as many audience members as possible the speaker then usually sets out to explain his most recent work, which usually has little or nothing to do with the work of the people he has just finished praising, But many years ago in the days when Joel Lebowitz still ran conferences in the second floor of an abandoned movie theater I heard a talk given by Freemen Dyson which followed a much different strategy. Dyson began by saying that if you wanted to learn the things which he understood then he advised you to read his papers and that instead of repeating what was in his recent papers he was in fact going to talk about things which he did not understand. I have pondered this for many years and finally think that I know what Dyson was driving at and so today I am going to follow his advise and talk about a few of the very many things which I do not understand.

1 What is "Understanding"?

The word "understanding" is used in many different ways and means many different things. For example some people will tell you that they do not "understand" some equation in your paper when what they really mean is that they believe that the equation is wrong. If you want to make sure that some one does not get hired all you have to do is to assert that the candidate does not “understand” something which the other members of the hiring committee think is “obvious” and the candidate is doomed.

But in this talk I will not use the word “understand” to mean that a mistake has been made or that a candidate is not worthy of consideration. Instead I will adopt the following definition:

No one can be said to understand a paper unless he is able to generalize the paper.

From this definition the following corollary follows:

No author can be said to understand his most recent paper

This use of the word “understanding” is extremely useful in advancing knowledge. It is closely related to a piece of advice I give to graduate students”

“Given the choice between attending a lecture in which you understand every word and a seminar in which you understand nothing always attend the seminar in which you do not understand anything because there is a chance you can learn something from it. Whereas if you already understand everything in a seminar it is obvious that you can learn nothing new.”

There is, of course a terrible public relations disadvantage in ever saying that you do not understand something. No one would dare put such a statement into a grant application for fear of an almost instant rejection. This is in spite of the fact that it is pointless to propose research on something you already do understand. I would never write in a letter of recommendation for position ranging from a post doc to a position in the Academy that a person does not understand something. But since I am now sufficiently senior that nothing can be done to me which I cannot retaliate against by retiring I will run the risk of appearing ignorant and while mindful of the fact that many of you know Freeman Dyson and are certain that I do not have his stature I will follow his advice and talk about things which I do not understand.

There are at least two reasons why both the author and the reader will not understand a paper.

The first reason is the the author may firmly believe that an equation, procedure, theory, or fundamental law of nature is correct when in fact it is not. If an incorrect equation is found quickly the lack of understanding is usually called a mistake. It is commonly thought by the general public that this type of misunderstanding can never happen in a scientific publication because scientific papers are all “refereed” and the referee and journal will not accept a paper unless it is “error free”. This belief is of course nonsense. The more inventive and profound a paper is the less anyone is able to referee the paper let alone understand it. Certainly in 1944 there was no one on the planet capable of understanding Onsager’s paper on the Ising model and I venture to say that there are not more than 10 people living or dead who can be said to have understood that paper.

If the problem involves an assumption which is later shown to be false or not applicable or if the error is subtle or even if it is the result of overlooking something in a long and inventive computation the use of the word "error" is totally unjustified.

If the underlying theory or law of nature turns out to be wrong the "correction" of the "error" is praised as a major scientific advance. Thus even though Einsteins theory of gravitation replaces Newton's theory we do not condemn Newton for "'making a mistake".

The second reason that the author or a reader will not understand a paper is that the author will quite naturally and correctly only write about what works and what he knows about. Therefore papers almost by definition present methods which are sufficient to solve a problem. What is almost, if not always, impossible to do is to prove that the method is necessary.

In this talk I will give examples of both types of misunderstanding with the emphasis on my lack of understanding of my own work

2 Virial coefficients

In the past 5 years my collaborators Nathan Clisby, now at Melbourne, Ivar Lyberg and I have investigated the computation of the coefficients B_k in the virial expansion

$$\frac{P}{kT} = \sum_{k=1}^{\infty} B_k \rho^k \tag{1}$$

for the hard "sphere" potential in D dimensions

$$U(\mathbf{r}) = \begin{array}{ll} \infty & \text{for } \ 0 \leq |\mathbf{r}| \leq \sigma \\ 0 & \text{for } \ \sigma < |\mathbf{r}| \end{array} \tag{2}$$

The integrals of B_k for $k = 1, 2, 3$ are all elementary and have been computed long ago in all dimensions D. The coefficient B_4 is far more difficult to evaluate. In $D = 3$ it was first computed by van Laar and by Boltzmann in 1899 and the case $D = 2$ was only computed by Rowlinson and by Hemmer in 1964. Clisby and myself [1] computed B_4 for $D = 4, 6, 8, 10, 12$ in 2004 and Lyberg [2] did the computation for $D = 5, 7, 9, 11$ in 2005. The results for B_3/B_2^2 and B_4/B_2^3 are given in table 1.

There are several things I do not understand about these these two papers. First of all the coefficients B_k can, in any dimension D, be expressed as $k-1$ fold integrals but what is completely unclear is why for $k = 3, 4$ these integrals can be evaluated in terms of the very particular transcendental numbers of table I. In particular it is extremely natural to conjecture as Lyberg did in [2] that for even dimensions

$$B_k/B_2^{k-1} = \sum_{n=0}^{k-1} a_n (\frac{\sqrt{3}}{\pi})^n \tag{3}$$

Table 1: Exact and numerical values of B_3/B_2^2 and B_4/B_4^3 in dimensions $2 \leq D \leq 12$

D	B_3/B_2^2	B_4/B_2^3	decimal expansion of B_4/B_2^3
2	$\frac{4}{3} - \frac{\sqrt{3}}{\pi}$	$2 - \frac{9}{2}\frac{\sqrt{3}}{\pi} + 10\frac{1}{\pi^2}$	$0.53223180\ldots$
3	$5/8$	$\frac{2707}{4480} + \frac{219}{2240}\frac{\sqrt{2}}{\pi} - \frac{4131}{4480}\frac{\arccos 1/3}{\pi}$	$0.28692950\cdots$
4	$\frac{4}{3} - \frac{3}{2}\frac{\sqrt{3}}{\pi}$	$2 - \frac{27}{4}\frac{\sqrt{3}}{\pi} + \frac{832}{45}\frac{1}{\pi^2}$	$0.15184696\cdots$
5	$53/2^7$	$\frac{2515393}{32800768} + \frac{3888425}{16400384}\frac{\sqrt{2}}{\pi} - \frac{67183425}{32800768}\frac{\arccos 1/3}{\pi}$	$0.07597248\cdots$
6	$\frac{4}{3} - \frac{9}{5}\frac{\sqrt{3}}{\pi}$	$2 - \frac{81}{10}\frac{\sqrt{3}}{\pi} + \frac{38848}{1575}\frac{1}{\pi^2}$	$0.03336314\cdots$
7	$289/2^{10}$	$\frac{299189248759}{29059601184} + \frac{159966456685}{435894091776}\frac{\sqrt{2}}{\pi}$ $- \frac{292926667005}{96865353728}\frac{\arccos 1/3}{\pi}$	$0.00986494\cdots$
8	$\frac{4}{3} - \frac{279}{140}\frac{\sqrt{3}}{\pi}$	$2 - \frac{2511}{280}\frac{\sqrt{3}}{\pi} + \frac{17605024}{606375}\frac{1}{\pi^2}$	$-0.00255768\cdots$
9	$6343/2^{15}$	$\frac{2886207717678787}{2281372811001856} + \frac{2698457589952103}{5703432027504640}\frac{\sqrt{2}}{\pi}$ $- \frac{8656066770083523}{2881372811001856}\frac{\arccos 1/3}{\pi}$	$-0.00858079\cdots$
10	$\frac{4}{3} - \frac{297}{140}\frac{\sqrt{3}}{\pi}$	$2 - \frac{2673}{280}\frac{\sqrt{3}}{\pi} + \frac{49048616}{1528065}\frac{1}{\pi^2}$	$-0.01096248\cdots$
11	$35995/2^{15}$	$\frac{17357449486516274011}{11932824186709344256} + \frac{6554115383300832799}{29832060466773360640}\frac{\sqrt{2}}{\pi}$ $- \frac{52251492946866520923}{11932824186709344256}\frac{\arccos 1/3}{\pi}$	$-0.01133719\cdots$
12	$\frac{4}{3} - \frac{243}{110}\frac{\sqrt{3}}{\pi}$	$2 - \frac{2187}{220}\frac{\sqrt{3}}{\pi} + \frac{11565604768}{337702365}\frac{1}{\pi^2}$	$-0.01067028\cdots$

More generally it can be asked if it is true that for all k the $k-1$ fold integrals for B_k reduce to the sum of products of one dimensional integrals. The explicit computations of [1] and [2] give no insight into this question.

For odd dimension there is no obvious way to explain the occurrence of the two transcendental numbers in table 1.

Indeed at a more elementary level it should be pointed out that in both [1] and [2] that at some point the algebra became so tedious that the authors resorted to the uss of Maple and Mathamatica which is the reason that there is no general formula known for B_4/B_2^3 for arbitrary integer D

However, the integrals which go into the evaluation of B_k have, for all k a geometric interpretation in terms of volumes of overlapping spheres and as such the evaluation of B_k can be considered a problem in classical geometry which would be understood by Euclid. It is surprising to me that such a simply stated problem in classical physics which has been in the literature for over 100 years should still not be understood

3 The Ising model

The Ising model is probably the most famous "solved" model in statistical mechanics. It certainly is the model of which we know the most. I have published papers on the Ising model for 40 years. Nevertheless the number of things I do not understand about the Ising model seems to increase every year.

The first thing I do not understand about the Ising model is Onsager's method of solution. Onsager makes crucial use in his 1944 paper [3] of the following infinite dimensional algebras

$$[A_m, A_n] = 4G_{m-n} \tag{4}$$

$$[G_m, A_n] = 2A_{n+m} - 2A_{n-m} \tag{5}$$

$$[G_m, G_n] = 0 \tag{6}$$

and it can be shown that this algebra follows from the two special cases

$$[A_0, [A_0, [A_0, A_1]]] = 16[A_0, A_1] \tag{7}$$

$$[A_1, [A_1, [A_1, A_0]]] = 16[A_1, A_0] \tag{8}$$

This algebra is called "Onsager's algebra" and is isomorphic to the sl_2 loop algebra modded out by a Z_2 automorphism.

I would like to understand is what is the representation theory of this algebra and the first study of this was made by von Gehlen and Rittenberg[4] 40 years after Onsager first introduced it. This beautiful piece of understanding yields the superintegrable chiral Potts model. What is not understood is if there are still further representations. Moreover the role of Onsager's algebra in the solution of the Ising model or the chiral Potts model is not understood. The fact that both the Ising model and the chiral Potts model are representations of the same algebra must mean that a great deal of the structure of the Ising model correlation functions must carry over to the correlation functions of the superintegrable chiral Potts mode but no one understands how to do it.

Perhaps the most vivid personal example I can give of an author's not understanding his own paper is my experience with my 1976 paper with Wu, Tracy, and Barouch [5] about the correlation functions of the Ising model. In that paper we started from a Fredholm determinant expression for the correlation function $\langle \sigma_{0,0}\sigma_{M,N}\rangle$ and derived the"exponential" expressions for $T < T_c$

$$\langle \sigma_{0,0}\sigma_{M,N}\rangle = (1-t_-)^{1/4}\mathrm{exp}\sum_{n=1}^{\infty} F_-^{(2n)}(M,N) \tag{9}$$

with $t_- = (\sinh 2E_v/kT \sinh 2E_h/kT)^{-2}$ and for $T > T_c$

$$\langle \sigma_{0,0}\sigma_{M,N}\rangle = (1-t_+)^{1/4}\sum_{n=0}^{\infty} G^{(2n+1)}(M,N)\mathrm{exp}\sum_{n=1}^{\infty} F_+^{(2n)}(M,N) \tag{10}$$

with $t_+ = (\sinh 2E_v/kT \sinh 2E_h/kT)^{2}$. In these expansions $F_-^{(k)}(M,N)$, $F_+^{(k)}(M,N)$ and $G^{(k)}(M,N)$ are explicitly derived $2k$ fold integrals. We also gave the first few terms in the form factor expansion for $T < T_c$

$$\langle \sigma_{0,0}\sigma_{M,N}\rangle = (1-t_-)^{1/4}\{1+\sum_{n=1}^{\infty} f^{(2n)}(M,N)\} \tag{11}$$

and for $T > T_c$

$$\langle\sigma_{0,0}\sigma_{M,N}\rangle = (1-t_+)^{1/4}\sum_{n=0}^{\infty} f^{(2n+1)}(M,N) \tag{12}$$

where $f^{(k)}$ is a $2k$ dimensional integral. It was obvious at the time that we did not understand the form factor expansions because we did not give the general term of (11) and (12). The computation of the full expansion of (11) and (12) was subsequently carried out in a sequence of papers [6]-[10] by several authors over the next 25 years.

But there are many other things which I have not understood about my 1976 paper. The Fredholm determinant representation of the Ising correlations which formed the starting point of the 1976 paper is only one of many possible starting points because besides the Fredholm determinant expansion there are an infinite number of ways to write the Ising correlations as determinants of finite size. Indeed the first work on large separation expansions of the row correlation function $\langle\sigma_{0,0}\sigma_{0,N}\rangle$ was done by Wu in 1966 [11] starting from an $N\times N$ Toeplitz determinant and the similar expansion for the diagonal correlation $\langle\sigma_{0,0}\sigma_{N,N}\rangle$ was written out in 1973 in [12]. The determinental representation used in these papers is

$$D_N = \det \mathbf{A}_N \tag{13}$$

where

$$\mathbf{A}_N = \begin{pmatrix} a_0 & a_{-1} & \dots & a_{1-N} \\ a_1 & a_0 & \dots & a_{2-N} \\ \vdots & \vdots & \ddots & \vdots \\ a_{N-1} & a_{N-2} & \dots & a_0 \end{pmatrix} \tag{14}$$

and

$$a_n = \frac{1}{2\pi i}\oint_{|z|=1} \varphi(z) z^{-n-1}\, dz, \tag{15}$$

where the path of integration is counterclockwise. and the function $\varphi(z)$ is

$$\varphi(z) = \left(\frac{(1-\alpha_1 z)(1-\alpha_2 z^{-1})}{(1-\alpha_1 z^{-1})(1-\alpha_2 z)}\right)^{1/2}. \tag{16}$$

For the diagonal correlation function $\langle\sigma_{0,0}\sigma_{N,N}\rangle$

$$\alpha_1 = 0 \quad \text{and} \quad \alpha_2 = (\sinh 2K_1 \sinh 2K_2)^{-1} \tag{17}$$

where $K_j = E_j/kT$. For the row correlation function $\langle\sigma_{0,0}\sigma_{0,N}\rangle$

$$\alpha_1 = e^{-2K_2}\tanh K_1 \quad \text{and} \quad \alpha_2 = e^{-2K_2}\coth K_1. \tag{18}$$

What is very striking is that when you compare the form factor integrals $f^{(2)}(0,N)$ and $f^{(2)}(N,N)$ as obtained from the 1976 paper with the corresponding integrals for $f^{(2)}(0,N)$ and $f^{(2)}(N,N)$ as obtained from Wu's 1966 paper [11] you will discover that they "do not look the same" even though when expansed in a power series in t_- they are indeed equal as they must be if the formalism is to be consistent. A direct demonstration of this necessary equality is not given in the 1976 paper and in fact such a demonstration does not exist to this day.

Because the lowest order result of Wu [11] does not agree in form with the leading term in the form factor expansion of the 1976 paper there must be a representation of form factor expansions of the row and the diagonal correlations which "looks" much different from the expansions derived from the 1976 paper. This "new" expansion was recently presented in [13] and a proof was given in [14]. The results of those computations are as follows: For $T < T_c$

$$D_N = S_\infty \{1 + \sum_{n=1}^{\infty} f^{(2n)}(N)\} \tag{19}$$

with

$$S_\infty = \left[\frac{(1-\alpha_1^2)(1-\alpha_2^2)}{(1-\alpha_1\alpha_2)^2} \right]^{1/4} \tag{20}$$

$$f^{(2n)}(N) = \frac{1}{(n!)^2 (2\pi)^{2n}} \lim_{\epsilon \to 0} \prod_{i=1}^{2n} \oint_{|z_i|=1-\epsilon} dz_i \; z_i^N \prod_{k=1}^{n} P(z_{2k}) P(z_{2k}^{-1}) Q(z_{2k-1}) Q(z_{2k-1}^{-1})$$
$$\prod_{l=1}^{n} \prod_{m=1}^{n} (1 - z_{2l-1} z_{2m})^{-2} \prod_{1 \le p < q \le n} (z_{2p-1} - z_{2q-1})^2 (z_{2p} - z_{2q})^2. \tag{21}$$

and

$$Q(z) = ((1-\alpha_1 z)/(1-\alpha_2 z))^{1/2} = 1/P(z) \tag{22}$$

and for $T > T_c$

$$D_N = \widehat{S}_\infty \sum_{n=0}^{\infty} f^{(2n+1)}(N) \tag{23}$$

with

$$\widehat{S}_\infty = \left[(1-\alpha_1^2)(1-\alpha_2^{-2})(1-\alpha_1\alpha_2^{-1})^2 \right]^{1/4} \tag{24}$$

$$f^{(2n+1)}(N) = \frac{i}{n!(n+1)!(2\pi)^{2n+1}} \lim_{\epsilon \to 0} \prod_{i=1}^{2n+1} \oint_{|z_i|=1-\epsilon} dz_i \; z_i^N \prod_{l=1}^{n+1} \widehat{P}(z_{2l-1}) \widehat{P}(z_{2l-1}^{-1}) z_{2l-1}^{-1}$$
$$\prod_{m=1}^{n} \widehat{Q}(z_{2m}) \widehat{Q}(z_{2m}^{-1}) z_{2m} \prod_{p=1}^{n+1} \prod_{q=1}^{n} \frac{1}{(1 - z_{2p-1} z_{2q})^2}$$
$$\prod_{1 \le j < k \le n+1} (z_{2j-1} - z_{2k-1})^2 \prod_{1 \le r < s \le n} (z_{2r} - z_{2s})^2. \tag{25}$$

and

$$\widehat{Q}(z) = ((1-\alpha_1 z)(1-\alpha_2^{-1} z))^{1/2} = 1/\widehat{P}(z). \tag{26}$$

For the diagonal correlations of the Ising model the form factors (21) and (25) specialize to the results first given in [13] for $T < T_c$

$$f_d^{(2n)}(t;N) = \frac{t^{n(N+n)}}{(n!)^2}\frac{1}{\pi^{2n}}\int_0^1 \prod_{k=1}^{2n} x_k^N\, dx_k \prod_{j=1}^{n}\left(\frac{x_{2j-1}(1-x_{2j})(1-tx_{2j})}{x_{2j}(1-x_{2j-1})(1-tx_{2j-1})}\right)^{1/2}$$
$$\prod_{1\le j\le n}\prod_{1\le k\le n}(1-tx_{2j-1}x_{2k})^{-2}\prod_{1\le j<k\le n}(x_{2j-1}-x_{2k-1})^2(x_{2j}-x_{2k})^2 \tag{27}$$

where t is t_- and for $T > T_c$

$$f_d^{(2n+1)}(t;N) = t^{((2n+1)N/2+n(n+1))}\cdot\frac{1}{\pi^{2n+1}}\cdot\frac{1}{n!\,(n+1)!}\cdot\int_0^1\prod_{k=1}^{2n+1} x_k^N\, dx_k$$
$$\prod_{j=1}^{n+1}((1-x_{2j})(1-t\,x_{2j})\,x_{2j})^{1/2}\prod_{j=1}^{n+1}((1-x_{2j-1})(1-t\,x_{2j-1})\,x_{2j-1})^{-1/2}$$
$$\prod_{1\le j\le n+1}\prod_{1\le k\le n}(1-tx_{2j-1}x_{2k})^{-2}\times$$
$$\prod_{1\le j<k\le n+1}(x_{2j-1}-x_{2k-1})^2\prod_{1\le j<k\le n}(x_{2j}-x_{2k})^2 \tag{28}$$

where $t = t_+$ and the last product in (28) is unity for $n = 0, .1$

The functions $f_d^{(k)}(t;N)$ are integrals of algebraic functions and therefore they must satisfy for each fixed N a linear ordinary differential equation in the variable t of the form

$$O_k(N) f_d^{(k)}(t;N) = 0 \tag{29}$$

Recently these annihilating linear differential operators $O_k(N)$ have been obtained [13] by use of formal computer algebra and found to have two very remarkable properties

1) Russian doll decomposition

$$O_{2n}(N) = L_{2n+1}(N)\cdot L_{2n-1}(N)\cdots L_3(N)\cdot L_1$$
$$O_{2n+1}(N) = L_{2n+2}(N)\cdot L_{2n}(N)\cdots L_4(N)\cdot L_2 \tag{30}$$

where $L_j(N)$ is a linear differential operator of order j

2) Direct product decomposition

$$O_{2n} = M_{2n+1}(N)\oplus M_{2n-1}(N)\oplus\cdots\oplus M_1(N)$$
$$O_{2n+1} = M_{2n+2}(N)\oplus M_{2n}(N)\oplus\cdots\oplus M_2(N) \tag{31}$$

where $M_j(N)$ is a linear operator of order j.

In (30) and (31)

$$L_1 = M_1 = \frac{d}{dt} \tag{32}$$

and

$$L_2 = M_2 = L_E = t(1-t)\frac{d^2}{dt^2} + (1-t)\frac{d}{dt} + 1 \tag{33}$$

which is the operator which annihilate the hypergeometric function

$$F(\frac{1}{2}, -\frac{1}{2}; 1; t) = \frac{2}{\pi}E(t^{1/2}) \tag{34}$$

where $E(t^{1/2})$ is the complete elliptic integral of the second kind. Furthermore the operators L_j and M_j for $j \geq 3$ are equivalent (in the sense of operator equivalence) the $j-1$ symmetric product of L_E

From these results we find that all $f^{(n)}(t, N)$ have the following form

$$f_d^{(2n)}(t; N) = t^{N-1} \sum_{l=0}^{n} \sum_{m=0}^{2l} a_{l,m}^{e}(t, N) K^m E^{2l-m}$$
$$f_d^{(2n+1)}(t; N) = t^{N/2} \sum_{l=0}^{n} \sum_{m=0}^{2l+1} a_{l,m}^{o}(t, N) K^m E^{2l+1-m} \tag{35}$$

where $a_{l,m}^{e}(t, N)$ for $N \geq 1$ and $a_{l,m}^{o}(t, N)$ for all N are rational functions of t and

$$E = \frac{2}{\pi}E(t^{1/2}) = F(\frac{1}{2}, -\frac{1}{2}; 1, t) \tag{36}$$
$$K = \frac{2}{\pi}K(t^{1/2}) = F(\frac{1}{2}, \frac{1}{2}; 1, t) \tag{37}$$

As a few examples we note that

$$f_d^{2}(t, 0) = \frac{1}{2}(K - E)K$$
$$f_d^{(2)}(t, 1) = \frac{1}{2}\{1 - 3KE - (t-2)K^2\}$$
$$f_d^{(2)}(t, 2) = \frac{1}{6t}\{6t - (6t^2 - 11t + 2)K^2 - (15t - 4)KE - 2(t+1)E^2 \tag{38}$$

$$f_d^{(3)}(t, 0) = \frac{1}{6}\{K - (t-2)K^3 - 3K^2E\}$$
$$f_d^{(3)}(t, 1) = \frac{t^{-1/2}}{6}\{4(K - E) - 6K^2E - (2t-3)K^3 + 3KE^2\} \tag{39}$$

and

$$f_d^{(4)}(t, 0) = \frac{1}{24}\{4(K - E)K - (2t-3)K^4 - 6K^3E + 3K^3E^2 \tag{40}$$

It is remarkable all k the k fold integral $f_d^{(k)}(t, N)$ can be reduced to a sum of products with polynomial coefficients of one dimensional integrals. This is particularly remarkable because the two dimensional integral for $f_d^{(2)}(t, N)$ has been known [12] since at least 1973 without anyone suspecting that such a factorized from was possible. Thus the results of [13] provide an understanding for the results of [12].

4 Results versus conjectures

The properties of the form factor integrals $f_d^{(n)}(t; N)$ surveyed in (29) -(40) give new understanding of Ising model correlation functions. However, these properties themselves are not understood. Indeed, the entire study of the form factor integrals presented in [13] raises the question of the meaning of the words "results" and "conjecture".

In ref. [13] the form factor integrals for fixed (and reasonable small) N are expended in a power series on t on the computer and from a large, but finite, number of terms programs such as "seriestodiff" which are available on Maple are used to produce a differential operator $O_k(N)$ which annihilates $f^{(k)}(t; N)$. The factorization (30) is then produced by use of the "factor" command. This has been done for $0 \leq N \leq XX$. From those expressions a general form for all N was recognized and this is the form in which the results for $L_k(N)$ have been presented in [13].The direct sum form (31) has been similarly produced by using the homomorphism commands available in Maple but we have been unable to abstract a form which fits all the results in terms of an arbitrary N.

This totally computer based approach is by its very nature strictly valid only for a finite number of terms on the original power series expansion in t and only for a finite number of values of N, Therefore in the most literal sense of the words "we do not have a derivation of any of the results summarized in (29)-(40)" and again in the strictest sense of the words the very extensive set of results presented in [13] can be called a "conjecture".

In particular we do not even have an analytic proof of the evaluations of $f_d^{(2)}(t; N)$ given in (38).

However, in spite of this total lack of what a mathematician would call "rigor" and even what a mathematician would call "proof" there is not a shred of doubt that every single result presented in [13] is correct.

What is totally lacking is an understanding of what properties of $f_d^{(n)}(t; N)$ allow the factorization and direct sum properties to happen. This is of major importance because the decomposition of multiple dimensional integrals into sums of products of one dimensional integrals has been previously seen in the study of the correlation functions of the isotropic Heisenberg antiferromagnetic chain [15]. One understanding of the result of [15] has been given in terms of solutions of the qKZ equations [16]. The results found in [13] certainly demonstrate that this decomposition is an extremely general phenomenon and that its under sanding will be a major step forward in the study of correlation functions of all integrable models.

5 The 6 and 8 vertex models

A particularly nice example of how understanding is advanced is seen in the developments leading to Baxter's 1972 solution [17] of the 8 vertex model.

In 1967 Lieb discovered [18] that the ice model could be solved by the same Bethe ansatz [19] used to solve the XXZ spin chain. This solution of the ice model was soon understood to be generalizable to what is now called the six vertex model in a horizontal field [20]. The relation between the 6 vertex model and the XXZ chain was first understood by Wu and myself [21] by demonstrating the commutation relation between the six vertex transfer matrix $T_6(u)$ and the XXZ Hamiltonian H_{XXZ}

$$[T_6(u), H_{XXZ}] = 0 \tag{41}$$

For the case of zero horizontal electric field this commutation relation was subsequently understood by Sutherland [22] to be a special case ot the commutation of the transfer matrix of the (symmetric) eight vertex model T_8 with the Hamiltonian H_{XYZ} of the XYZ spin chain

$$H_{XYZ} = -\sum_{j=1}^{N} \{\sigma_j^x \sigma_{j+1}^x + \Gamma \sigma_j^y \sigma_{j+1}^y + \Delta \sigma_j^z \sigma_{j+1}^z\} \tag{42}$$

with

$$\Delta = \frac{\mathrm{cn}2\eta\,\mathrm{dn}2\eta}{1 + k\mathrm{sn}^2 2\eta} \tag{43}$$

$$\Gamma = \frac{1 - k\mathrm{sn}^2 2\eta}{1 + k\mathrm{sn}^2 2\eta} \tag{44}$$

of

$$[T_8(u), H_{XYZ}] = 0 \tag{45}$$

and the commutation relation (45) was understood by Baxter [17] by showing that it follows from the commutation relation of the transfer matrix of the 8 vertex model

$$[T_8(u), T_8(u')] = 0 \tag{46}$$

which he proved by deeply understanding the star triangle equation first introduced by Onsager [3].

The understanding of Baxter [17] was so profound that he was able to compute the free energy of the 8 vertex model for the special case where the "crossing parameter" η satisfies

$$2L\eta = 2m_1 K + i m_2 K' \tag{47}$$

where K and K' are the complete elliptic integrals of the first kind of modulus k and $k' = (1 - k^2)^{1/2}$.

But just as Baxter [17] understood Onsager [3] and the previous authors [18]-[22] who had studied 6 and 8 vertex models many authors (including Baxter himself) have worked hard since 1972 to understand the 1972 paper of Baxter [17]. Indeed it may be said that most of the work being presented at this workshop is in large measure an attempt to understand Baxter's several different solutions [17],[23]-[26] of the 8 vertex model and I cannot hope to summarize all of what has been done. Instead I will concentrate on a few attempts to understand the meaning of the restriction (47).

The introduction of (47) was a completely new feature of the 8 vertex model which at the time of its introduction had no counterpart in the 6 vertex model. However, in the course of over 3 decades the understanding has slowly emerged that in fact there is a deep connection between the condition (47) and the XXZ Hamiltonian

$$H_{XXZ} = -\sum_{j=1}^{N}\{\sigma_j^x\sigma_{j+1}^x + \sigma_j^y\sigma_{j+1}^y + \Delta\sigma_j^z\sigma_{j+1}^z\} \tag{48}$$

with

$$\Delta = \cos 2\gamma \tag{49}$$

with

$$\gamma = \pi m_1/L \tag{50}$$

which is what (42) and (47) specialize to when $k \to 0$.

The key property which the 8 vertex model with (47) shares with the 6 vertex model with (50) is the existence of multiplets of states with degenerate eigenvalues for both the transfer matrices and the spin Hamiltonians.

For the 6 vertex model these degenerate multiplets were first seen numerically and then understood [27] by demonstrating that when the root of unity condition (50)holds the 6 vertex transfer matrix has a symmetry algebra given by the loop algebra of sl_2 at least when the eigenvalues of

$$S^z = \frac{1}{2}\sum_{j=1}^{N}\sigma_j^z \tag{51}$$

satisfy

$$S^z \equiv 0 \pmod{L} \tag{52}$$

From this understanding there has followed a great deal of understanding of the relation of representation theory of affine Lie algebras with the Bethe ansatz [28],[29].

It is abundantly clear [30]-[32] that there must be a similar understanding of the degenerate multiplets of the 8 vertex model in terms of some symmetry algebra but until now the symmetry algebra is not known. It is very tempting to conjecture that the symmetry algebra is the quantum affine Lie algebra $U_q(\widehat{sl}_2)$ where q is a root of unity $q^{2L} = 1$ because it is known that in this case the L^{th} powers of the generators of the algebra are in the center [34] and of the value of these central elements is set equal to zero there is a reduction [35]to the loop algebra of sl_2. It is therefore plausible to conjecture [33] that for the 8 vertex model the corresponding algebra will be $U_q(\widehat{sl}_2)$ only now the central elements will have a value which only vanishes in the 6 vertex limit where $k \to 0$.

This conjecture could be tested if the finite dimensional representations of $U_q(\widehat{sl}_2)$ at roots of unity with non vanishing central elements were known. There are indeed several places in the literature [34],[35] where $U_q(\widehat{sl}_2)$ at roots of unity is studied but none of them give explicit representations for the case with nonvanishing central elements. Therefore we have discovered once more case where understanding is required.

6 Do we understand "Statistical Mechanics"?

The final topic I wish to consider is what do we mean by the term statistical mechanics.

Perhaps the most basic way we introduce statistical mechanics to students is by means of the microcanonical ensemble which rests on the notion that for a generic system the only conserved quantity is the energy. Thus we study properties of large systems by averaging over all sates with the either the same energy or, slightly more generally, over all states whose energy lies in some small interval.

The assumption that energy is the only conserved quantity and that we may average over all states with the same energy is called the ergodic hypothesis.

However, this does not genuinely represent the properties of real large systems once we take the thermodynamic limit. The reason is that systems in infinite space have more conservation laws than just energy. In particular a generic system will at least be invariant under spatial translations and rotations. Thus in the thermodynamic limit the phase space will decompose into pieces which will be representations of the space group. This is the origin of the phenomena of first order phase transitions and "metastability" which are a extremely common properties of real macroscopic systems and which are seen in the numerical studies of hard spheres in 3 dimensions.

But all the models which we study by means of star triangle equations are characterized by the property of having an infinite number of conservation laws. Thus we have the right to ask to what extent does the study of these integrable models give us an understanding into real systems which do not have any conservation laws beyond the space group of infinite space?

The more or less standard answer to this question is that there is a principle of universality which associates a class of generic systems to each integrable model. But no actual theorem has even been proven.

I personally believe that the best system to use to examine the question of the relation of integrable to generic systems is the Ising model in a magnetic field because while the Ising model at $H = 0$ is integrable and the Ising model at $T = T_c$ and $H \neq 0$ was shown by Zamolodchikov [36] to be integrable in the field theory limit the Ising model for $T \neq T_c$ and $H \neq 0$ is almost certainly not integrable in any sense.

. The reason that the study of the Ising correlation functions is so very important because we can begin to study the Ising model with $H \neq 0$ by expanding the free energy about $H = 0$ in terms of sums over the correlation functions. This produces for fixed T a power series in H and the coefficient of H^2 is the magnetic susceptibility

$$\chi = \frac{1}{kT} \sum_{M=-\infty}^{\infty} \sum_{N=-\infty}^{\infty} \{\langle \sigma_{0,0}\sigma_{M,N}\rangle - \mathcal{M}^2\} \tag{53}$$

where $\mathcal{M}$ is the spontaneous magnetization. By use of the form factor expansions derived from [5] these susceptibility sums may be written as

$$\chi_{\pm} = \sum_{j=1}^{\infty} \chi^{(j)} \tag{54}$$

where in the sum j is even (odd) for T below (above) T_c and where

$$\chi^{(j)} = \frac{1}{kT} \sum_{M=-\infty}^{\infty} \sum_{N=-\infty}^{\infty} f^j(M,N) \tag{55}$$

The sums for $\chi^{(j)}$ have been investigated by Nickel [37],[38] who made the extremely important discovery that each $\chi^{(j)}$ has singularities in the complex T plane and that as j increases these singularities form a dense set. Therefore if there is no cancellation the susceptibility will have a natural boundary.

The connection between solvability, in the sense of having an infinite number of conservation laws, with the notion of severe restriction on the analyticity of the free energy was first proposed by Guttmann [39]. Nickel's papers [37],[38] provide an understanding of Guttmann's work. The papers of Boukraa, Hassani. Maillard, Nickel, Orrick, Perk, and Zenine [40]-[44] provide an understanding of [37],[38] and recent papers [13],[14],[45]of which I have been a coauthor provide some understanding of all of these previous papers.

However, to truly understand these papers we need to definitively prove that there is no cancellation and that there does indeed exist a natural boundary in the susceptibility of the Ising model. Furthermore we need then to show that this natural boundary exists in all finite number of derivatives of the free energy and then we finally need to show that for some range of finite magnetic fields there is a natural boundary in the complex temperature plane in the free energy of the Ising model. If all of this can be proven true we will finally have some real understanding of the relation of solvable models to genuine statistical mechanics.

7 Is science objective?

We are all taught as children that science is objective. That if you do pioneering creative work which discovers new things and advances human understanding that you will be rewarded.

We are also taught that babies are brought by storks and are found under cabbage leaves.

Sometime between the ages of 10 and 14 we learn where babies actually do come from. But the myth of objectivity in research funding, academic hiring and the awarding of prizes is something which many people continue to believe for their entire lives.

The topics discussed in this lecture have all been carefully chosen to be questions which I believe are important but for which I do not think that at the present anyone has any answers for. Therefore the children's view of research would suggest that they would be ideal topics for a research proposal. In fact, exactly the opposite it true.

When Clisby and I published our derivation of B_4 for hard spheres in dimensions $D = 4, 6, 8, 10, 12$ we included the conjecture for B_k in even dimensions which I presented earlier. The referee forced us to remove it because "we had no evidence to support it".

The computer results I reported above for the Ising form factor integrals could surely not be published Communications of Mathematical Physics because they are not rigorous and they cannot be published in Physical Review Letters because they are too mathematical. In the paper we did publish we were forced to remove some of the discussion because it was "too speculative" and that we may not have met the "guidelines of the journal"

I dare not put into a refereed article the conjecture of that representations of quantum groups at roots of unity with nonvanishing central elements may explain the degeneracies of the 8 vertex model for fear of instant rejection by the referee.

The moral of this is that there is overwhelming pressure to never admit that we do not understand something. Any admission of a lack of understanding is used as a reason to not fund a grant, to reject a job application or to deny a prize. The person who confidently asserts that he understands a subject always has more popularity than the person who points out gaps in our understanding.

So I conclude with a piece of advice to all those who would like to believe that there is an objective basis for rewards in science. In your grant applications, letters of recommendation and statements of research interest write as though you understand everything. But deep in your heart remember that ignorance must always be the precondition of knowledge.

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