FROM NUCLEI
TO BOSE CONDENSATES

Festschrift for the 65th birthday of

Rajat K. Bhaduri

on May 6, 2000

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April 2000

Impressum:

Manuscript produced in LaTeX

Cover photographs: Stephanie M. Reimann and Susanne Viefers

Cover design: Franz Stadler

Printed at Chennai, April 2000

FOREWORD

On May 6, 2000, Rajat Bhaduri will celebrate his 65th birthday. At the same time, he will retire from his official faculty position at the Department of Physics and Astronomy of the McMaster University. We know that he will not retire from physics. But there is good reason to congratulate him on this day, and to thank him for the many original and inspiring contributions he has made to physics over the last 38 years. Most of them he developed with colleagues and friends all over the world.

We feel that Rajat should not be allowed to celebrate this day alone. Since he has been sharing his joy for physics with so many colleagues over so many years, we thought that those of us who can should be allowed to share also this memorable day with him. So, without asking him for permission, we have arranged a surprise party. We will hold a symposium for him on May 6, 2000, at McMaster University. During this symposium we will tell him when, how, and why he has given us new ideas and new impulses for our research in physics. But also how he has inspired us in many other ways and shared his friendship with us.

Not all of Rajat's friends and collaborators who would have liked to come are able to make the trip to Hamilton for that particular day. But those who can come look forward to it very much. And everybody who wants to give a talk will do so. Not for long, because at the end of the day we all want to be fit for a joyful dinner party, together with family members.

We are grateful to Akira Suzuki, our far-East correspondent, and to Jimmy Law, our local Ontario representative, for assisting in the organization of the symposium. In particular, we thank Jimmy for organizing the dinner party and taking care of many Hamilton connections. We thank Manju Bhaduri for being active behind the scenes while keeping up the secret – a most difficult job for one who is so close. Thanks are also due to Donald Sprung and the McMaster faculty for their support.

Most of the scheduled speakers of the symposium were able to send us a written manuscript ahead of time. Their papers are collected in this Festschrift, which also includes contributions from some colleagues who cannot participate at the meeting. The bibliography at the end of the volume contains all references to work in which Rajat was directly involved, but is by no means a complete list of his publications (which is at least twice as long). We thank Franz Stadler for professional help in preparing the cover of this booklet.

We want to present this Festschrift to Rajat Bhaduri on his birthday, as a testimony of some of the physics that he has inspired, and as a tribute to our friendship.

Regensburg, March 2000

Matthias Brack
Stephanie Reimann

Chennai, April 2000

M. V. N. Murthy

CONTENTS

1 RAJAT KUMAR BHADURI

Run, rabbit, run,
Dig that hole, forget the sun,
And when the work at last is done,
Don't stop,
It's time to start another one ...

Roger Waters

Rajat Kumar Bhaduri was born on May 6th, 1935, the youngest among nine brothers and sisters, in the town of Raipur in the province of Madhya Pradesh which is in the central part of India. He did his schooling and junior college in Raipur and in 1953, joined the Physics honours program in Presidency College, Calcutta. Upon graduation in 1955, he joined the M.Sc. program in "Radiophysics and Electronics" in Calcutta University. This was a three year program in what would be called telecommunications today. His records in Calcutta University were exemplary. Upon completion, he was selected by the Atomic Energy of India and, as was the practice, had to participate in a one year training program. It was in this training school that he switched directions and decided to choose theoretical physics as his vocation. He joined the Tata Institute of Fundamental Research and his first research project [4] was in the nuclear matter problem under the direction of Kailash Kumar, in which he was using the Thomas-Fermi model. Kailash Kumar had fond memories of working at McMaster University in Hamilton, Canada under Professor M. A. Preston and suggested to Rajat that he move to McMaster.

Rajat came to McMaster for his Ph.D. in the spring of 1961. For his Ph.D. he tested the suitability of a velocity dependent nucleon-nucleon potential for the nuclear two-body and many body problem [6, 7, 8]. He graduated in the fall of 1963 under the direction of Preston and went on a post-doctoral fellowship to work with Professor R. F. Peierls at Oxford. He came back to McMaster in 1965. In 1967 he went back to Tata Institute of Fundamental Research and after a brief sojurn in Bombay decided to come back to Canada and joined the faculty at McMaster. This is where he has done most of his work.

Rajat's early interest was in nuclear physics and, in the seventies, he co-authored a book "Structure of the Nucleus" [1] with M. A. Preston (a revision of a book originally written by Preston) on nuclear physics. His interests are widespread. In the eighties he wrote a book on "Models of the Nucleon" [2], a subject which straddles nuclear and particle physics. He has always been interested in semiclassical methods in all branches of physics and, with Matthias Brack, co-authored the book "Semiclassical Physics" [3] in the nineties. We do not know the subject of his book to be written in the next decade. This celebration in his honour has been appropriately called "From nuclei to Bose condensates", as it conveys some idea of his breadth of interest.

Apart from many students who did their doctorate under his supervision, Rajat collaborated, and still does, with many colleagues in Canada, Europe and Asia. He maintains a strong research contact in India (Madras, Bhubaneswar, Bangalore, Bombay and Roorkee). His love of physics and sunny outlook towards life are infectious. He lives in Dundas with his wife Manju, who teaches in Hamilton. All three of his children live in the Hamilton area.

He is addicted to physics, bridge and tennis, in that order.

Subal Das Gupta

2 HOW I ESCAPED COLLABORATING WITH RAJAT

During 1958-59, Rajat Bhaduri was a trainee (II batch) in the Training School of the Atomic Energy Establishment (AEE), Bombay. I had preceded him by one year and so considered myself senior to him, although in reality he was senior to me, having come to the Training School after finishing his Master's degree. But I did not really know him during this period, although I had valued one of his examination papers, along with those of others in his class. So I was his tutor and deserve his respect which he seldom gives to me.

In those days, the recruitment into the Tata Institute of Fundamental Research (TIFR) at Bombay was mainly from among the successful trainees from the Atomic Energy Establishment. Rajat, P. P. Divakaran, Sudhanshu Jha, K. V. L. Sarma, C. V. K. Baba, N. Mukunda and myself were a few from these early batches. At TIFR, in the beginning of our career, Rajat and myself had something in common. Both of us started in Nuclear Physics and both were consulting the Nuclear Theorist Dr. Kailash Kumar. Whereas I meandered into Particle Physics, Rajat stuck to Nuclear Physics and made rapid progress, not only in Physics but also in Life.

I suppose that Kailash Kumar introduced his bright student Rajat to his erstwhile mentor at McMaster, University Professor Mel Preston. Rajat decided to quit TIFR and go to Canada for his Ph.D. He became famous because of the following story that circulated among the youngsters at TIFR. Because of the training received at AEE, we had to give a bond stipulating that we serve the AEE or TIFR for a certain number of years. When an official of TIFR confronted Rajat with the bond, the latter shot back that he can speak to his lawyer. And, the lawyer was none other than his own brother! This phase of the story ends here since I lost touch with him for a while after he left TIFR.

Our world lines crossed again in 1963 at Oxford, with Rajat as a postdoc of Prof. R. Peierls and me as a student of Prof. R. H. Dalitz (who had been a student of Peierls). Rajat was newly married and came with his young wife Manju. Oxford was a rather dreary place, especially because I was busy completing my Ph.D. thesis and could not participate in any of the academic or cultural activities there. It was the presence of Rajat and Manju that provided the human warmth that helped me to bear with Oxford and concentrate on my work.

During the Oxford period, Rajat, Manju, Ashish Datta (a friend of the Bhaduries and later of myself too) and I went on a tour of Europe. Ashish was at the wheel, I was the map-reader and conductor, Rajat was the joker and entertainer and Manju provided the dose of respectability required by this bunch of vagabond Indians. We started from Paris, went to the southern coast, came to Switzerland, passed through Germany and then to Italy. It was a memorable trip.

The scene now shifts back to Bombay and the period is the late 60's. I have a job in TIFR (I did not break the bond like Rajat!), but Rajat also is offered a job there. But fate intervenes. Bombay is a difficult place to live unless you are rich. In those days it was much worse since the TIFR campus (with the comfortable quarters that it boasts of now) had not yet come up and many of us lived far away in north Bombay. Our quarters were located in one of the worst areas infested with buffaloes and mosquitoes; in fact, it was called "buffalo colony". Rajat made one visit to me in our apartment and in his typical quick and woolly-headed fashion hired a similar apartment situated in an equally rotten locality and told Manju to move there. She, being much more sensible and level-headed, refused to do that. The net result was that they rejected Bombay and moved to Hamilton. Thus I lost a valuable collaborator because of buffaloes and mosquitoes.

After that, our world lines diverged for quite a while, especially because I moved southward to Madras. In the early 80's, Rajat wrote to me saying that he wanted to learn QCD from me and proposed to spend some time with me at Madras. Actually his letter addressed to me at Madras reached me after a long delay since I was spending an extended period in Japan. Thus by being away at the right time, I escaped again from being his collaborator. Whether he really learnt QCD is another question.

I finally come to the modern times, the 90's. We began to meet each other more frequently, in Bombay or in Madras. Rajat tried to teach me his current passions – chaos, anyons, semiclassical physics etc. etc., but all in vain. He even invited me to Hamilton, hoping to convert me. Instead, I lectured there on neutrinos and tried to convert him!

During one of his recent visits to Madras, one day we found ourselves arguing with each other vehemently on some question of quantum statistics. The argument was becoming progressively hotter and we might have come to blows at any moment. We stopped short of that, but neither he nor I convinced the other or agreed with the other. Of course, a not-altogether-trivial factor that contributed to the argument was the percentage of alcohol in our blood which was rising rapidly at that very moment!

Although I have not yet written a joint paper with him yet, both of us are still young and we have high hopes!

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3 VELOCITY-DEPENDENT FORCES: FROM NUCLEAR PHYSICS TO PRE-MAXWELLIAN ELECTRODYNAMICS

Velocity-dependent realistic nucleon-nucleon forces

In order to account for the strong repulsion seen in high-energy nucleon-nucleon (NN) scattering, it had been assumed throughout the 50's that the NN potential contains, despite its overall attraction, an infinitely hard static core.[1] This made life very difficult for all those theorists who were attempting to understand the properties of nuclei, and particularly the limiting case of infinite nuclear matter, in terms of the NN interaction, because it meant that ordinary perturbation theory would simply blow up. The answer to this difficulty was the series of papers by Brueckner and others, but their theory was very complicated, and even though the first results on nuclear matter were encouraging, there were always questions of convergence and the need for corrections of still greater complexity.

However, at the Kingston conference in 1960, Peierls[2] pointed out that while the repulsive hard core was certainly consistent with the high-energy scattering data, it was not the only possibility. In particular, he suggested that it might be possible to represent the strong short-range repulsion that certainly exists by a velocity- (or momentum-) dependent NN interaction,

$$V(r,p) = g(r) + \frac{1}{2}\{p^2 f(r) + f(r)p^2\}\,. \tag{1}$$

Since such a NN force is non-singular ordinary perturbation theory is at least formally applicable, and there was some hope at first that it might even converge rapidly enough to offer a practical approach to the calculation of nuclear properties in terms of the NN scattering data.

A short time later Rajat Bhaduri entered the picture, when he came to Mac to begin graduate work under Mel Preston. For his thesis work he took up Peierls' idea, one of the first to do so, and showed firstly how the nuclear-matter results are far from unique when a velocity dependence is admitted to the force, even under the constraint of fitting the NN data. Nevertheless, by paying sufficient attention to the precise form of the velocity dependence it was possible to get the same nuclear-matter results as given by a static hard-core potential fitted to the same data [5, 6, 7, 8].

However, it was quite clear by this time that as long as the high-energy scattering data were being fitted, even velocity-dependent forces were not soft enough to allow perturbation theory to be used in any meaningful way, and that Brueckner theory, or something equally complicated, could not be avoided. Thus there was no advantage to retaining velocity-dependent forces, and those people who were still trying to understand nuclear properties in terms of realistic NN forces drifted back to fitting the NN data with simpler static potentials, sometimes with infinite hard cores, sometimes, as in the case

[1] R. Jastrow, Phys. Rev. **81**, 165 (1951)

[2] R. E. Peierls, in: Proc. of Int. Conf. on *Nuclear Structure*, Kingston, ed. by D. A. Bromley and E. W. Vogt (University of Toronto Press, 1960), p. 7

of Don Sprung and Roland de Tourreil, with "super-soft" cores, but never so soft that perturbation theory could be used successfully.[3]

Velocity-dependent effective forces

But while many workers continued to slog away on Brueckner theory, and its various extensions and alternatives, others began to follow the more modest, but probably more immediately rewarding, approach to nuclear-structure theory opened up in the middle 60's with the demonstration by Michel Baranger and collaborators that nuclear Hartree-Fock (HF) calculations were a feasible proposition.[4] This is a variational method in which the trial wave-function has the independent-particle form, the rationale for which lies in the undoubted validity of the shell model. But an equally well established feature of nuclear physics is the strong short-range repulsion in the realistic NN force, and since the short-range correlations to which it will give rise are not present in the trial function, the force used in the HF calculation cannot be the real one but rather must be an effective force in which the realistic short-range repulsion has been considerably softened. Thus, in following this approach one gave up, at least temporarily, the hope of relating nuclear structure to the "realistic" NN force, although implicitly the long-term intention must have been to determine some universal effective force that could correlate as much nuclear data as possible, after which one might hope to be able to relate this effective force to the realistic force. (Later it was realized that even without being able to relate the effective force to the real force, the HF method provided an excellent means of extrapolating nuclear data to those experimentally inaccessible nuclei that play a vital role in stellar nucleosynthesis.)

As to the choice of effective force, Rajat and Ed Tomusiak made the point, at a very early stage, that it must lead to the correct saturation properties of nuclear matter if finite nuclei were to be fitted [9]. However, this leaves considerable freedom, and while the Baranger group used an effective force that had the velocity-dependent form of Eq. (1), the Gogny group has enjoyed considerable success with purely static potentials.[5] However, velocity-dependent effective forces really came into their own with the application of the Skyrme force[6] to HF calculations by Vautherin and Brink.[7]

Direct inelastic scattering

At some point towards the end of the 1960's it occurred to me that it would be rather nice to see to what extent one could use the same effective interaction in HF and in calculations of the direct inelastic scattering of nucleons. There was, of course, no reason why the two forces should be identical, but they are not completely unrelated either, and the simplest starting assumption to make is that they are in fact identical. Since I was already involved in HF calculations using velocity-dependent forces, this meant that we had to do direct-reaction theory with such forces, and the first thing I learned was that no one had ever worked out the formalism for this case. So I set a graduate student, Elie Boridy, on the problem, and in the fullness of time he reported that the velocity

[3] J. Côté, B. Rouben, R. de Tourreil, and D. W. L. Sprung, Nucl. Phys. **A 273**, 269 (1976)

[4] K. T. R. Davies, S. J. Krieger, and M. Baranger, Nucl. Phys. **84**, 545 (1966)

[5] J. Dechargé and D. Gogny, Phys. Rev. **C 21**, 1568 (1980)

[6] T. H. R. Skyrme, Phil. Mag. **1**, 1043 (1956); Nucl. Phys. **9**, 615 (1959)

[7] D. Vautherin and D. M. Brink, Phys. Rev. **C 5**, 626 (1972)

dependence gives rise to far bigger effects than we expected, namely, there is a relaxation of some standard selection rules.[8]

To see what happens it is easier if we consider just the scattering of spinless particles such as alphas. (The effective interaction between the α-particle and a target nucleon can be regarded as resulting from a folding of the effective NN force over the α-particle, and it will presumably be velocity-dependent if the effective NN force is.) Now according to the "normal-parity" rule, if the target nucleus is initially in a 0^+ state α-particles can excite only states of normal parity, $\pi = (-)^{J'}$, where J' is the total angular momentum of the excited target nucleus. In a single-stage process with a purely static and central effective force this rule holds rigorously if exchange is neglected, essentially because in the multipole expansion of such a force the only operators that can appear are of the form Y_l.

However, things are much more complicated in the presence of a velocity-dependent force of the form of Eq. (1). Because of the famous identity

$$\boldsymbol{\nabla} = \hat{\mathbf{r}}\frac{\partial}{\partial r} - \frac{i}{r^2}(\mathbf{r} \times \mathbf{L})\,, \tag{2}$$

where $\mathbf{L} = \mathbf{r} \times \mathbf{p}$, the multipole expansion of the force will now contain operators of the form $\Big[[Y_1\mathbf{L}]_1 Y_l\Big]_l$, which will clearly excite states of parity $(-)^{l+1}$. Thus the normal-parity rule for the inelastic scattering of α-particles will be violated for a momentum- dependent force.

Experimentally, the normal-parity rule is indeed found to be violated, but this does not prove that the effective interaction must be velocity-dependent, since there are other possible explanations: two-stage processes (including compound-nucleus formation), spin-orbit forces, and exchange effects. Rather than evaluate the relative importance of these different mechanisms, I want to devote the rest of this paper to trying to get some insight into how the selection rules can be changed just by making the force velocity-dependent. From the point of view of the formalism, of course, there is no problem: the new tensors do the trick, but the force is still central, and one would like some physical understanding of what is going on.

We begin by noting that if we denote by $\mathbf{l}_\alpha$ and $\mathbf{l}'_\alpha$, respectively, the initial and final angular momentum vectors of the α-particle, then we must have

$$\mathbf{l}_\alpha = \mathbf{l}'_\alpha + \mathbf{J}'\,, \tag{3}$$

since the initial state of the target nucleus has zero angular momentum. Then if there is no change in the *direction* of the alpha's angular momentum, it follows that $J' = |l_\alpha \pm l'_\alpha|$. Since the parity change of the target is given by $\delta\pi = (-)^{l_\alpha + l'_\alpha}$ we see at once that a violation of the normal parity rule requires that the alpha's angular momentum change direction during the scattering, i.e., the alpha's trajectory must undergo "orbit tilt" (for nucleons, spin flip plays the same rule). (In this respect it is significant that of the several new tensors that the momentum dependence gives rise to, the only ones that break the normal-parity rule are those that contain $\mathbf{L}$: it is the operators $\mathbf{L}^{\pm 1}$ that do the tilting.)

Of course, when the plane of the alpha's trajectory tilts, the plane of the shell-model orbit of a nucleon in the target nucleus must tilt in the opposite direction in such a way that angular momentum is conserved. It is easy to see how a velocity-dependent force will

[8] E. Boridy and J. M. Pearson, Phys. Rev. Lett. **27**, 203 (1971); Nucl. Phys. **A 193**, 113 (1972)

do this. If we naively think of a nucleon orbiting around in the nucleus like an electron in a coil, its velocity relative to the bombarding alpha will vary, depending on where it is in the coil. Thus the force on one side of the coil will be different from the force on the other side, and a torque will result.

The Weber force

Getting this simple physical picture of what was coming out of all the tensor algebra was quite satisfying, but we became really excited when the image of orbit tilting reminded us of what actually happens when a wire through which flows an electric current passes by a small coil carrying a current: according to Ampère's law the two tend to swing together into the same plane. So it began to look as though one might be able to re-write electrodynamics in terms of velocity-dependent forces of the form of Eq. (1). Of course, so far all this is just qualitative, so let us see what happens quantitatively.

We must first recall that the expression given by Eq. (1) does not represent a "force" at all, but rather is to be inserted into a Hamiltonian, thus

$$H = \frac{p^2}{2m} + V(r,p)\,. \tag{4}$$

Treating this as a purely classical object, and using Goldstein, especially the section on velocity-dependent forces in Ch. 1, we find for the corresponding force[9]

$$\mathbf{F} = \Big\{ - g'(r) + h(r)\ddot{r} + \frac{1}{2}h'(r)\dot{r}^2 \Big\}\,\hat{\mathbf{r}}\,, \tag{5}$$

where

$$h(r) = 2m^2 \frac{f(r)}{1+2mf(r)}. \tag{6}$$

If this is to have any relation to electrodynamics, then clearly for the static term, $g(r)$, we should take the Coulomb potential, q_1q_2/r. If we then choose $f(r)$ in such a way that $h(r) = g(r)/c^2$, it turns out that we actually get the correct Ampère law for the force between two wires each carrying an electric current,

$$\mathbf{F} = -\frac{I_1I_2}{c^2}\oint\oint \hat{\mathbf{r}}\frac{d\mathbf{r_1}\cdot d\mathbf{r_2}}{r^2}. \tag{7}$$

Alas, we soon found that we had been scooped – by 125 years. An expression of the form (5), with the above choice for $f(r)$ and $g(r)$, had already been proposed in 1848 by Weber[10] for the force between two moving charges. And of course, although Weber's theory had a number of successes it also had some fatal flaws, the most conspicuous of which in retrospect is the inability to provide for a wave motion.

So Maxwell's equations are necessary after all. Nevertheless, it is remarkable that the old Weber theory, while incorrect as a theory of electrodynamics, should have resurfaced in nuclear physics,[11] the velocity-dependent forces represented in Eq. (1) being nothing

[9] J. M. Pearson and A. Kilambi, Am. J. Phys. **42**, 971 (1974)

[10] W. Weber, Ann. Phys. (Leipzig) **73**, 193 (1848); see also E. Whittaker: *A History of Theories of Aether and Electricity*, Ch. VII

[11] It is interesting to note that a modified Weber force can also account *exactly* for the precession of the perihelion of Mercury: see E. Whittaker, *loc. cit.*

but a generalization of the old Weber force. Rajat was one of the earliest practitioners of these forces in nuclear physics, right at the beginning of his career, but they are still widely used, especially in the form of the Skyrme force. It is nice to know that they have such a venerable pedigree.

I am indebted to two colleagues, Pierre Depommier and Jean LeTourneux, for some crucial remarks.

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4 DEFORMATION EFFECTS IN URANIUM ON URANIUM COLLISIONS

It has been conjectured that deformation effects will be very significant in U on U collisions at relativistic energies. The effects are calculated by modeling of collisions of two deformed intrinsic states. We point out this overestimates the effects of deformation. Without polarized beams there is practically no effect. Beams polarized in the 2+ states will show some effect but much reduced from the predicted results.

Introduction

There have been speculations if deformation would strongly influence observables in heavy ion collisions at high energy. For example, what difference one would expect to see in event by event analysis of Uranium on Uranium (U on U) collisions as opposed to Pb on Pb. The difference between the two is that the ground state band of U can be generated from a deformed intrinsic state whereas Pb ground state as well as excited states are basically spherical. Model calculations considered collisions of two deformed objects: tip-tip collisions (long axes head on) and body-body (short axes head on and long axes parallel) collisions. Significant differences are found, for example, in the elliptic flow, in the central density achieved, in K+ production etc.

Our contention is that to analyze the influence of deformation on observables one has to pay particular attention to how the colliding beams are prepared.

This work was done in collaboration with Charles Gale, who is also speaking in this symposium but on a different topic.

The Uranium ground state

Suppose the colliding beam has only the ground state of even-even Uranium. What is the nature of the ground state? One can do a shell model calculation to obtain this but it is well-known that a good approximation to the ground state can be obtained by doing a deformed Hartree-Fock-Bogolyubov calculation from which the ground state can be projected. Thus

$$\Psi_{JM}(x) \propto \int d\Omega D^J_{M0}(\Omega)^* \hat{R}(\Omega)\Phi_0(x) \tag{1}$$

We use the convention of Rose for D functions. Here the subscript 0 in Φ means that the Hartree-Fock-Bogolyubov solution has axial symmetry (k=0); Ω stands for the three Eulerian angles α, β and γ. Eq. (1) is easily established by noticing that the Hartree-Fock-Bogolyubov solutions can be written as

$$\Phi_0(x) = \sum a_{I0}\Psi_{I0}(x) \tag{2}$$

The physical solutions are Ψ_{I0}'s; a_{I0}'s satisfy $\sum a_{I0}^2 = 1$. If the beam has only the ground state of U then we should use $J = M = 0$. This is completely spherically symmetric. Thus we have two spherically symmetric densities hitting each other.

The density in the ground state

The shell model $\Psi_{00}(x)$ of Eq. (1) is very complicated and to obtain the ground state density from it will be very hard. We should exploit the fact that these nuclei are very well described by the Bohr-Mottelson model according to which we write

$$\Psi^{BM}_{JM}(x) = \left(\frac{2J+1}{8\pi^2}\right)^{1/2} D^J_{M0}(\Omega)^*\tilde{\Phi}_0(\Omega x') \tag{3}$$

Here $\tilde{\Phi}_0$ is the BM intrinsic state. The number of coordinates in x' is $3N-3$ where N is the number of particles. The symbol $\Omega x'$ means that the intrinsic state is at orientation $\Omega = \alpha, \beta, \gamma$ with respect to space fixed system; $(\frac{2J+1}{8\pi^2})^{1/2} D^J_{M0}(\Omega)^*$ is the amplitude that the intrinsic state is at this Ω.

Exploiting the fact that the intrinsic state has $k=0$, the density in the JM state is

$$\rho_{JM}(\vec{r}) = \int sin\beta d\beta d\gamma |Y_{JM}(\beta,\gamma)|^2 \tilde{\rho}(\beta,\gamma,x') \tag{4}$$

Here we have indicated that the intrinsic state density $\tilde{\rho}(x')$ is tilted (its symmetry axis is tilted) at angle β, γ with respect to axes in the lab. It is this $\tilde{\rho}$ that is used by Shuryak and Li. In keeping with their parametrization we take $\tilde{\rho}$ to be a spheroid with semi-axes R_l and R_s. For simplicity, constant density is assumed in the intrinsic state. If the nucleus were spherical, incompressibility of nuclear matter dictates the radius of the equivalent spherical nucleus would have been $(R_s^2 R_l)^{1/3}$. If the beam has only the ground state we should use Y_{00} in the above equation. The resulting density $\rho_{00}(r)$ is entirely spherical. Any deformation that may be apparent in a given event will be due to fluctuations of positions within this spherical nucleus. This effect will be small. It is worthwhile noting that even though $\tilde{\rho}$ is constant, in the spin zero ground state $\rho_{00}(r)$ is constant only up to distance $r = R_s$ and beyond will decrease gradually to zero at $r = R_l$.

Polarized beams

Granting that deformation effects are lost in a beam which has the ground state only we consider polarized beams. Let us call the beam direction to be the z-direction. Then if both the target and the beam are in Ψ_{20} states we will have approximately tip-tip collisions. Even then quantum mechanics significantly smears out the effect. For quantitative estimates let us consider $\langle|z|\rangle$ and $\langle|x|\rangle$ where $\langle|z|\rangle$ is the value in the beam direction and $\langle|x|\rangle$ is the value perpendicular to the beam direction. For tip-tip collisions of deformed intrinsic states these are 3.15 and 2.44. Of significance is their ratio which is 1.29. With colliding nuclei each in Ψ_{20} in the beam direction these changed to 2.83 and 2.61, respectively. The ratio comes down from 1.29 to 1.08. In Y_{22} states (body-body collisions), this ratio is 0.93 compared to $1/1.29 = 0.76$ if collisions between intrinsic states are considered. The numbers for polarized beams were found by numerical computation.

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5 ROTATING NUCLEI AND OTHER TOYS

I have noticed that I am the only experimentalist giving a talk at this celebration of Rajat's sixty-fifth birthday. I find this to be somewhat unnerving. Many experimental groups have what we call a "tame theorist". This is a person who is capable of explaining theoretical concepts to experimentalists. Rajat has been one of our "tame theorists". How absurd that sounds to refer to Rajat as tame!

This talk is really about images and connections. When one thinks of a scientist, one often has a mental picture. This picture is often not really close to reality. However some of these images have become very famous; recognized by scientists and the public at large. For example, there is the famous one of Einstein riding a bicycle seen in Figure 1. Now all of us have an image of Rajat in our mind. These images are probably all different. Some of us picture him at a blackboard and others may see him pecking away at a computer terminal. I'll come back to my picture of Rajat at the end of my talk.

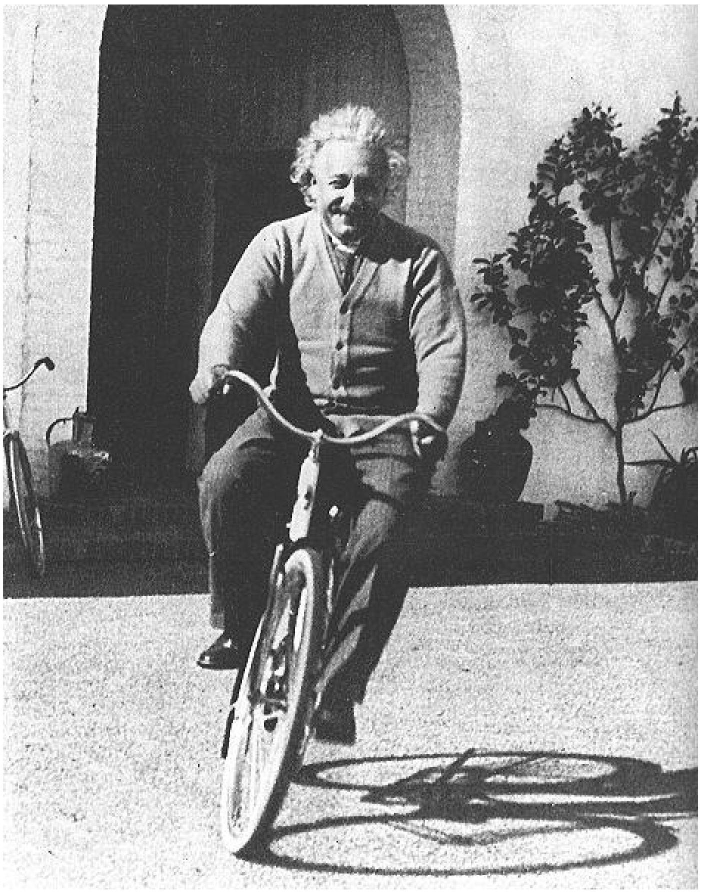

Figure 1: Einstein riding a bicycle. We often have a mental picture of a scientist

I have been associated with Rajat for a long time. I would like to give you my personal view of the great time that we have had together. One of the peculiar things about our association is that most of our interactions have dealt with spinning objects. Figure 2 is adapted from a figure made by Witek Nazarewicz (another tame theorist) and shows what he refers to as "Rotations in the Universe". This figure illustrates an interesting relationship between the time taken for an object to rotate around once, i.e., its period and the size of the object. The amazing thing about this figure is that it shows objects whose size differ by 60 orders of magnitude! It includes things like the earth and galaxies and more hypothetical objects such as superstrings and they all fit nicely on one linear relationship. It really does show how rotations are universal. Rajat has made a number of important contributions that are related to this figure and I have had the good fortune to take part in some of these.

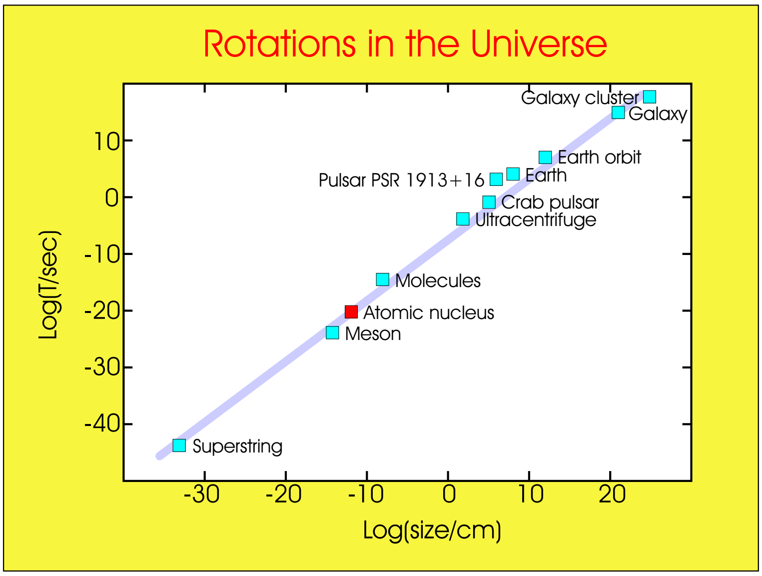

Figure 2: Rotations in the Universe. This figure which was adapted from one made by Witek Nazarewicz shows that rotations are ubiquitous.

One would think that my most obvious connection to Rajat's work should involve the physics of the nucleus. He wrote one of the standard graduate texts with Mel Preston. Indeed, a number of years ago Rajat and I worked for some time on an interesting problem in nuclear physics. It is not understood why rotational bands in neighbouring nuclei often have identical spectra. This is the so-called "identical band" problem. We tried a number of things to explain this. I thought about vortices in rotating nuclear fluids and the similarity of the rotating nucleus to the models for rotating stars that were developed by Subrahmanyan Chandrasekhar.[1] We spent some time on this and I talked about it in a meeting in Strasbourg [45]. However, the idea of how vortices are involved in the motion of rotating nuclei was developed much more completely by another speaker in this meeting: Philippe Quentin and his colleagues.

When I first taught mechanics, Rajat helped me with the solutions to a number of problems in planetary motion; objects rotating around the sun. He was always willing to help but occasionally ran off to work on another problem in rotational motion. Playing tennis and putting spin on a tennis ball is one of his passions. I said that he ran off to

[1]S. Chandrasekhar, *Ellipsoidal Figures of Equilibrium* (Yale University Press, 1969)

play tennis. Possibly "ran" is the wrong word, because he was more often racing away on his bicycle.

Many times during the past 30 years I went to his office to discuss something that I didn't understand, or to suggest a new idea to him. He always found time to give me an impromptu lecture on his board about it. If I still didn't understand when he was finished then he would often go to his filing cabinet. Then he would bring out a yellowed document, "a little unpublished work". This would turn out to be something he had done years before and showed that he had already worked on what I thought was my new bright idea.

We once published a peculiar paper on the rotational spectrum of the nucleon [31]. This was an interesting idea of his that the spectrum of excitations of the nucleon could be classified in the same manner as excitations of the nucleus. Thus the spectrum of the baryons are generated by considering them as a rotating bag of quarks.

Our next step, or should I say spin, with rotating objects was to study the periodic orbits associated with the rotating harmonic oscillator [54]. We'll hear more about this in Kaori Tanaka's presentation, but suffice it to say it has applications in nuclei and in quantum dots.

You can see that I've had a great time with Rajat, so far. So I've made up a new version of the "Rotations in the Universe" slide. It is the rotations in Rajat's universe, Figure 3. You can see that Rajat has "taken quite a spin" from the very small to the very large; quarks only 10^{-18} m across in baryons with a radius of 10^{-15} m, nuclei, quantum dots, tennis balls, bicycles, spinning planets and stars and planetary orbits up to a size of 10^{12} m. This represents a range of 30 orders of magnitude! But Rajat, we don't need to stop here. We'll have to hurry though. We have still 30 more orders of magnitude to go!

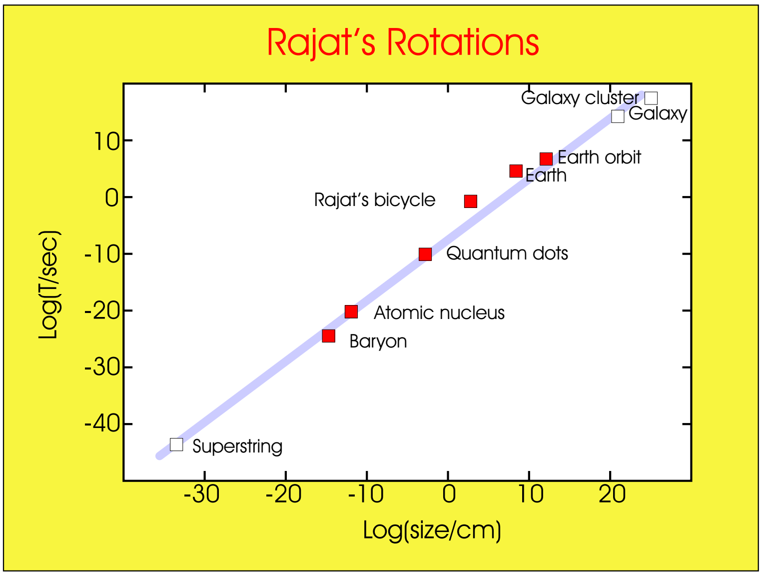

Figure 3: Rotations in Rajat's Universe. In this figure the filled squares illustrate some of the rotations that Rajat has studied. The open squares represent some of the problems in the 30 orders of magnitude that Rajat hasn't tackled yet.

Now let's go back to the idea of images. Remember that I said that we think of a scientist in terms of a mental picture. I want to secure Rajat's financial health in his retirement. I propose that he sell pictures of famous physicists. The business may have to start small, but after some initial success I am sure that it will grow. And I propose the image shown in Figure 4 for his first sale.

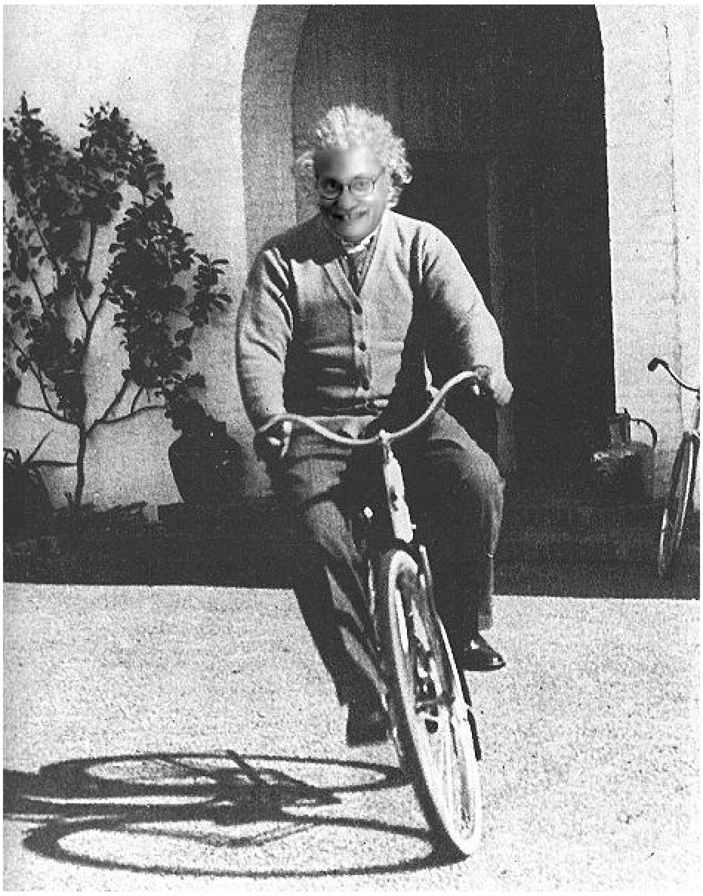

Figure 4: In order to finance Rajat's retirement, the author suggests that Rajat begin selling pictures of famous scientists. This could be his first best seller.

Thank you, Rajat. Happy Birthday.

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6 ANGULAR MOMENTUM CONTENT OF DEFORMED INTRINSIC STATES

In quantum mechanics there is more to angular momentum than the mere rotation of a classical distribution of matter, should it be rigid or not. One well-known example of this fact is related with the concept of an intrinsic state, central to the liquid-drop description of nuclear vibrations and rotations in the Bohr-Mottelson approach.[1] In practical many-body calculations relying, for most of them, on an independent fermion approximation, one produces such intrinsic deformed states which violate the rotational symmetry, as is well known. Peierls and Yoccoz[2] have shown many years ago how to restore this symmetry within an appropriate configuration mixing of the generator-coordinate method type.

In a paper of Rajat Bhaduri [22] whose pedagogical clarity, technical elegance and physical insight is typical of the style of his and his collaborators' works, he studied together with S. Das Gupta the angular momentum content of such intrinsic states calculated within the Hartree-Fock or Hartree-Fock-Bogolyubov approximation as obtained à la Peierls-Yoccoz. As a matter of fact, this paper took the standpoint of viewing the intrinsic state as a kind of thermal average of states belonging to a rotational band. In doing so, they assumed a perfect rotor energy character for the states projected out of the intrinsic wavefunction. Through a well-justified use of the high-temperature limit for the corresponding partition function, they came out with a very useful approximate expression for the components of the considered intrinsic state on good angular momentum states. The "temperature" parameter standing in this approach appears to be proportional to the intrinsic-state expectation value of the squared many-body angular momentum.

In the following, I would like to make two remarks related with the above summarized paper. They stem out of a study which has been initiated in a collaboration with Igor N. Mikhailov (BLTP Dubna).[3]

The first one relies on the trivial fact that one cannot formally distinguish between a Boltzmann factor for a rotational energy and a distribution factor behaving as an exponential in terms of $J(J+1)$. It appeared to us that the physics of an intrinsic state is rather well formulated in terms of a rotor, forced by a shape-constraining field, or in other terms as a kind of quantum pendulum. Upon treating it in a way similar to what has been done by Lo Iudice and Palumbo[4] to describe the scissor modes, one finds, indeed, in the high intrinsic J^2 limit, that the ground-state wavefunction has exactly the same expansion coefficients in angular momentum as obtained in Ref. [22]. This clarifies, we think, the somewhat obscure nature of the temperature parameter introduced in this paper.

For a practitioner of Hartree-Fock calculations, the actual necessity of dealing with microscopic solutions of deformed nuclear states which are merely intrinsic states and not true observed states, comes somewhat as a frustration. And then one wonders whether one could think of a nuclear system that could be really polarized, so that at least in

[1]initiated in Aa. Bohr, Dan. Mat. Fys. Medd. Vid. Selsk., **26**, No. 14 (1952)

[2]R. E. Peierls and J. Yoccoz, Phys. Soc. **A 70**, 381 (1957)

[3]I. N. Mikhailov and P. Quentin, Phys. Lett. **462 B**, 7 (1999); I. N. Mikhailov and P. Quentin, Proc. Int. Conference on *Fission and Neutron Rich Nuclei*, Saint Andrews (Scotland, U.K.), June 1999 (World Scientific, in press); I. N. Mikhailov, Ch. Briançon and P. Quentin, Proc. Int. Symposium Soloviev, RIKEN (Japan), 1999 (World Scientific, in press)

[4]N. Lo Iudice and F. Palumbo, Phys. Rev. Lett. **41**, 1532 (1978)

one single case one could come closer with a Slater determinant, or quasiparticle vacuum as well, to the description of a real physical situation for a change. Nuclei embedded in strong quadrupole lattice fields could provide such opportunities. However, as well known, the decipherament of the resulting signals is not very easy and mixes our difficulties of describing accurately both the nuclear structure and the lattice physics. A transient nuclear system could provide, we think, such an opportunity – and this is the topic of my second remark.

Just after the scission time (loosely defined as the time after which the strong interaction does not play any more a significant role in the fissioning system evolution) each fragment is indeed polarized by the other. Then we considered that due to the Heisenberg principle, the orientation information so provided should reflect itself in a distribution of the canonically conjugated quantities, namely the angular momenta in each fragment, that should be hidden somewhat in the intrinsic-state distribution of the deformed fragment as studied by Bhaduri and Das Gupta [22]. Of course, while doing so one should carefully consider the total angular momentum conservation for the whole system (e.g., for the spontaneous fission of an even-even nucleus one should conserve a vanishing total spin). Such a distribution thus yields a finite average value for the angular momenta of the fragments which have been measured since a very long time in some kind of averaged way and is now the subject of intense experimental efforts upon using the very high selectivity gamma-ray multidetector arrays (like EUROBALL or GAMMASPHERE), coupled or not with charged-particle (the fragments) multidetectors (like SAPHIR) or fragment-mass analyzers.[5] A natural question is, of course, to know whether or not this uncertainty principle mechanism (which we have dubbed as an orientation pumping mechanism) is able to account for most of the observed fragment spin for spontaneous or low-energy fission. This is all the more interesting that so far the standard explanation for the observed average values was referring to a completely different mechanism, namely a thermal excitation of collective modes before scission, like the one known as the bending mode.[6]

This is where the simple formula for the expansion coefficients found in the paper of Bhaduri and Das Gupta [22] comes into play. As a first estimate of the fragment angular momentum content, we described the total wavefunction after scission as a product of two separated intrinsic state wavefunctions (say e.g. two deformed BCS wavefunctions) projected onto a vanishing total angular momentum state. In order to minimize as much as possible other sources of fragment angular momentum generation (so that we can come to stronger conclusions on the ability of the pumping mechanism to yield about the right spin content of each fragment) we considered here a zero relative angular momentum between the fragments. Then one obtains when using the expansion coefficients of Ref. [22], the following very simple expression for the squared angular momentum expectation value J_i^2 corresponding to the fragment i in terms of the intrinsic expectation values of the same operator for both fragments j_i^2

$$\frac{1}{J_1^2} = \frac{1}{J_2^2} = \frac{1}{j_1^2} + \frac{1}{j_2^2} \tag{1}$$

Of course the equality of the moments reflect the conservation of the vanishing total

[5] for old and recent experimental results see, e.g., the references given in the papers of I. N. Mikhailov and P. Quentin quoted above in Ref.[3]

[6] see, e.g., M. Zielinska-Pfabe and K. Dietrich, Phys. Lett. **49 B**, 123 (1974)

angular momentum. When computed for realistic fission fragments one gets typically $j_i^2 \approx 100\hbar^2$ yielding thus average spin for each fragments in the $J_i \approx 7\hbar$ range as observed. This conclusion should only assign to the bending mode contribution a corrective role. As a matter of fact it has appeared increasingly clear that the latter model explanation was somewhat inadequate in many respect. In particular, it necessitates the introduction of so-called temperature of the order of 3 MeV which are inconsistently high with respect to experimental excitation energies. Furthermore it was unable to explain the observed structure of the average angular momentum as a function of the total excitation energy deposited in the fragments, contrarily to the present mechanism which provides a clean cut explanation of the low value of the momenta for cold fission (no neutron emitted) and its sharp rise when the number of emitted neutrons increases.

Clearly, more refined versions of the model are needed. They are currently studied in particular in so far as the projection on good angular momentum states are concerned.

The primary intent of this short note was to convey the deep gratitude that I am strongly feeling, as many physicists do, who have been lucky enough to come somewhat close even for a limited time to Rajat Bhaduri.

Thank you very much, Rajat, for the generous share of illuminating remarks and inspiring comments.

Happy birthday!

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7 THE PAULI PRINCIPLE IN VIRTUAL STATES OF PERTURBATION THEORY

In reformulating quantum electrodynamics (QED) in his own style, Feynman advocated in 1949: "It is obviously simpler to disregard the exclusion principle (of Pauli) completely in the intermediate states."[1] This was based on the observation that effects of all virtual processes that violate the Pauli principle formally cancel out. Feynman's prescription to disregard the Pauli principle in all virtual states underlies the Feynman diagram technique that accomplished enormous transparency of QED. Feynman's prescription is also widely applied to many-body problems in quantum mechanics with interesting implications.[2]

One of the problems which Rajat Bhaduri, Benoit Loiseau, Carl Ross and I worked on some thirty years ago is concerned with the three-nucleon (3N) force. The simplest and probably the most important mechanism of the 3N force [19] is the two-pion-exchange process among three nucleons.[3] This can be viewed as the Pauli-blocking effect on the two-pion-exchange process between two nucleons (NN). When the NN are imbedded in nuclear matter, part of the two-pion-exchange effect is suppressed due to the Pauli principle. This would mean that the NN interaction in nuclear matter is different from the one in vacuum. It would not be related to the NN scattering data in any simple manner. According to Feynman, however, one can ignore the Pauli principle in intermediate states. One can ignore the Pauli blocking in the NN force in nuclear matter and use the "free" NN force. On the other hand, however, one has to include a 3N force which takes care of the Pauli blocking effect on the NN interaction in nuclear matter. We also worked on the ΛNN force [11, 12, 14]. This was motivated by the "over-binding" of ${}_{\Lambda}\mathrm{He}^5$ [10]. By the way Ref. [10] was the first paper that I wrote with Rajat. We were carried away to the extent that we examined a ΣNN interaction [13] and effects of atomic three-body interactions in liquid ^{4}He [20].

Let me turn to another problem in which Rajat and I were both interested. Although we did not write any paper together, we had many discussions on it. This is concerned with the fractional fermion number (see Ref. [2], Chapter 8). I suspect that this problem has something to do with Feynman's prescription I mentioned above. Consider a system described by the one dimensional Dirac equation, with a given potential of the form of $\beta S(x) + V(x)$, where β is one of the usual Dirac matrices. This is 2×2 in one space dimension. $S(x)$ is a Lorentz scalar and $V(x)$ is the zeroth component of a Lorentz vector. Let us consider the vacuum in the hole theory. The vacuum is such that all negative energy states are filled. It contains an infinite number of fermions. Its fermion number is infinite but its depends on the potential assumed. It turns out that the potential-dependent part of the fermion number of the vacuum is $N = (1/\pi) \int_{-\infty}^{\infty} V(x)dx$, which is independent of $S(x)$. This N can take any fractional value depending on $V(x)$.[4]

[1] R. P. Feynman, Phys. Rev. **76**, 749 (1949)

[2] H. Miyazawa, Prog. Theor. Phys. (Japan) **6**, 801 (1951); S. D. Drell and J. D. Walecka, Phys. Rev. **120**, 1069 (1960); D. Kiang and Y. Nogami, Nuovo Cimento **A 51**, 858 (1967)

[3] B. Loiseau and Y. Nogami, Nucl. Phys. **B 2**, 470 (1967); B. Loiseau, Y. Nogami and C. K. Ross, Nucl. Phys. **A 165**, 601 (1971); *ibid.* **A 176**, 665 (1971)

[4] see, e.g., M. Stone, Phys. Rev. **D 31**, 6112 (1985); Z.-Q. Ma, H. T. Nieh and R.-K. Su, Phys. Rev. **D 32**, 3268 (1985); Y. Nogami and D. J. Beachey, Europhys. Lett. **2**, 661 (1986)

The fractional fermion number that arises as I described above leads to the following conundrum.[5] Consider the relativistic nuclear shell model which has become very popular in recent years. For simplicity let us consider its one-dimensional version. The shell model potential is of the form of $\beta S(x)+V(x)$. The nucleon number and also the nuclear charge of a model nucleus is fractional in general for the reason I described in the preceding paragraph. This is of course a disaster. A similar situation is found in the Skyrmion-bag model of hadrons [2]. The quark number that is contained in the bag is fractional. This is due to a specific boundary condition imposed on the bag surface. The fractional part of the quark number of the bag, however, can be compensated by the fractional quark number that is carried by the Skyrmion field surrounding the bag. In the case of the nuclear shell model, however, we don't have such a Skyrmion field, at least in the usual way as we conceive it. There is nothing that can eliminate the fractional part of the nucleon number. Something is wrong.

When I learned QED as a student I had a question, which still haunts me. In QED we face infinities which we can renormalize away. Apart from the mass renormalization, we have three renormalization constants, usually denoted with Z_1, Z_2 and Z_3. The charge is renormalized as $e = (Z_2 Z_3/Z_1)^{1/2} e_0$ where e_0 is the bare charge. The Ward-Takahashi identity, which is related to the gauge invariance of QED, leads to $Z_1 = Z_2$ and hence $e = {Z_3}^{1/2} e_0$. The Z_3 is a divergent integral, which depends on the cut-off. It can be any fractional number. I believe that this was historically the first example of the fractional fermion number.

What is the mechanism of the charge renormalization? Consider a test charge e_0 placed in a vacuum. This charge causes a change in the charge distribution around it, resulting in a change in the total charge (the test charge plus the charge due to the vacuum polarization). This is strange. Unless part of the polarized charge escapes to infinity, the vacuum polarization would not change the total charge. In the language of the Feynman diagrams the vacuum polarization is described in terms of a loop diagram in which a photon creates a virtual pair of particle and anti-particle (positron) in vacuum. How does this process cause a change in the total charge? The electron-positron pair is neutral after all. I would like to see a version of QED in which the charge renormalization is unnecessary, i.e., $Z_3 = 1$.

The validity of Feynman's prescription seems to have been taken for granted so far. In a recent paper my Brazilian collaborators, Chico Coutinho and Lauro Tomio, and myself tested Feynman's prescription on a one-dimensional bag model, the same model as I discussed above with $S(x)$ taken as an infinite square well potential.[6] For $V(x)$ we assumed $V(x) = \lambda x$. We examined the polarizability of the vacuum, which is essentially the second order energy shift of the vacuum caused by $V(x) = \lambda x$. We compared two formally equivalent methods of calculation, I and II. Method I takes account of the Pauli principle in intermediate states whenever it is applicable. In method II the Pauli principle is completely ignored, i.e., Feynman's prescription is applied.

We showed that, if the energy shifts of all occupied levels of the vacuum (all the negative energy levels) are summed up in method II, the terms violating the Pauli principle formally cancel out. Thus the two methods appear equivalent. This illustrates how

[5] Y. Nogami, in: *Strong Interaction and Hadron Structure*, ed. by S. Sawada (Nagoya Univ., Nagoya, Japan, 1990), p. 234

[6] F. A. B. Coutinho, Y. Nogami and L. Tomio, Phys. Rev. **A 59**, 2624 (1999)

Feynman's prescription (formally) works. We examined this equivalence by calculating the energy shift explicitly. It turned out that the two methods lead to different energy shifts; the equivalence of the two methods does not hold in this example. This means that Feynman's prescription does not always work. I think such a strange situation arises only when the negative energy sea is included. My speculation is, this has something to do with the fractional charge that I discussed above. The fractional fermion number also involves the infinite negative energy sea.

I have talked about a few different topics which I suspect are somehow related at a deep level. I have no conclusion. This talk is only a very preliminary, premature report on one of my hobbies.

I would like to take this opportunity to express my deep appreciation of the constant stimulation that I have received from Rajat in the last thirty-five years. I wish Rajat a very happy retirement that will begin in a few months. Einstein[7] said:

> *Science is a wonderful thing if one does not have to earn a living at it ...*
> *Only when we do not have to be accountable to anyone*
> *can we find joy in scientific endeavor.*

I am sure Rajat will continue to be a source of enlightenment and excitement for all of us for many years to come.

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[7]Letter to a student (1951), quoted by H. Dukas and B. Hoffmann in: *Albert Einstein, The Human Side* (Princeton Univ. Press, 1979), p. 57

8 HOW WE WROTE A PAPER FROM CHICAGO TO DUNDAS

Before I get to the subject matter of the title, let me first touch on a couple of time lines.

The first was, Rajatda and I did not meet at Oxford. I was a green graduate student at Battersea College in London, and I was told by my supervisor that he (Prof. Elton) was spending a year at University of Washington. When he was away, I was to travel up to Oxford and talk to people there, especially Prof. Castillejo. So dutifully, whenever I was stuck I travelled up to Oxford for clarification. It was only after I arrived at McMaster did I learn that Rajatda was in Oxford at that time.

After I arrived at McMaster, I did not start working with Rajatda till after his sojourn back in India at the Tata Institute. Our collaboration began in earnest during M. R. Gunye's visit at McMaster, when we collaborated on working out the p-shell nuclei spectra [15]. This then led us to consider the ${}_{\Lambda}He^5$ hypernuclei, especially the tensor force effect [16]. Since we had the tools in place, Rajatda saw immediately that we should be able to do all the s-shell nuclei [17] with the effective forces from realistic potentials, using either the Kuo-Brown technique or the reference spectrum technique. This we did and, in the process, discovered the error in the second-order correction of the landmark Kuo-Brown paper.

It was then May 1969, and there was an international conference on hypernuclear physics at Argonne, so Gunye, Rajatda, Yuki and I went. We caught the overnight train from Dundas for Chicago. At the conference, the major problem seemed to have been the discrepancy between the lifetimes between the free Λ particle and when it was decaying from the hypernuclei (${}_{\Lambda}H^3$). This discrepancy was cleared up by later experiments. Also discussed was the binding energy of the Λ particle in nuclear matter, and both Rajatda and I picked up on the comment by Arthur Kerman, who suggested that the phase-shift approximation should be good for the Λ-N case. This was very appropriate, as I had just finished playing around with the phase shift inversion problem using a technique of Frank Tabakin.

So on the train coming back we began outlining the paper. By the time the train reached Dundas, we had essentially the final formulae done.

Calculations begun in earnest immediately, as I had a date to keep in England. We had help from Don Sprung and P. Banerjee who had done more detailed nuclear matter calculations, and they kindly let us use their technique to calculate our second order corrections. The calculations must have been finished in quick time, since I was in England getting married on June 7. Rajatda must have completed the writeup and typing of the paper and sent it off while I was away. It was received by Nuclear Physics on June 23 [18]. I think this was the fastest paper we have ever done, and it was accepted with no corrections.

After I moved to Guelph that Summer of 1969, our collaboration unfortunately lapsed, while I moved on to explore atomic physics problems. This was not far from nuclear physics, as the problems dealt with looking at atomic processes during nuclear beta decay, and also exotic atoms like kaonic and Σ atoms, where these were being used to probe the Kaon-nucleon and Σ-nucleon forces.

Luckily it picked up again 21 years later, when I spent a sabbatical year at McMaster in 1990. This also introduced me to other new collaborators. The collaboration has been very fruitful, as can be evinced from the list of papers collected at the end of this volume, and from the other talks. I am glad to say that most of them are here today for this celebration.

Although I have moved on again into the SNO project, our collaboration still continues. In fact only 3 months ago we had another paper accepted.

Let me thank Rajatda for a very fruitful collaboration over the years and all the physics he has taught me. I know that this is not really a retirement from research, just from the university. I hope we have many more years collaborating.

Finally, let me thank Manju, Ronnie, Tukun, and Ronju for treating me as part of the extended family, and the very many superb curry dinners at the Bhaduri's.

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9 WHERE HAS THE MISSING 70^- GONE?

We study the structure of $\Lambda(1405)$ *by means of a coupled-channel potential model and by fitting low-energy* $\overline{K}N$ *data, including the* K^-p *scattering length obtained from the latest x-ray measurements of the kaonic hydrogen atom. From the best fit obtained, we find two possible interpretations of* $\Lambda(1405)$*; either as (1) the* 70^- *three-quark state strongly coupled with* $\overline{K}N$ *and* $\pi\Sigma$*, or (2) a* $\pi\Sigma$ *resonance and/or an unstable* $\overline{K}N$ *bound state. In the latter case, the three-quark state that belongs to the* 70^- *multiplet is located slightly above the* $\overline{K}N$ *threshold and results in a sharp resonance peak in the* K^-p *elastic cross section at laboratory momentum* 170 *MeV/c. To explore their possibilities, measurements of the* $\pi - \Sigma$ *invariant mass distribution and the* K^-p *cross sections with finer resolution will be required.*

The interpretation of $\Lambda(1405)$ – either as an elementary baryon with three-quark structure or a meson-baryon composite – has been controversial for the last few decades. It has been a key issue in theoretical studies of the $\overline{K}N$ system at low energies and particularly in attempts at resolving the so-called kaonic hydrogen puzzle.[1,2,3,4,5,6,7,8]

The puzzle was concerned with an apparent discrepancy between the $1S$ level shift of the kaonic hydrogen atom determined from measurements of the atomic x-rays[9,10,11] and that from the low-energy $\overline{K}N$ scattering data.[1,5,12,13,14] The atomic data indicated a downward shift of the $1S$ level, while the scattering data were extrapolated to the K^-p threshold to predict an upward shift.[15] The puzzle itself, however, has been resolved recently by new elaborate measurements of x-rays from the atom, which revealed an

[1] E. A. Veit, B. K. Jennings, A. W. Thomas, and R. C. Barrett, Phys. Rev. **D 31**, 1033 (1985)

[2] B. K. Jennings, Phys. Lett. **176 B**, 229 (1986)

[3] M. Arima and K. Yazaki, Nucl. Phys. **A 506**, 553 (1990); M. Arima, S. Matsui, and K. Shimizu, Phys. Rev. **C 49**, 2831 (1994)

[4] G. He and R. H. Landau, Phys. Rev. **C 48**, 3047 (1993)

[5] P. B. Siegel and B. Saghai, Phys. Rev. **C 52**, 392 (1995), and earlier references therein

[6] K. S. Kumar and Y. Nogami, Phys. Rev. **D 21**, 1834 (1980)

[7] J. Schnick and R. H. Landau, Phys. Rev. Lett. **58**, 1719 (1987); P. J. Fink, Jr., G. He, R. H. Landau, and J. W. Schnick, Phys. Rev. **C 41**, 2720 (1990)

[8] K. Tanaka and A. Suzuki, Phys. Rev. **C 45**, 2068 (1992)

[9] J. D. Davies *et al.*, Phys. Lett. **83 B**, 55 (1979)

[10] M. Izycki *et al.*, Z. Phys. **A 297**, 11 (1989)

[11] P. M. Bird *et al.*, Nucl. Phys. **A404**, 482 (1983)

[12] Y.-A. Chao, R. W. Kraemer, D. W. Thomas, and B. R. Martin, Nucl. Phys. **B 56**, 46 (1973)

[13] A. D. Martin, Phys. Lett. **65 B**, 346 (1976); Nucl. Phys. **B 179**, 33 (1981)

[14] R. H. Dalitz, J. McGinley, C. Belyea, and S. Anthony, in Proc. Int. Conf. on *Hypernuclear and Kaon Physics*, Heidelberg 1982, ed. by B. Povh (Max Planck Institut für Kernphysik, Heidelberg, 1982), p. 201

[15] In Ref.[8] within a coupled-channel potential scheme, a good overall fit to all the low-energy $\overline{K}N$ data that was consistent with the atomic data of Refs.[9,10,11] was obtained. However, the resulting K^-p scattering amplitude was quite different at low energies from the one determined earlier from the Coulomb-nuclear interference.

upward shift of the $1S$ level:[16]

$$\epsilon + i\,\frac{\Gamma}{2} = -323 \pm 63 \pm 11 + \frac{i}{2}\,(407 \pm 208 \pm 100)\ \mathrm{eV}\,, \tag{1}$$

where the first and second errors correspond to the statistical and systematic errors, respectively. Through Deser's formula,[17] the above shift and width of the $1S$ level can be converted to the K^-p scattering length as

$$A_{K^-p} = (-\,0.78 \pm 0.18) + i\,(0.49 \pm 0.37)\ \mathrm{fm}\,, \tag{2}$$

where the errors have been estimated simply by adding those arising from the statistical and systematic errors in Eq. (1). This is compatible with the K^-p scattering length extracted from the scattering data.

The reason why the structure of $\Lambda(1405)$, observed as a resonance in the $\pi\Sigma$ invariant mass distribution, was a crucial point in explaining the puzzle is that $\Lambda(1405)$ lies just 25 MeV below the $\overline{K}N$ threshold and has a strong influence on the low-energy $\overline{K}N$ data. The negative $\mathrm{Re}\,A_0$, that arises from the negative $\mathrm{Re}\,A_{K^-p}$, can be interpreted as due to the existence of an isosinglet bound state of $\overline{K}$ and N, and it may be understood by the picture that $\Lambda(1405)$ is (mostly) a $\overline{K}N$ and/or $\pi\Sigma$ composite.[1,5,8,12,13,14] However, this does not necessarily rule out the possibility that $\Lambda(1405)$ has an elementary-baryon component. The SU(3) quark model indeed predicts a three-quark state that has the same quantum numbers as $\Lambda(1405)$, as a member of the 70^- multiplet with two partners, $\Lambda(1670)$ and $\Lambda(1800)$, of $J^\pi = 1/2^-$. In Ref.[3] it has been proposed that $\Lambda(1405)$ is dominantly this 70^- state and that its strong coupling with $\overline{K}N$ and $\pi\Sigma$ makes its mass much smaller than the other two members.

In this work, we address the question as to whether $\Lambda(1405)$ can be interpreted as the 70^- three-quark state, and if not, where the mass of the "missing" 70^- state can be. We study the coupled system of $\overline{K}N$ and $\pi\Sigma$ near the $\overline{K}N$ threshold by means of a coupled-channel potential model. To capture the essential features of the system in the energy range considered, we mostly focus on the isosinglet states. We introduce an elementary particle with $J^\pi = 1/2^-$ which represents the 70^- three-quark state (we call it Λ_0) and assume that its bare mass lies within the low-energy region for $\overline{K}N$. We adopt a separable potential to describe the meson-baryon interaction and a Yukawa-type form for the Λ_0-meson-baryon coupling. For the isotriplet states, we simplify the problem by explicitly using the $\overline{K}N$ channel only and including the $\pi\Sigma$ and $\pi\Lambda$ channels by means of complex coupling constants. By fitting the low-energy $\overline{K}N$ data including the K^-p scattering length of Eq. (2), and solving the eigenvalue problem for the isosinglet states, we examine the probabilities of the three-quark and meson-baryon components in $\Lambda(1405)$.

Two sets of parameters which best fit the low-energy $\overline{K}N$ scattering data, the K^-p scattering length determined from the latest measurements of atomic x-rays, and the $\pi\Sigma$ mass distribution have been found with the bare mass of the three-quark state lying within the low-energy region around the $\overline{K}N$ threshold. The first set allows us to interpret

[16]M. Iwasaki *et al.*, Phys. Rev. Lett. **78**, 3067 (1997); T. M. Ito *et al.*, Phys. Rev. **C 58**, 2366 (1998)

[17]S. Deser, M. L. Goldberger, K. Bauman, and W. Thirring, Phys. Rev. **96**, 774 (1954); T. H. Trueman, Nucl. Phys. **26**, 57 (1961)

$\Lambda(1405)$ as the three-quark state strongly coupled with the $\pi\Sigma$ and $\overline{K}N$ channels, as proposed in Ref.[3] The second set, on the contrary, reproduces $\Lambda(1405)$ as a $\pi\Sigma$ resonance and/or an unstable $\overline{K}N$ bound state. For this set of parameters, the three-quark state manifests itself as a narrow resonance in the scattering region of $\overline{K}N$; as a sharp peak in the K^-p elastic cross section around the data point at $k = 170$ MeV/c.

We have thus two possibilities, if the bare mass of the 70^- three-quark state lies in the low-energy region for $\overline{K}N$: the three-quark state gives rise to either $\Lambda(1405)$ or a sharp resonance in the $\overline{K}N$ scattering states – in the latter case $\Lambda(1405)$ is a meson-baryon composite. To explore these possibilities, measurements of the $\pi\Sigma$ mass distribution and the K^-p cross sections with finer resolution are required.

This work was done in collaboration with Kaori Tanaka,[18] Masahiro Kimura,[19] Takahiko Miyakawa,[20] Miho Takayama,[21] and Atsushi Hosaka.[22]

We would like to thank Yuki Nogami for many helpful comments and discussions.

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10 THE LOW-ENERGY NUCLEAR DENSITY OF STATES AND THE SADDLE-POINT APPROXIMATION

Long long ago, far far away, there was a thesis supervisor who loved to put statistical mechanical methods to unusual uses. One problem he suggested to a hapless graduate student was to calculate the effect of shell corrections on the low-energy nuclear density of states. Various approaches like putting oscillating terms in the single particle density were tried before the problem was abandoned as hopeless and the graduate student moved on to another crazy idea — using statistical methods to derive an Extended Thomas-Fermi model and using these methods to derive shell corrections for realistic potentials [23, 24, 25].

In this contribution I will return to the problem of the low-energy density of states for nuclear systems. After the 27 year break, it appears that progress can indeed be made.

The studies of nuclear level densities date back to the 1950s with work by Rozenweig,[1] and by Gilbert and Cameron.[2] The usual technique is to calculate the partition function and then invert the Laplace transform using the saddle-point approximation. At energies sufficiently high for shell and pairing effects to be washed out, the density of states is given in terms of the single-particle density of states (and its derivatives) at the Fermi surface, the shell-correction energy, and the pairing energy. At lower energies the results are more problematic, and typically crude extrapolations from the higher energy are used. At low energy the saddle-point approximation itself breaks down.[3]

Here I explore the lower energy region using a single-particle model. In contrast to higher energies, where the density of states depends on the shell correction and the smooth single particle density of states, in the lower-energy regime the density of states depends on the separation of single-particle levels and their degeneracy. Thus the dependences in the two regimes are rather different.

The grand canonical partition function for two types of particles can be written as

$$e^{\Omega} = \sum_{N',Z',E'} \exp(\alpha_N N' + \alpha_Z Z' - \beta E')\,, \tag{1}$$

where the sum is over all nuclei with N' neutrons, Z' protons and over all energy eigenstates E' . The sum over eigenstates can be substituted by an integral:

$$e^{\Omega} = \sum_{N',Z'} \int dE' \rho(E', N', Z') \exp(\alpha_N N' + \alpha_Z Z' - \beta E')\,, \tag{2}$$

where $\rho(E', N', Z')$ is the nuclear density of states. The inversion integral is

$$\rho(E, N, Z) = \frac{1}{(2\pi i)^3} \oint d\alpha_N \oint d\alpha_Z \oint d\beta\, e^S, \tag{3}$$

[1] N. Rozenweig, Phys. Rev. **105**, 950 (1957); *ibid.* **108**, 817 (1957)

[2] A. G. Cameron, Can. J. Phys. **36**, 1040 (1958); A. Gilbert and A. G. Cameron, *ibid.* **43**, 1446 (1965)

[3] M. K. Grossjean and H. Feldmeier, Nucl. Phys. **A 444**, 113 (1985)

where $S = \Omega - \alpha_N N - \alpha_Z Z + \beta E$. This integral is usually done by the saddle-point approximation (one of Prof. Bhaduri's favorite approximations). In the saddle-point approximation, the nuclear density of states is written as

$$\rho = \frac{e^S}{(2\pi)^{3/2} D^{1/2}}, \tag{4}$$

where D is the determinant of the second derivatives of S with respect to the parameters α_N, α_Z, and β. The determinant can be simplified to a product of factors by changing which variables are held fixed when the derivatives are performed.

The determinant is written as

$$D = \begin{vmatrix} \frac{d^2S}{d\beta^2} & \frac{d^2S}{d\beta d\alpha_N} & \frac{d^2S}{d\beta d\alpha_Z} \\ \frac{d^2S}{d\beta d\alpha_N} & \frac{d^2S}{d\alpha_N^2} & \frac{d^2S}{d\alpha_N d\alpha_Z} \\ \frac{d^2S}{d\beta d\alpha_Z} & \frac{d^2S}{d\alpha_N d\alpha_Z} & \frac{d^2S}{d\alpha_Z^2} \end{vmatrix}. \tag{5}$$

To simplify this, I change the independent variables to $\tau = 1/\beta$, $\mu_N = \tau\alpha_N$, and $\mu_N = \tau\alpha_Z$ and change the dependent variable to $\Omega' = \tau\Omega = \tau S + \mu_N N + \mu_Z Z - E$. In terms of the new variables, the equations determining the saddle point are

$$\frac{d\Omega'}{d\tau} = -S, \qquad \frac{\mathrm{d}\Omega'}{\mathrm{d}\mu_{\mathrm{N}}} = -\mathrm{N}, \qquad \frac{\mathrm{d}\Omega'}{\mathrm{d}\mu_{\mathrm{Z}}} = -\mathrm{Z}. \tag{6}$$

Using these equations the determinant can be written as

$$D = -\tau^5 \begin{vmatrix} \frac{dS}{d\tau} & \frac{dN}{d\tau} & \frac{dZ}{d\tau} \\ \frac{dS}{d\mu_n} & \frac{dN}{d\mu_N} & \frac{dZ}{d\mu_N} \\ \frac{dS}{d\mu_Z} & \frac{dN}{d\mu_Z} & \frac{dZ}{d\mu_Z} \end{vmatrix}. \tag{7}$$

In deriving this result I have used that in a determinant a multiple of a row (column) can be added to another row (column). In the first row the derivatives are at constant μ_N and μ_Z ; in the second row at constant τ and μ_Z, and in the third at constant τ and μ_N. I now change the variables held constant, using the equations

$$\left.\frac{dS}{d\tau}\right|_{\mu_N\mu_Z} = \left.\frac{dS}{d\tau}\right|_{NZ} + \left.\frac{dS}{dN}\right|_{\tau Z}\left.\frac{dN}{d\tau}\right|_{\mu_N\mu_Z} + \left.\frac{dS}{dZ}\right|_{\tau N}\left.\frac{dZ}{d\tau}\right|_{\mu_N\mu_Z} \tag{8}$$

and

$$\left.\frac{dS}{d\mu_N}\right|_{\tau\mu_Z} = \left.\frac{dS}{dN}\right|_{\tau Z}\left.\frac{dN}{d\mu_N}\right|_{\tau\mu_Z} + \left.\frac{dS}{dZ}\right|_{\tau N}\left.\frac{dZ}{d\mu_N}\right|_{\tau\mu_Z}, \tag{9}$$

$$\left.\frac{dS}{d\mu_Z}\right|_{\tau\mu_N} = \left.\frac{dS}{dN}\right|_{\tau Z}\left.\frac{dN}{d\mu_Z}\right|_{\tau\mu_N} + \left.\frac{dS}{dZ}\right|_{\tau N}\left.\frac{dZ}{d\mu_Z}\right|_{\tau\mu_N}. \tag{10}$$

Subtracting $\left.\frac{dS}{dN}\right|_{\tau Z}$ times the second column and $\left.\frac{dS}{dZ}\right|_{\tau N}$ times the third column from the first column, I have

$$D = -\tau^5 \left.\frac{dS}{d\tau}\right|_{NZ} \begin{vmatrix} \frac{dN}{d\mu_N} & \frac{dN}{d\mu_Z} \\ \frac{dZ}{d\mu_N} & \frac{dZ}{d\mu_Z} \end{vmatrix}. \tag{11}$$

This procedure can be repeated to yield

$$D = -\tau^5 \left.\frac{dS}{d\tau}\right|_{NZ} \left.\frac{dN}{d\mu_N}\right|_{\tau Z} \left.\frac{dZ}{d\mu_Z}\right|_{\tau\mu_N} . \tag{12}$$

Note the progression on the variables that are held constant. This procedure can be extended in an iterative manner to any number of constants of motion. As if by magic, a determinant has been turned into a product.

Does this new form help us in practice? Indeed yes! To calculate the density of states as a function of energy for fixed particle number I need the entropy S as a function of energy at fixed N and Z for the exponent in the numerator. The temperature can then be obtained from $dS/dE|_{NZ} = 1/\tau$ and $dS/d\tau|_{NZ} = -1/(\tau^3 d^2S/dE^2)$. This leaves the derivatives of the particle numbers to be separately evaluated. Thus I have three independent functions to parameterize.

To see that this modified form of the density of states agrees with the standard form, I consider the independent-particle model with a constant single-particle density of states g. The entropy then is $S = 2\sqrt{aE}$ where $a = \pi g/6$. The temperature is $\tau = \sqrt{E/a}$, $dS/d\tau|_{NZ} = a/2$, and $dN/d\mu_N = dN/d\mu_Z = g$. This then gives the well-known formula

$$\rho = \frac{\sqrt{\pi}}{12} \frac{\exp[2\sqrt{aE}]}{E^{5/4}a^{1/4}} . \tag{13}$$

Now let us consider a normal quantum system with a discrete spectrum. In this case there are in general no closed forms for the various functions, so I consider the zero-temperature limit of $dN/d\mu_N$. I start with an open-shell situation where there is a partially filled shell. For this discussion it is only necessary to consider the properties of the partially filled level. I take level to have a degeneracy of g_1, an energy of ϵ_1 and a partial occupancy of d. As τ goes to zero the saddle-point condition for the number of particles becomes $g_1/(1 + \exp[(\epsilon_1 - \mu)/\tau]) = dg_1$. Solving for μ, I have $\mu = \epsilon_1 + \tau \ln[d/(1 - d)]$. Note that this formulae breaks down for d equal to zero or one, corresponding to closed shells. The derivative $dN/d\mu$ is given as $g_1 d(1 - d)/\tau$. Note that it diverges as τ goes to zero.

For a closed shell it necessary to consider two levels, the last filled level and the first unfilled level. I denote the energies and degeneracies of these levels as ϵ_1, g_1 and ϵ_2, g_2. The saddle-point condition is now $g_2/(1+\exp[(\epsilon_1 - \mu)/\tau]) + g_2/(1+\exp[(\epsilon_2 - \mu)/\tau]) = g_1$. Solving for μ, I have $\mu = (\epsilon_1 + \epsilon_2)/2 - \tau \ln[g_1/g_2]$ for small temperatures. The derivative $dN/d\mu$ is given as $2\sqrt{g_1 g_2}\exp[(\epsilon_1 - \epsilon_2)/(2\tau)]/\tau$. This goes to zero exponentially fast as τ goes to zero. Note that in neither case is the shell correction involved.

To be useful we need an expression for S as a function of the energy. Here I again use a few tricks. It turns out to be easier to parameterize E as a function of τ. Since $\tau dS = dE$, I have $S = \int_0^\tau \frac{1}{\tau'}\frac{dE}{d\tau'}d\tau' + S(\tau = 0)$. The last term is the integration constant and is given once the degeneracy of the ground state is known. It contributes to the exponent but not to the denominator were a derivative is taken.

Next I need E as a function of τ. For many systems there are quite reliable approximations. For very low temperatures much smaller than the level spacing, the energy does not change significantly. However above some critical temperature, τ_0, it starts to increase rapidly. For temperatures near this region the energy can be parameterized quite simply by $E - E_0 = c(\tau - \tau_0)^2\theta(\tau - \tau_0)$. I have checked this approximation using a simple shell model and found that it works quite well except if there are several levels approximately equally distant from the Fermi surface. The parameters τ_0 and c depend on the level spacing and degeneracy near the Fermi surface. Again they do not depend on the shell correction.

Before being useful at very low energies, a short-coming of the saddle point approximation must be overcome. It is well known that at low energies the saddle-point approximation tends to diverge as the denominator goes to zero. In many cases this problem can be fixed by using a technique from Ref.[3] which handles the contribution to the nuclear density of states from the ground-state delta function explicitly. Work in combining this idea with the present approach is in progress.

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11 25 YEARS OF SEMICLASSICAL PHYSICS

Prelude

From 1973 - 1975 I was working as a young post-doc at the Niels Bohr Institute (NBI) in Copenhagen. I had been involved with Strutinsky's shell-correction method and studied his numerical energy-averaging procedure in some detail, and was aware of its close relation to the semiclassical extended Thomas-Fermi (ETF) model. I had read the paper by Bhaduri and Ross [21] on the equivalence of the two methods, and a paper of Jennings[1] which had a close overlap with my Ph.D. thesis.[2] Upon returning from a Summer School in Romania to the NBI in October of 1974, I noticed a pair of newly-arrived guests: an Indian-looking senior scientist with friendly eyes, accompanied by a young famulus with red sidewhiskers. That is to say, before I saw them I heard that dry cough which I soon learned to recognize amongst hundreds. (I think it has become more seldom after Rajatda stopped smoking pipes.) Before long, the two revealed themselves as Rajat K. Bhaduri and his Ph.D. student Byron K. Jennings.

Extended Thomas-Fermi model

Quite immediately, we got involved in discussions about ETF versus Strutinsky averaging, and within few days I was engaged in heavy algebra. We wanted to check if the agreement of the two methods improved for realistic Woods-Saxon potentials when higher orders in $\hbar$ were included in the ETF model and, in particular, if it persisted when a spin-orbit interaction was added to the potential [24]. There was no MAPLE and no MATHEMATICA at that time: we had to go through the Wigner-Kirkwood expansion on our bare feet, using hundreds of sheets of scratch paper. This work on the ETF model was very inspiring and in the process, I earned my first PRL [25].

We soon became friends and continued our discussions in the kitchen, with Rajatda cooking chicken curry and Byron chopping the onions, as it became an established tradition amongst the Bhaduri students. (I don't remember if I myself was of any use in the cooking process – but I have later gladly adapted the onion chopping tradition with my students.) With Byron we applied the ETF model to rotating nuclei[3] which were becoming fashionable during those days at the NBI. Later, when Byron and I both were at Stony Brook, we endeavoured on the use of the ETF model to develop the kinetic energy density functional $\tau[\rho]$ up to fourth-order gradients. We got the right result,[4] although another Canadian had beaten us to it.[5] I became fascinated by the semiclassical density variational method using the ETF model and consecrated quite some effort on the self-consistent determination of average nuclear properties.[6] Later I applied the self-consistent ETF density variational method also to metal clusters.[7]

[1] B. K. Jennings, Nucl. Phys. **A 207**, 538 (1973)

[2] M. Brack and H.-C. Pauli, Nucl. Phys. **A 207**, 401 (1973)

[3] M. Brack and B. K. Jennings, Nucl. Phys. **A 258**, 264 (1976)

[4] M. Brack, B. K. Jennings, Y. H. Chu, Phys. Lett. **65 B**, 1 (1976)

[5] C. H. Hodges, Can. J. Phys. **51**, 1428 (1973)

[6] M. Brack, C. Guet, H.-B. Håkansson, Phys. Reports **123**, 275 (1985) and references therein

[7] M. Brack, Phys. Rev. **B 39**, 3533 (1989); see also M. Brack, Rev. Mod. Phys. **65**, 677 (1993)

In 1977 - 1978 I spent some time at the Institut Laue-Langevin (ILL) at Grenoble. Together with Peter Schuck and Mireille Durand, we attempted to overcome the notorious turning-point divergences of the ETF model. Once again it was Rajatda who triggered our better understanding by his paper on the partial resummation of the Wigner-Kirkwood series [26]. Extending his idea, we calculated semiclassical densities directly from realistic potentials,[8] and Jonny Bartel later expanded the method to calculate average nuclear properties in his Ph.D. thesis.[9] During Rajatda's visit to the ILL, we not only went hiking on the glaciers of the French Alps, but we also applied the resummation technique to the Coulomb problem and the ground-state energies and densities of atoms [28, 29]. We wrote up our papers when I visited McMaster the following fall for a longer period.

Interlude

During my first years at Regensburg University from 1978 on, I was busy preparing my lectures and worked on the semiclassical energy density method. In the fall of 1985 I went to a NATO summer school in Portugal, in order to present our semiclassical results for nuclei. There I met Rajatda and Byron again after a break of several years. They were very excited about something new: the description of the baryon excitation spectrum in a non-relativistic deformed quark model. (It was then that I heard for the first time Murthy's name, who in the mean time had joined the Mac community.) As before, I could not resist their attempts to involve me and joined the enterprise. Since I'm focusing here on semiclassics, let me pass quickly over this part of our collaboration, in which Murthy took the main lead [34], and also over two other adventures with Rajatda that led into the D section of the Physical Review [30, 40]. I will also leave the story of the three-anyon spectrum [44, 50], in which I was more of a spectator than a collaborator, to Murthy and Jimmy.

Periodic orbit theory

But it was in the context of discussing the question of integrability or possible chaoticity of the three-anyon system, that my next involvement with semiclassics occurred – and ended up taking serious hold of me, although it happened in small steps. Together with Rajatda, Murthy and Jimmy at McMaster, we wanted to educate ourselves on the level statistics of a chaotic system and, just for fun, picked up the famous textbook potential of Hénon and Heiles. As the old Strutinsky practitioner that I was, I ran the spectrum through my energy-averaging routine and extracted the oscillating part of its level density. And soon we were looking at a beautiful beating pattern that reminded us of the likewise famous textbook example of the beating level density of a spherical cavity, investigated long ago by Balian and Bloch.[10] I had learned about the physical realization of these beats, the so-called supershells, in the abundance spectra of metal clusters,[11] and also about their interpretation in terms of the semiclassical periodic orbit theory (POT). So now we had to look for periodic orbits of a classical particle in the Hénon-Heiles potential. The linear orbits A were easy to guess, but the rest needed numerical work. Jimmy the wizard immediately came up with a solver for the classical equations of motion, and soon

[8] M. Durand, M. Brack, P. Schuck, Z. Phys. **A 286**, 381 (1978)

[9] J. Bartel, M. Durand, M. Brack, Z. Phys. **A 315**, 341 (1984)

[10] R. Balian and C. Bloch, Ann. Phys. (N.Y.) **69**, 76 (1972)

[11] H. Nishioka, K. Hansen and B. R. Mottelson, Phys. Rev. **B 42**, 9377 (1990)

we had found the loop orbit C; the "Smiley" orbits B took a little more searching. Next we had to get wise on Gutzwiller's trace formula,[12] since these are isolated orbits. That part was much harder – so hard, indeed, that we at first cheated our way around a correct calculation of the amplitudes and Maslov indices. We fabricated a semiclassical level density, guessing (wrongly) not only the Maslov indices but also the low-energy limit of the amplitudes which we knew to go over into the linear ones of the analytical harmonic oscillator. Still, we got a reasonable agreement with the smoothed quantum level density.

After presenting our preliminary results in seminar talks to POT specialists (Rajatda in Orsay and myself in the "CATS" group at the NBI), we realized that we had to do a more serious job. During our next common sabbatical, part of which I spent with Rajatda in India, we joined Murthy in Madras and computed the monodromy matrix in order to calculate correctly the Gutzwiller amplitudes of the three orbits. Although we still cheated on the Maslov indices (which I never have learned to understand completely), we got a PRL accepted [52]. Today I am ashamed of this paper, because the good result was helped by a cancellation of several errors. Let this be a *memento* to all those (not only young) scientists whose ambition it is to publish as many PRLs as possible: a Physical Review Letter is no proof of solid work!

The following spring, we reunited at Mac, including Jimmy, and this time we learned one practicable way to compute the Maslov indices. (Thanks to Stephen Creagh, who kindly underwent a week-long on-line squeezing over e-mail.) We also increased our fun by including a magnetic flux-line into the potential, although this made things really tough. (It was during one of those hot working sessions that I was hit on the head – but that story belongs to the dinner party.) Even in our "Chaos" paper [61], the overall amplitude of the semiclassical level density and the counting of orbits was not understood – only in an Erratum that question came to a happy end. (The problem of the low-energy oscillator limit where the Gutzwiller amplitudes diverge has, actually, only recently been solved after I learned about uniform approximations.[13]) I will come to the role of the orbit bifurcations, which we so far had ignored completely, at the end of this contribution.

I definitely felt the need to learn more about POT. In order to force myself to it, I announced special lectures on this topic in Regensburg during the summer of 1993. Inspired by Rajatda, who presented a series of lectures in Tokyo and had them nicely put into TeX, I did the same and let my lecture notes grow when I repeated and extended my lectures in 1994. Some time that year, I was approached by World Scientific, who invited me to write a book on mean-field theory. But I had just given painful birth to a longer review article and did not have the courage for further extended writing – at least not alone. On the other hand, my vanity did not allow me to forget that offer ... And I had those lecture notes lying around. When I met Rajatda next time, I asked him if he might be interested in writing a book – not on mean-field theory but on the subject that we were passionately involved with at the time, the POT. And, for that matter, on our old hobby the ETF model. He was all for it. In March of 1995 we sketched the first contents of "Semiclassical Physics", and Rajatda got the publishers of his previous two books [1, 2], Addison and Wesley, interested in the project. This was the beginning of our largest collaboration. A collaboration that has deepened our friendship and brought us lots of fun, fruitful fights, insight into fascinating parts of mathematical physics, more mutual

[12] M. C. Gutzwiller, J. Math. Phys. **12**, 343 (1971)

[13] M. Brack, P. Meier, K. Tanaka, J. Phys. **A 32**, 331 (1999)

contacts with our families, and more Indian (and Swiss) meals. The book was finished in December of 1996 and appeared soon thereafter [3] (alas, with lots of misprints!). Apart from a brief study of the relation of the Weyl expansion to the POT series [68], it has remained our latest collaboration – but I am confident that there is more to come. So far I have not been able, though, to hook on to Rajatda, Murthy and MK's ideas on fractional statistics and on Bose condensates.

Actually I am still having a lot of fun with semiclassics. Just in order to intoxicate you, let me briefly discuss two topics that have kept me busy for some time – and which I am far from having understood. I am sure that if Rajatda could get infected with these problems, I would sooner come to an end with them.

Why is Thomas-Fermi so good?

The first problem arose last year at McMaster, when we were discussing the fractional statistics in a two-dimensional system (see Ref. [69]). The justification for absorbing a δ-function interaction into fractional occupation numbers goes over the two-dimensional kinetic energy density functional $\tau[\rho]$ which in TF theory is proportional to ρ^2:

$$\tau_{TF}[\rho] = \frac{\hbar^2}{2m}\pi\rho^2, \quad \text{where} \quad \tau(\mathbf{r}) = -\frac{\hbar^2}{2m}\sum_{i\,occ}\phi_i^*(\mathbf{r})\,\nabla^2\phi_i(\mathbf{r})\,, \quad \rho(\mathbf{r}) = \sum_{i\,occ}|\phi(\mathbf{r})|^2\,, \tag{1}$$

like the potential energy density of a δ-function interaction. Nobody knows the exact functional $\tau[\rho]$, and there is no *a priori* reason to believe that the TF functional should hold on a quantum-mechanical level. So why did it work nevertheless? For the mere fun of it, I computed the exact quantum-mechanical densities of the circular billiard (which are just sums of squared Bessel functions), in order to check the TF functional (1). It turned out to perform very well, except near the boundary, *including the shell effects* (Figure 1, left). This was a total surprise because, after all, I had been preaching since years that the (E)TF functionals for $\tau[\rho]$ should only hold for *averaged* densities, without quantum shell effects (cf. Ref. [3]). The same kind of agreement is also found in a three-dimensional

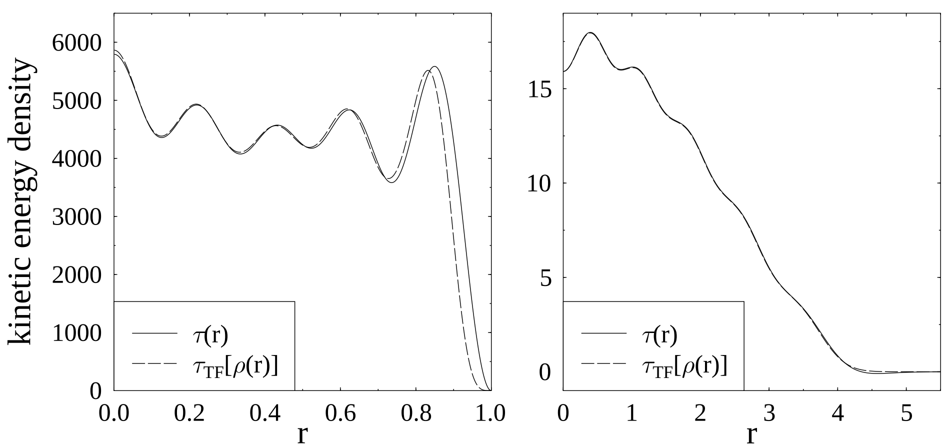

Figure 1: *Solid lines:* exact quantum-mechanical kinetic energy density $\tau(r)$. *Dashed lines:* TF functional $\tau_{TF}[\rho(r)]$ (1) using the exact quantum-mechanical spatial density $\rho(r)$. *Left:* for 102 particles in a circular billiard with unit radius. *Right:* for 110 particles (10 filled shells) in a two-dimensional harmonic oscillator potential.

spherical billiard. Maybe, one might ponder, is this due to the constant potential inside? So I next looked at isotropic harmonic oscillators. The result is even better there (Figure 1, right); it becomes still better with increasing dimension. The particular thing about two dimensions is that the functional (1) is quadratic in ρ. If we insert the exact density $\rho(r)$ of a two-dimensional harmonic oscillator with $M+1$ filled shells into it and integrate $\tau_{TF}[\rho]$ analytically, we obtain the *exact quantum-mechanical* total kinetic energy:

$$\int \tau_{TF}[\rho(r)]\, d^2r = \hbar\omega \sum_{\mu=0}^{M} (\mu+1)^2 = \frac{1}{2} \sum_{i\,occ} \epsilon_i \; = E_{kin}(M)\,. \tag{2}$$

This holds exactly only in two dimensions. But even there it ought to be forbidden. Is the TF functional perhaps exact in this case? In the variational sense, it is *not*, because the exact density $\rho(r)$ is not a solution of the TF variational equation. On the other hand, we know that the ETF model for $\tau[\rho]$ gives a zero Weizsäcker coefficient in two dimensions [3], and there is evidence that there are no higher-order gradient corrections at all in two dimensions.[a] But that the integral (2) gives the exact kinetic energy is perhaps a partial explanation why fractional statistics work for a two-dimensional δ-function interaction. Perhaps our result is relevant in the context of what mathematicians call the "Lieb-Thirring inequalities".[b] As we are not sure whether physicists or mathematicians are least interested in these results, we have not published them yet.

Zooming into chaos

The other subject that fascinates me are the bifurcations of the A orbit in the Hénon-Heiles potential. This is the linear orbit along a symmetry axis, along which the particle pendulates in the direction of the barrier (located at energy $e = 1$). With increasing energy, it undergoes an infinite number of bifurcations; in the limit $e \to 1$ its period becomes infinity. A measure of stability is given by the trace of the stability matrix M. Bifurcations occur whenever trM = 2. In Figure 2 we show trM versus energy e for the orbit A (with its Maslov index increasing by

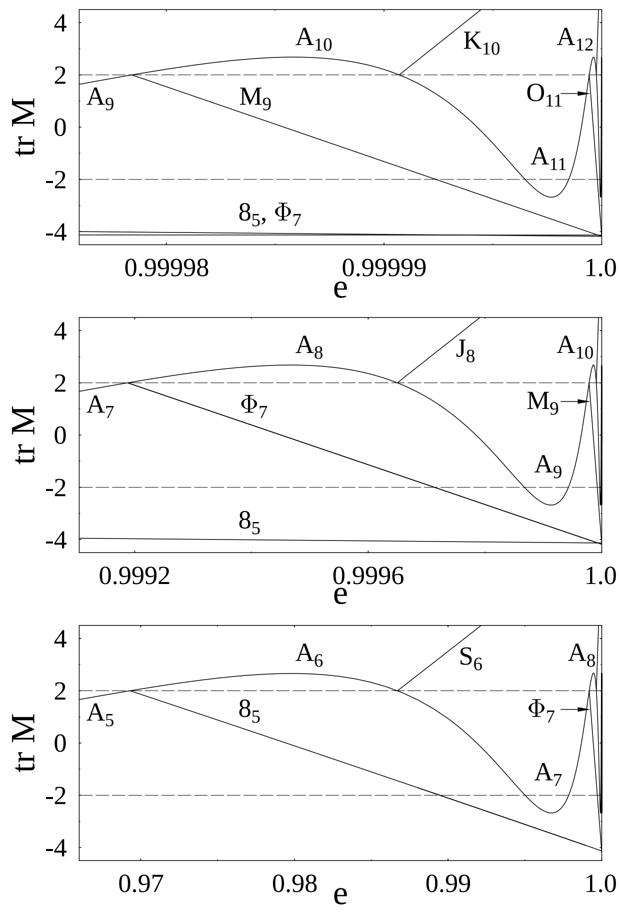

Figure 2: Trace of stability matrix M of orbit A and the orbits born at successive pitchfork bifurcations in the Hénon-Heiles potential, versus scaled energy e. Subscripts are Maslov indices. *From bottom to top:* successively zoomed energy scale near $e = 1$.

[a] M. Brack and B. van Zyl, to be published

[b] see, e.g., T. Weidl, Comm. Math. Phys. **178**, 135 (1996)

one unit at each bifurcation) and for the orbits born at these bifurcations. In the lowest panel, we see the uppermost 3% of the energy scale available for the orbit A. The first bifurcation occurs at $e \simeq 0.969$, where A_5 becomes unstable (with trM > 2) and a new stable orbit, which I call 8_5 (see its squeezed "figure 8" shape in Fig. 3), is born. At $e \simeq 0.987$, orbit A_6 becomes stable again and a new unstable orbit S_6 is born. In the middle panel, we have zoomed the uppermost 3% of the latter energy scale. Here the behaviour of A repeats itself, with the new orbits Φ_7 and J_8 born at the next two bifurcations. Zooming with the same factor to the top panel, we see the birth of M_9 and K_{10}. This can be repeated *ad infinitum*: each new figure will be a replica of the previous one, with all the Maslov indices increased by two units and with trM of orbit A oscillating forever. Note that we have only shown here the primitives (i.e., the first repetitions) of each orbit. The higher repetitions of A will also undergo regular bifurcations and exhibit a corresponding fractal behaviour. This infinite proliferation of stable and unstable orbits creates an increasingly mixed phase space and paves the "Feigenbaum route" to chaos.[14]

Let us have a look at the shapes of the orbits born at these bifurcations. In Figure 3 they are shown from left to right with increasing Maslov indices, all evaluated at the barrier energy $e = 1$. Note that these are all stable or unstable periodic oscillations around the original linear orbit A, which is oriented here along the y axis. The closer they are born to $e = 1$, the smaller is their amplitude in the x direction when they have reached the barrier. Therefore, in the upper part of the figure, the x axis has been zoomed by a factor 0.163 from each panel to the next. The orbits look practically identical in the lower

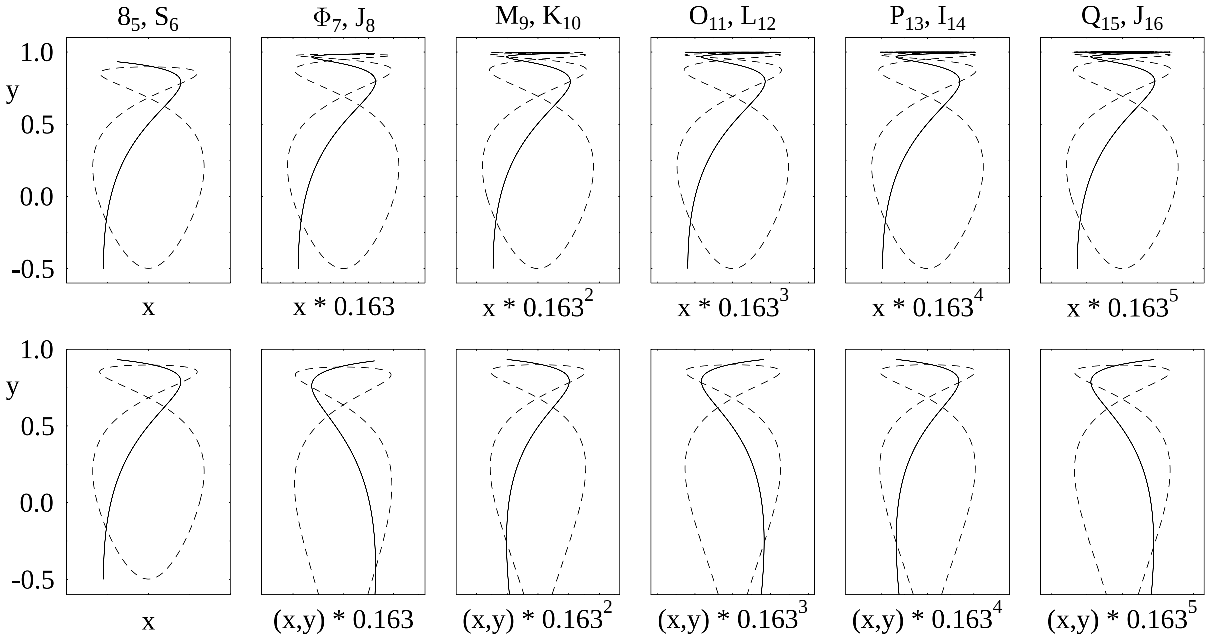

Figure 3: Orbits bifurcated from the A orbit in the Hénon-Heiles potential, shown with increasing Maslov indices from left to right, all evaluated at the barrier energy $e = 1$. *Dashed lines:* stable orbits (odd indices), *solid lines:* unstable orbits (even indices). *Top panels:* successive scaling of x axis only; *bottom panels:* successive scaling of both axes with the same factors; along the y axis only the top part (starting from $y = 1$) is shown.

[14] M. J. Feigenbaum, Physica **7 D**, 16 (1983); see also J. M. Greene *et al.*, Physica **3 D**, 486 (1981)

97% of the y scale; only near the barrier, which lies at $y = 1$, do they oscillate more and more. In the lower part of the figure, we have zoomed also the y axis by the same factor from one panel to the next, and plotted only the uppermost part of each orbit (starting from $y = 1$). In these blown-up scales, the tips of the orbits exhibit a perfect self-similarity that goes along with the fractal behaviour of their bifurcations.

So far these are, of course, just results of classical non-linear dynamics. To use them for semiclassics, we have to include the contributions of all these orbits into the trace formula which, however, has to be improved. The standard formula of Gutzwiller does not hold, as its amplitudes diverge at the bifurcations. But since these bifurcations lie increasingly closer with increasing energy, the uniform approximations for isolated bifurcations[15] also cannot be used. There is some hope for handling the present situation with a method that was developed by our Ukrainian friends under the leadership of Sasha Magner (another semiclassics addict). For the elliptic billiard they were able to incorporate an infinity of bifurcations of the short diameter orbit and all its repetitions in an improved trace formula.[16] We are presently trying to apply their technique to the A orbit in the Hénon-Heiles potential and similar other potentials,[17] which leads us to fascinating mathematical physics. The $y(t)$ motion of the A orbit is given by Jacobian elliptic functions. The motion of a small perturbation $\delta x(t)$ in the perpendicular direction is given by the harmonic-oscillator equation with a periodically time-dependent frequency, which is a special case of a type of differential equations studied over 150 years ago by Lamé.

So there is still a lot of hard and interesting work to do – and for sure also a lot more fun to be had. And all this only because on a spring day in 1992 at McMaster, Rajatda came to think of the Hénon-Heiles potential ...

Postlude

To end my story, let me thank Rajatda for all the fascinating physics that he has lead me to discover over 25 years. For the curiosity and the drive by which he attacks new problems and opens up new lines of thought. For the fun we had in our discussions, but also for the fights – which at times even turned out to be indicators that I was not totally wrong. For sharing his enormously broad overview of many different branches of physics. But first of all, for extending his friendship and his fine human culture to me, and thereby introducing me to the cultures of his two home countries, India and Canada.

I also want to include Manjudi, Ronnie, Ranju, Mallika and Sharmila in my thanks for many unforgettable hours in my Indian-Canadian homes at Dundas (in the first house on Skyline Drive, I met also "Frosty" the puppy dog), with good food, entertaining movies and games.

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[15] see, e.g., H. Schomerus and M. Sieber, J. Phys. **A 30**, 4537 (1997)
[16] A. Magner, S. N. Fedotkin, *et al.*, Prog. Theor. Phys. (Japan) **102**, 551 (1999)
[17] A. Magner, S. Fedotkin and M. Brack, work in progress

12 CHICKEN CURRY AND HOT NUCLEI

An enjoyable start for my Ph.D. thesis

My first encounter with Professor Bhaduri goes back to the summer of 1978. I had just finished my undergraduate studies at Darmstadt and came as a graduate student to the Institut Laue-Langevin in Grenoble to work on semiclassical approximations in nuclear physics. I believe I had already been told that when I would come to Grenoble, there would also be one of the world's specialists on semiclassical nuclear physics, a certain Professor Rajat Bhaduri from McMaster University, spending part of his sabbatical year in Grenoble. At first I was very impressed, keeping somehow at distance to somebody so far superior to me, the little ignorant student. But when I met the man personally, my admiration increased and my "keeping distances" was immediately swept away. There was such a friendliness and directness in his attitude that you simply could not resist. It also turned out that the Institute had reserved for me a room that was only a very short distance away from where Professor Bhaduri lived during his stay in Grenoble.

At work he immediately came to explain me what the Wigner-Kirkwood expansion[1,2] of the partition function was all about, and how one could obtain therefrom a semiclassical expansion of the energy [21, 24]. That was hardly how a teacher would talk to his student, but could be rather characterized by: "Look how much pleasure you can get out of physics". We worked hard, but it was never strenuous and we had just a lot of fun.

Since we lived very close to one another, Professor Bhaduri invited me to his place – probably already the very first evening – for cooking and having a bottle of French wine together. He told me that he didn't really know to cook, that his wife Manju was a much better cook, but that there was one thing he was not too bad at: *chicken curry*. He also mentioned that his collaborators always had the job of cutting the onions and I gladly conformed to that rule. For the following weeks we often sat together with *chicken curry* and *Côtes du Rhône*, and by the end of his stay I had almost become an expert in curry. And since he has always shared his knowledge with others, I would like to share with you what I have learned about chicken curry [27] (see box on the next page).

At the end of that evening it had just become impossible to say "Professor Bhaduri" any more, and this internationally renowned man, author of the book *Nuclear Structure* [1] which I still nowadays, 25 years after its first appearance, highly recommend to my students as one of the best in this field of physics, had become "Rajat".

Resummation technique and saddle-point method

During the following weeks, Rajat explained a lot of physics to me and I started to get familiar with the semiclassical approximations. I worked on the *resummation technique* which allows to cure the so-called turning-point problem for local densities like $\rho(\mathbf{r})$ and $\tau(\mathbf{r})$ that can be expressed as inverse Laplace transforms of the Bloch density $C(\mathbf{r},\mathbf{r}';\beta)$. Due to the specific form of the semiclassical expansion[2,3] of C, which constitutes an expansion simultaneously in $\hbar$ and the derivatives of the single-particle potential $V(\mathbf{r})$, one obtains terms like

[1] E. P. Wigner, Phys. Rev. **40**, 749 (1932)

[2] J. G. Kirkwood, Phys. Rev. **44**, 31 (1933)

[3] B. Grammaticos, A. Voros, Ann. Phys. (N.Y.) **123**, 359 (1979)

Chicken Curry (recipe for 2 persons):

- Take one medium sized chicken and cut into pieces. Take off the skin and any fat if present (in later years this part of the recipe had become inessential).
- Cut 3-4 onions, 2-3 cloves of garlic and a piece of fresh ginger into pieces and fry the whole in vegetable oil at moderately high temperature in a big pot.
- When the onions start to get golden, put the pieces of chicken and the spices: 2 coffee spoons of turmeric, 1-2 coffee spoons of curry powder (depending on how hot the curry is and how hot you like to make it – Indian people like it **really hot**) and fry. (According to the Chef: "the spices need to be fried together with the chicken".)
- Put 3-4 potatoes cut into pieces (4 to 6 each), salt, pepper, some chili powder and a few seeds of cardamom. Cover and let cook at rather low heat for about half an hour (but **don't burn it!**).
- Put several tomatoes cut into pieces and wait until they have started to dissolve, making some nice gravy.
- Serve with rice.

Bon appétit!

$$\mathcal{L}^{-1}_{\beta\to\lambda}\left[\frac{1}{\beta^n}e^{-\beta V(\mathbf{r})}\right] = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty}\frac{1}{\beta^n}\,e^{[\lambda-V(\mathbf{r})]\beta}\,d\beta = \frac{1}{\Gamma(n)}\,[\lambda-V(\mathbf{r})]^{n-1}\,\Theta[\lambda-V(\mathbf{r})]\,, \quad (1)$$

which diverge at the classical turning point. The idea of the resummation technique is now to resum, to all powers in $\hbar$, the derivatives of $V(\mathbf{r})$ up to a certain order. Doing this up to first derivatives one obtains a Bloch density of the form [26]

$$C^{(1)}(\mathbf{r},\mathbf{r}';\beta) = \left(\frac{m}{2\pi\beta\hbar^2}\right)^{3/2} e^{-\beta V(\mathbf{R})-\frac{m}{2\hbar^2\beta}\mathbf{s}^2+\frac{\hbar^2}{24m}\beta^3(\nabla V)^2} = C^{(TF)}(\mathbf{r},\mathbf{r}';\beta)\,e^{\frac{\hbar^2}{24m}\beta^3(\nabla V)^2}, \quad (2)$$

with $\mathbf{R} = (\mathbf{r}+\mathbf{r}')/\mathbf{2}$ and $\mathbf{s} = \mathbf{r}-\mathbf{r}'$. Taking the inverse Laplace transform of this Bloch density one obtains a density matrix which is perfectly well defined everywhere even beyond the classical turning point. One can proceed in the same way resumming all derivatives of $V(\mathbf{r})$ up to second order. By a transformation to local normal coordinates it is possible to make the problem separable and write the *harmonized* Bloch density $C^{(2)}(\mathbf{r},\mathbf{r}';\beta)$ as a product of three Bloch densities of one-dimensional oscillators with frequencies ω_i.[4]

[4] M. Durand, M. Brack, P. Schuck, Z. Phys. **A 286**, 381 (1978)

It appears that while the harmonized Bloch density is rather complicated, the linearized form of $C(\mathbf{r},\beta)$ generates densities that fall off too fast in the surface. It required again the ingenuity of Rajat to propose a form of the Bloch density [26] that combines the simplicity of the linearized form $C^{(1)}(\mathbf{r};\beta)$ with the advantages of having some second-order derivatives included, by writing

$$C^{(Bh)}(\mathbf{r};\beta) = \left(\frac{m}{2\pi\beta\hbar^2}\right)^{3/2} \left[1 - \frac{\hbar^2}{12m}\beta^2\,\nabla^2 V\right] e^{-\beta V(\mathbf{r}) + \frac{\hbar^2}{24m}\beta^3(\nabla V)^2}. \tag{3}$$

The Bloch densities $C^{(1)}(\mathbf{r},\beta)$ and $C^{(2)}(\mathbf{r},\beta)$ discussed above correspond to approximating $V(\mathbf{r})$ locally by a linear or a harmonic potential, respectively. These linear or harmonic potentials possess local quantum oscillations that have, in fact, nothing to do with the quantum oscillations of the potential that one is considering and that are of global nature. This, in turn, creates density oscillations in the nuclear interior that are completely spurious. One therefore needs to find a method to average out these spurious oscillations for obtaining the correct semiclassical contribution. It turns out that the saddle-point method is such a procedure.

If one knows the exact Bloch density and solves the inverse Laplace transform

$$\rho(\mathbf{r},\mathbf{r}') = \mathcal{L}^{-1}_{\beta\to\lambda}\left[\frac{C(\mathbf{r},\mathbf{r}';\beta)}{\beta}\right] = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} e^{\lambda\beta}\frac{1}{\beta}C(\mathbf{r},\mathbf{r}';\beta)\,d\beta \tag{4}$$

(with $c > 0$) without introducing any approximations, it is clear that one will obtain the exact density matrix including all quantum oscillations. The problem is now that using the above approximate forms of the Bloch density one will obtain, together with the average part of the density, the spurious quantum oscillations mentioned above. The average, i.e. semiclassical part of the density is given by the singularity of the integrand of Eq. (4) at $\beta = 0$, whereas the contributions from other singularities in the complex β plain correspond to the quantum oscillations.

Already at that time Rajat pointed out to me that the saddle-point or steepest-descent method[5] allows one to separate out the semiclassical contribution from this integral [26]. Writing the integral in Eq. (4) in the form

$$\frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} e^{\lambda\beta}\frac{1}{\beta}C(\mathbf{r},\mathbf{r}';\beta)\,d\beta = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} e^{S(\beta)}\,d\beta\,, \tag{5}$$

expanding the complex-valued function $S(\beta)$ around β_0 defined by $\partial S/\partial\beta|_{\beta_0} = 0$ up to second order, and taking into account higher-order terms in a series expansion of the exponential, one obtains

$$\frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} e^{S(\beta)}d\beta = \sqrt{\frac{2\pi}{S''(\beta_0)}}\,e^{S(\beta_0)}\left[1 + C_1 + C_2 + \cdots\right], \tag{6}$$

where C_1, C_2, etc., are saddle-point corrections defined in terms of the higher-order derivatives of $S(\beta)$ at $\beta = \beta_0$.

[5]P. Morse, H. Feshbach: *Methods of theoretical physics*, Vol. I (Mc-Graw-Hill, 1953)

To illustrate the semiclassical nature of the saddle-point method let us shortly consider the level density of a spherically-symmetric harmonic oscillator.[6] It is given by the inverse Laplace transform of the exact partition function

$$Z(\beta) = \int C(\mathbf{r};\beta)\, d^3r = \mathcal{L}_{\varepsilon\to\beta}[g(\varepsilon)] = \frac{1}{8}\,\sinh^{-3}(\frac{\beta}{2}\hbar\omega)\,. \tag{7}$$

The corresponding semiclassical level density is known [21] to be given by

$$g_{sc}(\varepsilon) = \frac{\varepsilon^2}{2\,(\hbar\omega)^3} - \frac{1}{8\hbar\omega}\,. \tag{8}$$

Performing the integral by the saddle-point method without corrections yields

$$g_0(\varepsilon) = \frac{1}{\sqrt{6\pi}}\frac{1}{4\hbar\omega}(x^2-1)\left(\frac{x+1}{x-1}\right)^{3x/2}, \tag{9}$$

where $x = 2\varepsilon/3\hbar\omega$. In the semiclassical limit of large quantum numbers, $x \gg 1$, this reduces to

$$g_0(\varepsilon) = \frac{1}{9}\frac{e^3}{\sqrt{6\pi}}\frac{\varepsilon^2}{(\hbar\omega)^3} \approx 1.028\frac{\varepsilon^2}{2(\hbar\omega)^3}\,. \tag{10}$$

Taking into account successively the first and the second saddle-point corrections, one obtains in the limit $x \gg 1$ the approximations

$$g_1(\varepsilon) \approx 0.9995\left[\frac{\varepsilon^2}{2(\hbar\omega)^3} - \frac{36}{35}\frac{1}{8\hbar\omega}\right], \qquad g_2(\varepsilon) \approx 0.9999\left[\frac{\varepsilon^2}{2(\hbar\omega)^3} - \frac{2520}{2521}\frac{1}{8\hbar\omega}\right]. \tag{11}$$

This example is given here as a demonstration that the steepest-descent method is indeed a semiclassical approach, in a way similar to the Strutinsky smoothing. But another reason is that Rajat likes this kind of *Mickey Mouse examples*, as he calls them, from which you can learn a lot of physics (and math).

As another illustration of the semiclassical character of the saddle-point method, let me give in Tab. 1 the energies of $N = Z$ nucleons in a spherical Woods-Saxon potential,

$N=Z$	E_{TF}	E_1	E_2	E_{Bh}	E_{ETF}	E_{Str}
20	-717.6	-754.0	-686.3	-683.5	-680.8	
36	-1368.2	-1420.6	-1322.2	-1318.2	-1315.0	-1315.4
82	-3344.6	-3432.5	-3267.1	-3259.1	-3254.9	-3256.0
130	-5484.5	-5602.4	-5381.3	-5369.0	-5364.0	-5364.4
208	-9044.7	-9203.9	-8906.3	-8887.5	-8881.5	-8882.8

Table 1: Semiclassical energies of $N = Z$ particles in a spherical Woods-Saxon potential ($V_0 = 44.0$ MeV, $a = 0.67$ fm, $R_0 = 1.27\ A^{1/3}$ fm), using the Bloch densities $C^{(TF)}$, $C^{(1)}$, $C^{(2)}$, $C^{(Bh)}$, and the saddle-point method.[6] The ETF energy (E_{ETF}) and the Strutinsky-averaged quantum-mechanical energy (E_{Str}) are taken from Ref. [25].

[6] J. Bartel, M. Durand, M. Brack, Z. Phys. **A 315**, 341 (1984)

calculated with the saddle-point method and using the different Bloch densities. It clearly shows that in addition to its simplicity, the Bhaduri approximation (3) yields the best results, often far better than the harmonized approximation. It has also been shown[6] that both these approximations are able to approximate the quantum-mechanical density distributions very well on the average, i.e., washing out shell oscillations in the interior.

One can now attempt to use these semiclassical approximations to perform selfconsistent semiclassical calculations, based on the partial resummation technique together with effective nucleon-nucleon interactions of the Skyrme type.[7,8] When Rajat left Grenoble that summer of 1978, he invited me to McMaster for collaborating with Michel Vallières on the selfconsistency problem, using the partial $\hbar$ resummation. So I went to Hamilton in the spring of 1979 where I worked with Rajat and Michel for 2 months. I had a very nice time staying with Rajat and his family in the beginning, meeting Manju, his wife, Ronnie and Ranju, their two sons, and Mallika (Tukun), their little daughter who was 3 or 4 years old by then. She often came to me asking: "Jonny, do you have candies?" How could I resist?

After my stay at McMaster, Michel Vallières came to Grenoble where we continued our work and finally got it to converge, after solving[9,10] the problem of numerical instabilities that emerged in the Hartree-Fock like iterative procedure of the selfconsistency cycle and that are coming from the asymptotic nature of the saddle-point correction series.

Hot stuff

As I mentioned hot nuclei in the title of my talk, it is about time to start talking about semiclassical calculations at finite temperature. One can show[11] that the single-particle free energy can be written as

$$F_{sp} = \sum_j \varepsilon_j^{(T)}\, n_j^{(T)} - T\,S = \int \varepsilon\, g_{_T}(\varepsilon) d\varepsilon\,, \tag{12}$$

where (the Boltzmann constant is put to unity and temperatures T measured in MeV)

$$g_{_T}(\varepsilon) = \sum_j \frac{1}{4T} \cosh^{-2}\left(\frac{\varepsilon - \varepsilon_j}{2T}\right) = \int_{-\infty}^{\infty} f_T(\varepsilon - \varepsilon') \sum_j \delta(\varepsilon' - \varepsilon_j)\, d\varepsilon' \tag{13}$$

is the temperature-averaged single-particle level density convoluted with the function

$$f_T(\varepsilon) = \frac{1}{4T} \cosh^{-2}\left(\frac{\varepsilon}{2T}\right). \tag{14}$$

Using the folding theorem for Laplace transforms one can write the temperature-dependent partition function $Z_T(\beta)$, which is the (two-sided) Laplace transform of the level density $g_{_T}(\varepsilon)$, in the form

$$Z_T(\beta) = Z(\beta)\, \tilde{f}_T(\beta)\,, \tag{15}$$

[7] T. R. Skyrme, Nucl. Phys. **9**, 615 (1959)
[8] D. Vautherin and D. M. Brink, Phys. Rev. **C 5**, 626 (1972)
[9] J. Bartel and M. Vallières, Phys. Lett. **114 B**, 303 (1982)
[10] J. Bartel, Ph.D. thesis (Universität Regensburg, 1984), unpublished
[11] M. Brack, P. Quentin, Nucl. Phys. **A361**, 35 (1981)

where $Z(\beta)$ is the partition function at zero temperature and $\tilde{f}_T(\beta)$ the two-sided Laplace transform of the temperature averaging function $f_T(\varepsilon)$, Eq. (14)

$$\tilde{f}_T(\beta) = \mathcal{L}_{\varepsilon\to\beta}[f_T(\varepsilon)] = \frac{\pi\beta T}{\sin(\pi\beta T)} \,. \tag{16}$$

It then follows from Eq. (15) that the temperature-dependent Bloch density can be simply written as

$$C_T(\mathbf{r},\mathbf{r}';\beta) = C(\mathbf{r},\mathbf{r}';\beta)\,\tilde{f}_T(\beta) \,. \tag{17}$$

With the Bloch density at finite temperature defined in this way, all selfconsistent semi-classical calculations can be carried through exactly like in the case of cold nuclei.[12]

Coming back to the Mickey Mouse example of the single-particle level density in a spherical harmonic oscillator, but now considering the case of finite temperatures where all calculations can again be performed analytically, one can demonstrate how the excitation of a fermion system washes out all shell effects and how, at temperatures $T \gtrsim \hbar\omega/2$, the exact level density (containing all quantum effects) has become essentially of pure semiclassical nature. This is demonstrated in chapter 3 of Rajat's and Matthias' book *Semiclassical Physics* [3].

One can also generalize the extended Thomas-Fermi theory to finite temperatures,[13] which allows to derive functional expressions for the kinetic energy density $\tau^{(T)}_{ETF}[\rho]$ or the free-energy density $\mathcal{F}^{(T)}_{ETF}[\rho]$ and the entropy density $\sigma^{(T)}_{ETF}[\rho]$ in a similar way as it was done at zero temperature. One obtains again expansions, e.g., for the kinetic-energy density functional $\tau[\rho]$, in terms of the local density and its derivatives. The coefficients of this expansion are now given in terms of ratios of Fermi integrals of half-integer indices[13]. Since at finite temperatures the densities $\rho_{ETF}(\mathbf{r})$, $\mathcal{F}_{ETF}(\mathbf{r})$, $\tau_{ETF}(\mathbf{r})$, etc., are perfectly well defined in all space, we do not encounter here the turning-point problem that was present at zero temperature. One can therefore use the functional expressions derived at finite T and take the limit $T \to 0$ to test the validity of the ETF functionals which had been derived in the classically allowed region only, also beyond the classical turning point. It was the merit of Matthias to have made this demonstration.[14]

For my part I would like to thank you, Rajat, for your friendship and for the time we have spent together, doing physics, cooking curry or smoking a pipe – just another thing you got me into, saying that it might cure my cough. But whatever you do, together with Rajat it turns out to be fun. Thank you for that, Rajat, and

Happy Birthday!

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[12] J. Bartel, M. Brack, C. Guet, H.-B. Håkansson, Phys. Lett. **139 B**, 1 (1984)
[13] J. Bartel, M. Brack, M. Durand, Nucl. Phys. **A 445**, 263 (1985)
[14] M. Brack, Phys. Rev. Lett. **53**, 119 (1984); *ibid.* (Erratum) **54**, 851 (1985)

13 CHAOTIC EVOLUTION IN THEORETICAL PHYSICS FROM MCMASTER INITIAL CONDITIONS

A difficult exercise

It is a constant among different types of societies to prize features of personal characters most likely leading to some kind of social achievements. The adjective "positive" is often quoted but one has to admit that the concept is rather elusive. Being rational in such matters is certainly not easy. Certain criteria have to be set up and the choice here is of course "ad libitum" and certainly prejudiced. The task becomes even more difficult if the course of life and activities in physics are somewhat chaotic at first glance, with only occasional overlap with Rajat over the years. Moreover, as well known by now, an important characteristic of chaotic evolution is the memory loss of the initial conditions.

I will try to use criteria relevant enough to resist the erosion of time and allege that my evolution in physics is not so disordered to have lost the original messages of my McMaster tutors.

What kind of criteria?

In the search for time-resisting criteria it is natural to turn back to the philosophic heritage of our western world.

Socrates did not teach regularly, neither did he publish any book – and nevertheless his messages did not perish. He was hostile to any dogmatic teaching, and his method is basic to student education in general. His way was to lead his interlocutor to truly fundamental considerations, trough ironic questions and forcing them to face their own contradictions. He gathered considerable influence over the youth, and was sentenced to death for this very reason. Fortunately, times have changed in this respect and, as a graduate student, it was my feeling that Rajat followed the same approach to discussions, however safely, till today. This experience took place in the seventies during my Ph.D. years. I witnessed and was part of many heated discussions on the method of "shell corrections" and "three-body forces". Rajat's contribution with Yuki Nogami and Carl Ross [19] was the origin of a subsequent work with Carl, Yuki, and Donald Sprung,[1] ending only for me in 1988.[2]

But let me elaborate further on the conditions of a scientific dialogue, as I experienced it with Rajat during this period. As most graduate students deeply involved in their own subject, I had the natural tendency to practice first a monologue based on certitudes. In keeping with Socrates' practice, Rajat had a way to bring me to dialogue through apparently innocent if not ignorant questions to which he claimed not to know the answers. Since most of the time I did not know them either the effect was a salutary shake of my early born certitudes. I quickly learned that every piece of knowledge is always to be questioned and there is no subject, even apparently settled, which does not deserve a fresh and critical look up.

[1] P. Grangé, M. Martzolff, Y. Nogami, D. W. L. Sprung and C. K. Ross, Phys. Lett. **60 B**, 237 (1976)

[2] P. Grangé, A. Lejeune, M. Martzolff, J. F. Mathiot, Phys. Rev. **C 40**, 1040 (1989)

During my stay in Heidelberg and throughout my long collaboration with Hans Weidenmüller this conscious uncertainty was a major theme of our discussions. We came to question the foundation of the "statistical theory of nuclei" and discovered the importance of transient behaviour in the competitive decay of excited nuclei. The many-body problem in its semiclassical aspects, so carefully disentangled by Rajat and our colleagues here today, was a keystone to this investigation, the other being the stochastic evolution of the collective variables. Here was my first contact with "stochastic processes" and a subsequent chaotic evolution to field theory. But I did not really lose the memory of the initial training since I had interest in the subject from undergraduate courses and Yuki Nogami's graduate teaching.

Another fruitful experience can be phrased in terms of Plato's symbolic tale of the cave. Indeed, the newly born graduate facing permanent research obligations is very much like the prisoner in the cave. His early vision of Physics is enlightened by the back light of his masters' teaching. After 2500 years almost every argument of Plato's analysis is still valid. How to free oneself and take the way to the entrance of the cave to reach full light? Again the capacity to do so is the real signature of a successful education. But at the same time it is equally important to understand that this search for an intelligible world is a constant lesson of humility because of proper human limitations. The message I gathered from Rajat was to look beyond the appearance of the shadows in the cave and their puppeteers, focusing only on problems away from artifacts, in the most simple possible way ever.

For all these long-lasting principles I believe that Rajat accomplished a very distinctive career in research and teaching. I owe him full recognition, also for the warmth of his "Epicurean" hospitality. But this is another aspect which will certainly be emphasized by all his colleagues and friends here.

I want to thank all those who took part in setting up this memorable gathering, and Matthias Brack in particular.

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14 TALES FROM THE FLAT LAND

Rajat Bhaduri, Rajatda in short for many of us, has an impeccable sense of direction, quantized via the so-called "Bhaduri phase" ϕ_{Bh}. If he has to go somewhere, his intuition is to approximately start out in the opposite direction. Hence $\phi_{Bh} = \pi$. My wife, Hema, has the same impeccable sense of direction. Once, on a nice summer day in Dundas, Rajatda thought he should start his gardening. He set out to get some nice flowering plants from a nearby nursery, just about five minutes drive from home. Hema joined him in this endeavour as his navigator. They came back after about two and a half hours with some nice plants collected from all over the place except from the intended nursery. It was later "admitted" that they "lost their way" and instead of heading back to Dundas, they were half way down to Guelph or Waterloo. But then he found "new pastures" which would not have been obvious to those who head straight. In a sense this also illustrates Rajatda's search for new ideas and avenues in Physics. I have thoroughly enjoyed joining him in his enduring Odyssey all these years without a break. There is certainly a lot of excitement and fun extending from Physics to Cooking. I consider myself fortunate to belong to this big family of Bhaduri's; trust me it is really a big one.

I met Rajatda way back in 1981 in Bangalore when both of us were spending a few months at the Centre for Theoretical Studies. For those of us who had read that excellent book on Nuclear Structure by Preston and Bhaduri [1], he was of course a familiar name. I had just finished my Ph.D. and a year of teaching in the University of Mysore. We use to head for lunch together, where he would devour his favourite Dosa and I, my curd rice. We got used to discussing various topics of our common interest during those luncheon meetings. Occasionally we were joined in by his wife Manjudi and the children. In one of those lunch breaks, Rajatda asked me if I would join him at McMaster University as a post-doctoral fellow. I did say yes, though until then I had not seriously considered going abroad. However, I could join him only after nearly two years since I was committed to go to Tata Institute of Fundamental Research (TIFR) in Bombay.

It was my good fortune that Rajatda kept the offer open and he also assisted my wife Hema, whom I married while still at TIFR, in getting admission to McMaster University. Finally, I arrived in Hamilton on a nice summer day in May 1983, to be joined by Hema at end of August the same year. I also won an unofficial bet with Rajatda soon after arrival. Driving me from the airport, Rajatda quizzed me about my food habits. On learning that I am a vegetarian he announced that I would soon be eating chicken. It was challenging enough for me to announce this as a bet, with the time limit of one year. I have not touched a chicken, dead or alive, in my life as yet, let alone eat it. The bet itself fizzled out since we did not shake hands – how could I, since he was driving when we took the bet, and in any case I did not know at that time that this formality had to be completed for any bet to be official. Later when he innocently asked if we shook hands, I said no without realizing the big blunder about to be committed.

After some initial fishing around, we began our collaboration on the baryon spectroscopy. By the time I reached McMaster in May 1983, Rajatda, Byron and Jim had already started working on the spectroscopy of baryons, sowing the seeds of the so called deformed quark model [31]. The basic idea came from the experimental result that the first breathing mode excitation, the so called Roper resonance, was found to have an energy lower than the P-wave baryons. This is surprising considering the fact in a shell model type description the breathing mode is actually a second excitation which should be higher than the first excitation, namely the P-wave or odd parity excitations. This however is a very normal occurrence in nuclear physics where such phenomena are common whenever the mean field is deformed. The deformed quark model was inspired by this well known fact. In collaboration with Jishnu and Mira first [32, 33], and later with Matthias and Byron [34], we set out to do the full spectroscopy of nucleon and delta excitations with deformation of the mean field as the central idea. Indeed we did manage to pin down the Roper resonance to its right energy while getting reasonable agreement for not only P-wave baryons but also the higher resonances up to about 2.2 GeV. Later we also did the spectroscopy of strange baryons with some success [35]. While the quark models are yet to be put on a firm footing from the point of view of the theory of strong interactions, it remains a fact that the deformed quark model provides the simplest explanation to the Roper puzzle. Some experts in baryon spectroscopy still view this as an outstanding problem. Together with Matthias, we also looked at the radiative transitions in non-strange baryons. The deformed quark model remains, however, largely ignored which may be partly due to the fact that as a calculational tool it is not as amenable as the simple non-relativistic quark model.

After spending a couple of years on the deformed quark model, we drifted around to various, not so greener, pastures. Some time during 1985, there was a letter from Rajaji from Madras asking Rajatda to suggest some young people for faculty positions opening up in the Institute of Mathematical Sciences, Madras. Rajatda promptly suggested me and soon we settled down in Madras. During this period Rajatda also introduced me to the nuances of the so called EMC effect – this effect has to do with the differences between measured free nucleon and bound nucleon structure functions as a function of the momentum fraction. While analyzing this effect [38], I got introduced to aspects of perturbative quantum chromodynamics (QCD). Later I continued my work on perturbative QCD with my students in Madras, while Rajatda was working on chiral Lagrangians, the Nambu-Jona Lasinio model, etc. He even sat and wrote up his second book, "Models of the Nucleon – From Quarks to Soliton" [2] during that time.

I went back to McMaster in 1990 for a year on leave from Madras, and thus began the second phase of intense interaction with Rajatda. These were the heady days of high-temperature superconductivity. Several theoretical ideas were floating around, one of which is the so called "anyon superconductivity". Anyons had been around for over a decade by then, but became topical only after the high-temperature superconductivity was discovered. Anyons are particles with arbitrary statistics under exchange, and live in a two-dimensional world. The statistics is characterized by a single parameter that resembles a Berry phase (in the sense that it is topological). For integer values of this parameter one recovers the usual Bose and Fermi statistics. Rajatda had been interested for a few months in this even before my arrival, and as soon as I arrived he gave me a run down on anyons. I had heard a few seminars before on anyons but did not get much out of them. Rajatda gave me an introduction to the quantum mechanics of two anyons in his usual pedagogical manner and got me interested in the subject matter of anyons. Thus began our "random walk in the flat land" which is continuing even today. He had already calculated the two-anyon canonical partition function and the second virial coefficient using a semiclassical method (he loves this subject). This could also be calculated exactly, but the semiclassical method gives the exact answer, surprising but not difficult to understand. The real problem was in calculating the third virial coefficient in the equation of state – this requires in principle the solution of the three-anyon problem which is a difficult problem. We (with Jimmy, Avinash and Rajeev) made some progress using semiclassical methods [43]. Later Rajatda pursued this problem further after the three-body quantum spectrum was solved to some extent with Jimmy and Akira.

The insufficient progress, initially, in computing the third virial coefficient spurred us to actually look at the problem of solving the three-anyon spectrum exactly, at least the ground state and the low-lying excitations. Even the structure of the ground state was an outstanding problem. The last paper on this problem had actually been written by Yong Shi Wu in 1984, where the problems were first mentioned. By this time Matthias was also roped in and Jimmy was already there. We set out to do this numerically by diagonalizing the three-anyon Hamiltonian. By May of 1991, we had a pretty good idea about how the ground state interpolates between the bosonic and the fermionic limits as a function of the statistical parameter and we were looking at the excited states. Rajatda organized the 70th birthday symposium for Mel Preston during this period. One of the invitees was Gerry Brown from SUNY at Stony Brook. After the birthday symposium we met informally with Gerry Brown and described various things we were doing including the progress we were making with the three-anyon problem.

A couple of weeks later, Rajatda received a letter from Gerry Brown that a paper had been submitted to PRL from Stony Brook on precisely the three-anyon spectrum by his colleagues Sporre, Verbaarschot and Zahed. There were indications in his letter that the results might be similar to what we had. Rajatda showed me the letter around three in the afternoon. I was very disappointed since we had spent non-trivial amounts of time and effort on this problem and the result was sufficiently interesting and exciting. But before I could say anything, Rajatda announced that we were going to write a paper and submit it the next morning (which would mean a gap of two or three days from the Stony Brook paper). By then we had only the raw numbers on computer printouts, and TeX or LaTeX were not yet popular as they are now. Rajatda decided that he would write the initial part of the paper, I would write the rest of the text and Jimmy was informed on the phone to arrive immediately and prepare the figures for publication. Hema (who was visiting me that summer from Madras while attending an IEEE conference at Toronto) had the least idea of what was going on, and was asked by Rajatda to help us with the preparation of the manuscript. We assembled immediately after dinner in our offices on the third floor of the Senior Sciences Building and the race to finish began in right earnest. Rajatda and I wrote up the paper and as we finished each page, it was handed over to Hema who was patiently putting everything together on the computer using a long forgotten text processor called Chiwriter. By about four in the morning we had a working manuscript which was edited and finalized by about six in the morning. Jimmy had arrived by then and was preparing the figures. The completed paper was dispatched by ten in the morning through a Courier by Jimmy. But Matthias, our other collaborator, had no clue about what was going on and was informed about it later in the day. He joined us in the celebration later when he came back to McMaster. The enthusiasm rubbed off on the family members also, Manjudi would keep inquiring about the fate of our paper often. To the credit of the editors, the two papers, one from Stony Brook and this one from McMaster, appeared back to back in the same issue of PRL [44] (see also [50]).

Around this time, Rajatda brought to my notice a paper by Duncan Haldane on a generalization of the Pauli principle[1] which was not specific any dimension as anyons were (they lived only in this flatland). True to his insight, Rajatda recognized that this paper was important but at that time I did not understand the paper well enough to make any headway. One morning in Madras, a couple of years later, my collaborator Shankar asked a question about the second virial coefficient in connection with Haldane's paper which he was reading. Shankar also explained the basic idea of the paper. We then went on to relate the statistics parameter to the second virial coefficient[2] and later were able to show that the well-known Calogero-Sutherland model of interacting particles encodes the generalized Pauli principle as discussed by Haldane[3]. In fact, we called this generalized statistics "Exclusion Statistics" as opposed to Exchange Statistics (as in the case of anyons), a name that has now stuck. Rajatda and Diptiman Sen, in the mean time, also looked at exclusion statistics from the semiclassical point of view and arrived at similar conclusions. They wrote a paper in PRL showing again the connection between the generalized Pauli principle and the Calogero-Sutherland model [59]. I consider this paper as one of the most beautiful papers on this subject.

Together with Matthias and Jimmy, we also made our entry into fields like periodic orbit theory and chaos, and also a new class of exactly solvable models in dimensions more than one. Matthias will say more on the POT. It is a collaboration that is continuing even to this day.

Rajatda has interest in sports, he is an avid follower of the tennis scene. His passionate interest in bridge is well known to all his friends. During the initial days he tried to induce me into playing bridge when he desperately needed a partner. He took me for one of those games after I became familiar with rules. I got so rattled after this game, what with all those raised voices, that I gave up playing bridge at least at this level. He however introduced me in to the charms of having a glass of Scotch Whisky in the evening before dinner. This has endured all these years. I enjoy a shot of Scotch in the evening even today (depending on the availability).

Looking back and surveying all these years of being with Rajatda (and also with Matthias who was there in most of these endeavours), one thing stands out. He taught every one of us to have fun, whether it is physics or cooking or gardening or even just having a shot at sunset. Many of his other visitors also gradually became a part of this fun-loving enterprise. Together he and Manjudi provided a home away from home for many of us.

I would like to use this opportunity to thank all the members of the Bhaduri family for being so supportive. I thank Hema for recollecting many of her own moments of joy with the Bhaduris' and help in preparing this personal recollection of our years with Rajatda and family.

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[1]F. D. M. Haldane, Phys. Rev. Lett. **67**, 937 (1991)
[2]M. V. N. Murthy and R. Shankar, Phys. Rev. Lett. **72**, 3629 (1994)
[3]M. V. N. Murthy and R. Shankar, Phys. Rev. Lett. **73**, 3331 (1994)

15 THE PRIVILEGE OF BEING A THEORETICAL PHYSICIST

I met Rajatda for the first time in 1986 at Bhubaneswar where he was participating in a symposium on "Current Trends in Physics" on the occasion of the completion of the decade of our institute (IOP). He was obviously the star of the meeting. I spoke in that meeting on the work on charged vortices which Samir Paul and I had just done.[1] Besides, I had just given a course of lectures on topological aspects of quantum field theory and chiral anomalies. Rajatda invited me to visit McMaster and give a set of lectures on the same topic in 1987. When I gave those lectures at McMaster, I was immediately struck by the fact that Rajatda took the lectures very seriously and asked me very probing questions, and I must say that my understanding of the subject became much better because of these discussions. Little did I realize that within few months Rajatda would master many of these intricate things and become an expert. The proof of that came when his remarkable book [2] soon came out. As far as I am aware of, this is the first book on *modern nuclear physics*. He also wrote several interesting papers using these ideas. Particular mention may be made of his work on bosonization and a generalized Thirring model [41].

Our collaboration really started after Rajatda and Murthy visited IOP for a month in early 1989. Coming from rather different backgrounds, I was somewhat apprehensive if we can talk on the same plane and find some common problem of interest. However, soon I found that Rajatda is a true theoretical physicist, an allrounder with a remarkable breadth of knowledge in several areas of physics. The first work that we did together was only possible because of Rajatda's vast knowledge of statistical mechanics and his ability to correlate ideas from one field to another. In particular, using Wigner-Kirkwood expansion we showed [42] that the entire contribution to the Witten index comes from one loop (i.e., the lowest non vanishing term in the $\hbar$-expansion) and that all terms to at least the next four orders in $\hbar$ vanish. I have a gut feeling that this must be true to all orders, but I do not know how to prove it. I also believe that there must be a formal proof that the Witten index must get a contribution from only one loop.

In August 1989 I visited Orsay and came across an interesting paper by Comtet and Ouvry,[2] who obtained a connection between the second virial coefficient a_2 of a non-interacting anyon gas and chiral anomaly in $1+1$ dimensions. Now it is well known that chiral anomaly gets contributions from only one-loop diagrams, and two and higher loops do not contribute to it. Hence I felt that a_2 must also be only one-loop. I must admit that it was a pure hunch and I had absolutely no idea at that time as to how to go about seeing if this is true or not. I mentioned my hunch to Rajatda. But this was the time before e-mail became order of the day, and so I lost contact with Rajatda. Next summer, Rajatda arranged my visit to McMaster as a Hooker Professor. As he met me at the airport, first thing that he told me was that he and Rajeev (Bhalerao) have already set up the formalism to study the virial coefficients of an ideal anyon gas in semiclassical approximation and that the initial indication was that indeed the semiclassical approximation could be exact for a_2 and that now they were trying to compute the third virial coefficient a_3 of the ideal anyon gas. I was absolutely amazed. But I should not have, because Rajatda has this uncanny ability to pick up the right hunch and, most importantly, he knows how to set up

[1] S. K. Paul and A. Khare, Phys. Lett. **174 B**, 420 (1986)

[2] A. Comtet and S. Ouvry, Phys. Lett. **225 B**, 272 (1989)

the formalism, do hard calculations and go much beyond trivial guesswork. Besides, he is very ambitious. I probably would have been happy just studying a_2 in the semiclassical approximation but he went much further and had already thought about the higher virial coefficients!

Over the last ten years, I have seen this trait so many times. During the two months that I was there, we convinced ourselves that indeed the semiclassical approximation is exact for a_2 but we could not make much progress about a_3 because while we knew how to isolate symmetric and antisymmetric contributions, we did not know how to isolate the mixed-symmetry contributions in the three particle case. In fact, so far as I am aware of, even now no one knows how to do it. I again lost contact with Rajatda, but little did I realize that he was not the person to give up easily. He has also that uncanny ability to make other people interested in what he is doing. Soon I found that Murthy and Jimmy had joined in and they were able to calculate a_3 numerically in Boltzmann basis and obtain an interesting relation between a_3 and a_2 in that basis [43]. There is no doubt that Rajatda was the leader of the collaboration and the whole thing was revolving around him. Of course, by now this has happened so many times but, it was a new experience for me at that time.

Rajatda visited me in July-August 1990 and we deliberated about various issues regarding anyons. We discussed about how the spectrum of two anyons can be computed in several potentials. In particular, we talked about the spectrum in an attractive Coulomb field. But, of course, we realized that this is an unphysical situation as it will imply that these are not identical anyons. The discussion probably ended there. But Rajatda, having the knack to realize what is physical, interesting and doable, had come to the conclusion that the thing to do was to consider the scattering of two identical charged anyons (and hence experiencing a repulsive Coulomb potential $\sim 1/r$). What he had in mind was essentially a three-dimensional system confined in a plane, as in the quantum Hall effect. As usual, he was able to create interest in this problem in Jimmy and MK (Srivastava) and they found elegant closed expression for Mott scattering of anyons. We showed that there is a marked asymmetry in the differential scattering cross-section between forward and backward angles [47].

I spent May-June of 1991 at Orsay where John McCabe and I looked at the nature of the three-anyon ground state in an oscillator potential by using perturbation theory around the three-fermion ground state. To our great surprise we found that there is a crossing of levels in the ground state.[3] It was quite a mysterious result and I communicated it to Rajatda by e-mail. To my great amazement he told me that they, too, had obtained the same result but in an entirely different way [44]. They had numerically obtained the three-anyon spectrum and found such crossings not only in the ground but even in the excited states. This was yet another proof that once Rajatda examines something, he studies it in great detail and has the knack to go for the key issues. Rajatda was one of the first to realize that the understanding of a_3 for an ideal anyon gas was crucial as it contains nontrivial information about braiding effects. So he immediately took up its computation [48] using perturbation theory around the three-fermion state. This is another trait of Rajatda. When important issues are involved, he is not afraid to go for big numerical computations. Subsequently, when he realized that only the nonlinear states contribute to a_3 and that I knew how to isolate the linear states from the nonlinear states using the

[3] A. Khare and J. McCabe, Phys. Lett. **269 B**, 330 (1991)

hyperspherical basis, he convinced us about the importance of the accurate computation of a_3 using only the nonlinear states. There is no doubt that during these three years Rajatda has made significant contributions towards our understanding of anyons.

Most of us usually have a single-track mind. While we are working in an area we usually close our eyes to other areas which could be connected to it. But not Rajatda! In 1993 while I was visiting him, he one day suggested that we should study Calogero-Sutherland type models because they seem to be connected to anyons. At that time this connection was unknown, but he has this intuitive way of seeing it before others. By then I knew Rajatda quite well and took his suggestion seriously. So I decided to read Calogero's classic papers of 1969 and 1971 and I am really grateful to Rajatda for his suggestion. It is only then that I realized the richness of these models, and in the last seven years I have been able to contribute a bit to this area. Coming back to McMaster in 1993, I carefully read Calogero's paper where he has obtained the exact solution of a three-body problem on a line with an inverse-square interaction.[4] I have had several discussions with Rajatda during this time and through these discussions it soon became clear that the key idea of Calogero was to use the Jacobi coordinates, remove the centre of mass and then show that the remaining problem corresponds to that of one particle in two dimensions, experiencing a noncentral but separable potential. We also realized that the Schrödinger problems so obtained in ρ and ϕ variables are examples of shape-invariant, exactly solvable problems in one dimension. It was then clear to us that one can now discover several new three-body problems in one dimension corresponding to either Coulomb or oscillator problems in the ρ variable and any of the known seven shape-invariant potentials in the ϕ variable [56]. We also showed in this paper that the equal mass three-body problem with the potential

$$V = \frac{\omega^2}{3}\sum_{i<j}(x_i - x_j)^2 - \lambda(x_1 - x_2)(x_2 - x_3)(x_3 - x_1)\,, \tag{1}$$

is an example of a classically chaotic motion, since after transforming to polar coordinates the potential is the famous Hénon-Heiles potential.

During this period we also realized that one can considerably enlarge the list of analytically solvable problems in quantum mechanics. In particular, we showed that those noncentral potentials for which the Schrödinger equation is separable are analytically solvable provided the separated problem for each of the coordinates belong to the class of exactly solvable one-dimensional problems [57]. As an illustration, we gave a list of such problems in spherical polar coordinates in both two and three dimensions, while the generalization to other coordinate systems was pretty obvious. Rajatda suggested that we write up this work for American Journal of Physics since it should be of interest even to graduate students. He also emphasized that we should write the paper in such a way that an average graduate student should be able to appreciate the key ideas and also should be able to work out the main steps. I have not seen many people who worry so much about readers, specially graduate students, while writing a paper.

In July-August 93 Rajatda visited us and gave an inspiring talk about some work which he had done with Murthy and Date (if my memory serves me right) about Riemann zeta function $\zeta(s)$. I was really impressed by his talk as well as by the fact that he can work in

[4]F. Calogero, J. Math. Phys. **10**, 2191 (1969)

so many different areas and on so many different concepts. Little did I realize then that next year I would be working with him on this topic. When I visited him in May-June 94, I found that he was still interested in the ζ function and since I, too, was eager to learn a bit about the celebrated zeta function, we started talking about the various issues. I must say that this was one of our most enjoyable collaborations. Here we were collaborating in an area in which neither of us was an expert, even though Rajatda's knowledge was obviously much more than mine. It was sheer fun trying out various ideas. Once again Rajatda demonstrated his knack for smelling key issues. He requested Jimmy to make a plot of real vs. imaginary part of $\zeta(s = \sigma + it)$ for several different values of t in the cases $\sigma = 1/2, 0.6$, and 1. As soon as we saw the plots at $\sigma = 1/2$, their similarity to the resonant quantum scattering amplitude was immediately clear to us. Using this analogy we were able to derive an approximate quantization condition for the location of the zeros of $\zeta(s)$ at $\sigma = 1/2$. This is what Rajatda was after for the last several months. In this collaboration I had another experience of Rajatda's ambitious attitude. He wanted to look for a potential whose density of states will be related to the smooth part of $\zeta(s)$ at $\sigma = 1/2$. It is only because of his persistence that we tried it and eventually showed that the corresponding potential is the inverted oscillator potential [60]. We were quite happy when several months later we saw a paper by Berry and Keating about the ζ function, and the model Hamiltonian suggested by them was related to the inverted oscillator by a canonical transformation.

After I went back to Bhubaneswar, I more or less stopped thinking about this problem – but not Rajatda! He was not satisfied with whatever we had done. He wanted to understand as to what happens to the memories of these zeros as σ moves away from 1/2. As usual, he was able to interest Steffi and Ed in these issues and started working with them. When I went back to McMaster in May-June 95, I found that a lot of progress had been made in understanding these issues. I then joined Ed and Rajatda; Steffi had already left McMaster (here is one more collaborator of mine whom I have never met!). I must confess that my contribution in this collaboration was minimal while as usual Rajatda was the key person. I particularly remember learning from him about the Gutzwiller trace formula. It was fun connecting the trace formula for $\sigma \gg 1$ to the one generated by a one-dimensional harmonic oscillator in one direction along with an inverted oscillator in the transverse direction and then to show that the Gutzwiller trace formula is exact for this system [67]. By the time this work was over, Rajatda had become an expert in this area and the proof of that is the excellent book that he and Matthias have written about semiclassical physics [3].

My latest collaboration with Rajatda started during his visit to Bhubaneswar in late 1995. Around that time, he along with Murthy and Diptiman had studied an N-body problem in two dimensions with both two- and three-body interactions. In particular, they obtained the exact N-boson ground state as well as some excited states and showed that all of them have novel correlations of the form

$$X_{ij} = x_i y_j - x_j y_i \,, \tag{2}$$

built into them. The spectrum obtained by them was linear in the coupling parameter. So all of us were curious to know if the entire spectrum is linear in the coupling parameter. Rajatda has this knack of separating the essential from the inessential and posing the key issues. During our discussion, it became clear that the answer to the question is not easy unless one can actually solve the two- or three-body problem. Obviously, the simplest one is the two-body problem. But soon we realized that because of the lack of translation invariance, even this problem is not easy to solve. However, after a lot of discussion we were able to show that the Schrödinger equation for the two-body problem can be cast in terms of the Huen equation. Rajatda then left for Chennai. As usual he was able to interest Jimmy, Murthy and Diptiman in this problem and we were able to show that in general even the two-body spectrum is not linear in the coupling parameter [65].

I have known Rajatda for more than 13 years. The thing that I admire very much is the importance that he gives to teaching. He has great respect for academics and in these days of liberalization he is one person who is very appreciative of those kids who are still opting for academics as their career. By now he has become a friend, elder brother and a family member. Not only me but Pushpa, Apu and Anu are always eagerly looking forward to his visits. He is a very warm-hearted person. He is passionate not only for physics but even for cooking, gardening and of course for bridge. I have played with him both as a partner and as an opponent and we have even played in a tournament together and I have survived his onslaught!

I must add here that any description of Rajatda will be incomplete unless one also mentions Manjudi and the family. Visit to him automatically means visit to the family and I know as a fact that all his visitors have been made to feel as members of the Bhaduri family.

Rajatda is one of the very rare persons who have a passion for physics and who consider it as a privilege to be a theoretical physicist. I have no doubt that retirement or no retirement, Rajatda will continue to do physics all his life. I wish him a good afternoon and look forward to a vigorous collaboration with him.

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16 SHELL STRUCTURE AND CLASSICAL ORBITS IN MESOSCOPIC SYSTEMS

When I started my Ph.D. program with Rajatda, he was more and more interested in the close connection between classical periodic orbits and quantum interference phenomena. He then got this fascinating idea concerning a cranked two-dimensional harmonic oscillator [54], and together with Shuxi Li and Jim Waddington, we had a lot of fun with it. When plotted as a function of cranking frequency, the spectrum of this system beyond the Landau level limit exhibits not only the Farey fan pattern, but also a sequence of shell gaps that strikingly resembles the Haldane hierarchy in the fractional quantum Hall effect (FQHE). Rajatda's original idea was to explain the FQHE in this mean field picture and we made several attempts in this direction [53]. We did not quite succeed to establish the relation between this model and the FQHE, but the shell structure beyond the Landau levels and its resemblance to the the Haldane hierarchy is very intriguing, and I learned a lot during this work. After all, this was my first encounter with Rajatda's insight, curiosity, strong drive and enthusiasm to find and attack a new problem. In fact, he was deeply in love with this problem, and whenever we found a promising result as we proceeded, he sometimes got too excited so that Jim had to "beat him down". I also remember Rajatda telling us, after some unsuccessful trials, that it was like a crush on a girl he had when he was a school boy – he knew it would not work out and he tried to keep the idea out of his mind, but the thought kept coming back! In this paper, I would like to summarize this work [54] and then talk a bit about another work that followed.

We study the spectrum of a particle in a rotating two-dimensional harmonic oscillator, whose Hamiltonian is given by

$$H = \frac{1}{2M}(\,p_x^2 + p_y^2\,) + \frac{1}{2}M\Omega^2(x^2 + y^2) + \omega\,(xp_y - yp_x)\,. \tag{1}$$

Here Ω is the oscillator frequency and ω is the cranking frequency about the negative z-axis. This problem is analytically solvable, and the eigenvalues can be written as

$$E_{n_r,l} = (\,2n_r + |l| + 1\,)\,\hbar\Omega \,+\, l\hbar\omega\,, \quad n_r = 0, 1, 2, \ldots, \quad l = 0, \pm 1, \pm 2, \ldots, \tag{2}$$

where n_r and l are the nodal and angular quantum numbers, respectively. In Fig. 1 the pattern of the energy levels (2) as a function of ω is shown for $0 \leq \omega \leq 2\Omega$. We define

$$\widetilde{\omega} = (\omega - \Omega), \quad \nu = \frac{\widetilde{\omega}}{2\Omega}, \tag{3}$$

and plot the levels as a function of the dimensionless quantity ν; for $-1/2 \leq \nu \leq 1/2$ in Fig. 1. As ω increases from zero, it breaks the original symmetry of the isotropic oscillator and lifts the degeneracy of the energy levels. It then gives rise to a series of shell gaps, each set of gaps concurring with the formation of classical periodic orbits. As we can see clearly in Fig. 1, the quantum gaps that are even larger than the original ones are generated, when $\omega = \Omega$. They are equivalent to the Landau levels for a charged particle in a uniform magnetic field. Writing the charge as $e = -|e|$ and taking the symmetric gauge, (1) with $\omega = \Omega = \omega_c/2$ is equivalent to the Hamiltonian for such a particle, where $\omega_c = |e|B/Mc$ is the cyclotron frequency. As is clear from this figure, the collapsing single-particle states in the lowest Landau level have all aligned (but different) angular momenta, and originate from different shells of the harmonic oscillator.

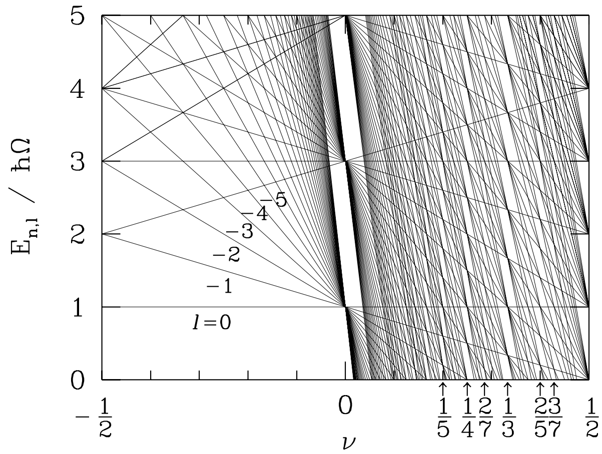

Figure 1: The energy spectrum (2) of a cranked two-dimensional harmonic oscillator, as a function of ν defined in (3). The formation of the Landau levels can be seen clearly at $\nu = 0$.

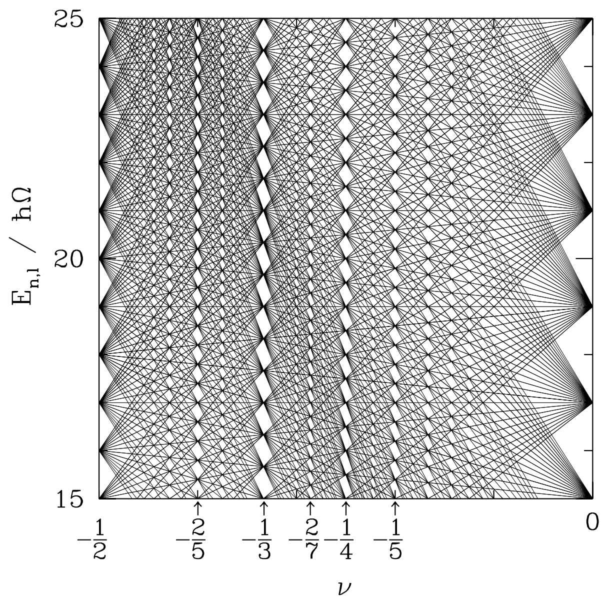

Figure 2: The same spectrum as Fig. 1, in the range $-1/2 < \nu < 0$ for higher excitation energies.

We illustrate in Fig. 2 the shell gaps for $-1/2 \leq \nu < 0$ for higher energies. The quantum gaps are formed when ω/Ω is a rational number, a fact that links these gaps to closed classical trajectories. This may be seen easily in the solutions of the classical equations of motion. Using the variable $z = x + iy$, the equations of motion can be written compactly as

$$\ddot{z} = (\omega^2 - \Omega^2) z + 2i\omega\dot{z}\,. \tag{4}$$

The first term on the right is an attractive harmonic force for $\omega < \Omega$, but becomes repulsive beyond the Landau level limit. The general solution of (4) is

$$z = Ae^{i(\omega-\Omega)t} + Be^{i(\omega+\Omega)t}, \tag{5}$$

where A and B are constants. The normal mode frequencies are $|\Omega - \omega|$ and $(\Omega + \omega)$, and a periodic orbit is obtained when the ratio of these frequencies is a rational fraction. Some of these periodic orbits are shown in Fig. 3 for various values of ν. When $\omega = \Omega$, the solutions of (4) are given by circles in the xy plane with arbitrary centres. Apart from this special case, the most prominent gaps occur in Figs. 1 and 2 for the simplest fractions. For example, the large gaps at $\nu = \pm 1/3$ correspond to the situation when one normal mode frequency is twice the other.

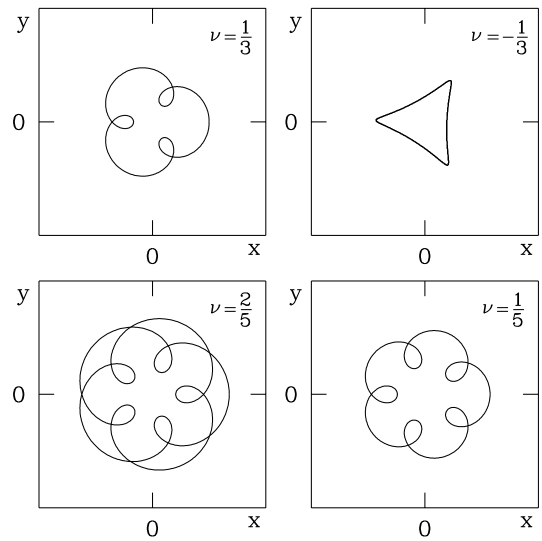

Figure 3: The classical periodic orbits of a particle obeying (5) for various rational values of ν.

Now we study a series of new quantum gaps that are generated when $\omega > \Omega$, i.e., $\nu > 0$. There are intriguing aspects in the level structure, particularly regarding the lifting of the degeneracy of the Landau levels. For convenience, let us study the states converging at the energy of the lowest Landau level, $E = \hbar\Omega$. At this energy, at $\nu = 0$, the zero node states [$n_r = 0$; see Eq. (2)] with the largest negative angular momentum values from each shell converge. The resulting degeneracy per unit area is found to be $\eta_0 = 2M\Omega/h$, which equals eB/hc if $\Omega = \omega_c/2$.

Now let us proceed to examine, in Fig. 1, the level degeneracies for $\nu > 0$. The repeating pattern in this region is known as a Farey fan,[1] and has been studied in the context of number theory and continued fractions. At the energy $E = \hbar\Omega$, inspection of the level at $\nu = 1/m$ (m an integer > 1) reveals that the number of converging single-particle states is exactly a fraction $1/m$ of the Landau level. For example, the successive harmonic oscillator states meeting at $\nu = 1/3$ at $E = \hbar\Omega$ have angular momenta $l = 0, -3, -6$, etc. in units of $\hbar$ (see Fig. 4). Thus, for every triplet of adjacent states in the lowest Landau level, e.g., $(0, -1, -2)$, there is one ($l = 0$ in this case) at $\nu = 1/3$. Similarly, at $\tilde{\omega}/2\Omega = 1/5$, the degeneracy/area is $\eta_0/5$. The collapsed single-particle states in a shell at $\nu = 1/m$ are each from a separate Landau level and have increasing number of nodes. For example, at $\nu = 1/5$, the $l = 0$ state has no node, the next state with $l = -5$ has one node, the $l = -10$ state has two nodes, and so on.

We may term the condensed levels at $\nu = 1/m$ as "mothers", since these gaps rise to a succession of "daughters" as seen in Fig. 4. One state from each shell of the $\nu = 1/3$ mother converges at $E = \hbar\Omega$, constituting the daughter at 2/5, just as the mother herself was formed from the collapse of the states from each Landau level. The Landau level, in turn, was formed by the collapse of the states from the separate oscillator shells. The single-particle states converging at the daughters have different nodal structures than the

[1] M. D. McIlroy, Proc. Symp. Appl. Math. **46**, 105 (1992) (ed. by S. A. Burr); J. C. Lagarias, *ibid.*, 35

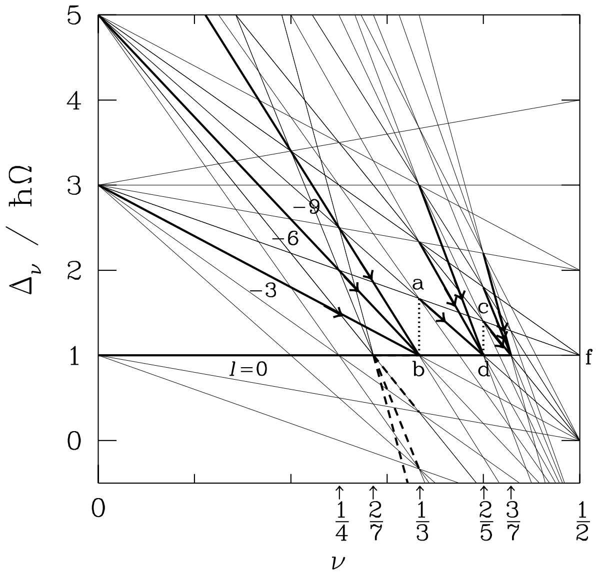

Figure 4: The "mother-daughter" sequence of degenerate levels. One state from each Landau level converges at $\nu = 1/3$ to form a mother. Similarly, one state from each shell at 1/3 converges at 2/5. Only a few converging lines are shown for clarity. In the triangle abf, the vertical lines ab and cd show the gaps at $\nu = 1/3$ and 2/5 respectively.

mothers. For example, consider the daughters at $\nu = 2/5$ and 3/7 at $E = \hbar\Omega$ that belong to the mother at $\nu = 1/3$. From (2), all the converging states at this energy obey the equation

$$(2n_r + |l|)\,\hbar\Omega + l\hbar\omega = 0\,. \tag{6}$$

It immediately follows that the converging states at $\nu = 2/5$ have $n_r = 2$ for $l = -5$, $n_r = 4$ for $l = -10$, etc. Similarly, for $\nu = 3/7$, $n_r = 3$ for $l = -7$, $n_r = 6$ for $l = -14$, etc. A similar construction could be made with even-denominator mothers, but then the daughters have alternately even and odd denominators. The complexity of the structure in the quantum states is reflected in the classical periodic orbits also, some of which are shown in Fig. 3. From this figure, note that the number of loops in the orbit is determined by the denominator q in $\nu = p/q$. For example, both $\nu = 1/5$ and $\nu = 2/5$ have periodic orbits with five loops, but the $\nu = 2/5$ orbit has a more complicated structure. The denominator q of $\nu = p/q$ also determines the magnitude of the quantum gap, as is apparent from Fig. 4. Denoting this gap by Δ_ν, we see that at $\nu = p/q$,

$$\Delta_\nu = \frac{h\omega_c}{q}\,, \tag{7}$$

where $\hbar\omega_c = 2\hbar\Omega$ is the gap at the Landau levels.

Finally, the sequence of the gaps generated by this simple dynamical model for $\nu > 0$ are the same, for the odd denominators, as the Haldane hierarchy[2] in the fractional

[2]F. D. M. Haldane, Phys. Rev. Lett. **51**, 605 (1983)

quantum Hall effect.[3] The hole-state sequences for the odd denominators in this hierarchy, e.g., $(\frac{2}{7}, \frac{3}{11}, \frac{4}{15}, \ldots)$ are generated in our model by the convergence of lines from the lower side of the $\Delta_\nu = \hbar\Omega$ line (see Fig. 4). The FQHE states, however, have a very different structure than the ones obtained in this model. In FQHE, the single-particle states of the lowest Landau level get thoroughly mixed by the Coulomb interaction between electrons, and have a highly correlated wave function of an incompressible quantum fluid.[4] There is little mixing of states from different Landau levels in such a state. By contrast, the wave function generated by our model has thorough inter-Landau level mixing and has no two-body correlations. Nevertheless, it is interesting that a sequence of quantum gaps resembling the Haldane ones may be produced from a single-particle model that is integrable.

Following this work, studying changes in the shell structure of a finite fermion system under perturbation and understanding them in terms of classical trajectories became the theme of my thesis. Stimulated by the fact that a single magnetic flux line added in the Hénon-Heiles potential drastically changes its supershell structure [61], Rajatda raised the question as to what happens to the electronic supershells of simple metal clusters[5] if a uniform magnetic field is applied. Using a spherical cavity as the mean field, the beating pattern of the shell structure of this system can be understood as due to an interference among the shortest periodic orbits of electrons.[6] This question was very interesting, since the classical dynamics of electrons are changed dramatically in a magnetic field. As it turned out, contrary to our anticipation, there is little perceptible change in the shell structure for realistic field strengths. As the field is increased yet further, however, the supershells get destroyed and a series of new beating patterns emerge.[7] Up to some field strength, this can be understood in terms of linear perturbation by the field, so that the field has similar effects as cranking. Semiclassically, the robustness of the original supershell structure and the formation of the new supershells can be explained by a trace formula for broken symmetry.[8] In Fig. 5 we show some examples of the oscillating part of the density of states (smoothed by a Gaussian of width γ); for various values of the scaled field strength $\kappa = (R/l_0)^2$, where R is the cavity radius and $l_0 = \sqrt{\hbar c/eB}$ the magnetic length. The quantum and semiclassical results are plotted in solid and dotted curves, respectively. At $\kappa = 2$ we see supershell structure which is different from the original one and the excellent agreement between the quantum and semiclassical results, while as κ increases, the semiclassical result starts deviating at lower energies. The arrows in the figure indicate a "confidence limit" as the value of kR, above which the cyclotron radius is larger than $5R$.

The most important thing I have learned from Rajatda during my Ph.D. years is to be open-minded and brave to tackle new kinds of physics and to look for common physical aspects among different subjects. Another thing he taught me is that eating (raw) green chili and drinking Scotch are important for physics and great ideas (as he has proved him-

[3] D. C. Tsui, H. L. Störmer and A. C. Gossard, Phys. Rev. Lett. **48**, 1559 (1982); R. R. Du, H. L. Störmer, D. C. Tsui, L. N. Pfeiffer and K. W. West, Phys. Rev. Lett. **70**, 2944 (1993)

[4] R. B. Laughlin, Phys. Rev. Lett. **50**, 1395 (1983)

[5] W. A. de Heer, Rev. Mod. Phys. **65**, 611 (1993); M. Brack, *ibid.*, 677

[6] R. Balian and C. Bloch, Ann. Phys. (N.Y.) **69**, 76 (1972)

[7] K. Tanaka, S. C. Creagh, and M. Brack, Phys. Rev. **B 53**, 16050 (1996)

[8] S. C. Creagh, Ann. Phys. (N.Y.) **248**, 60 (1996)

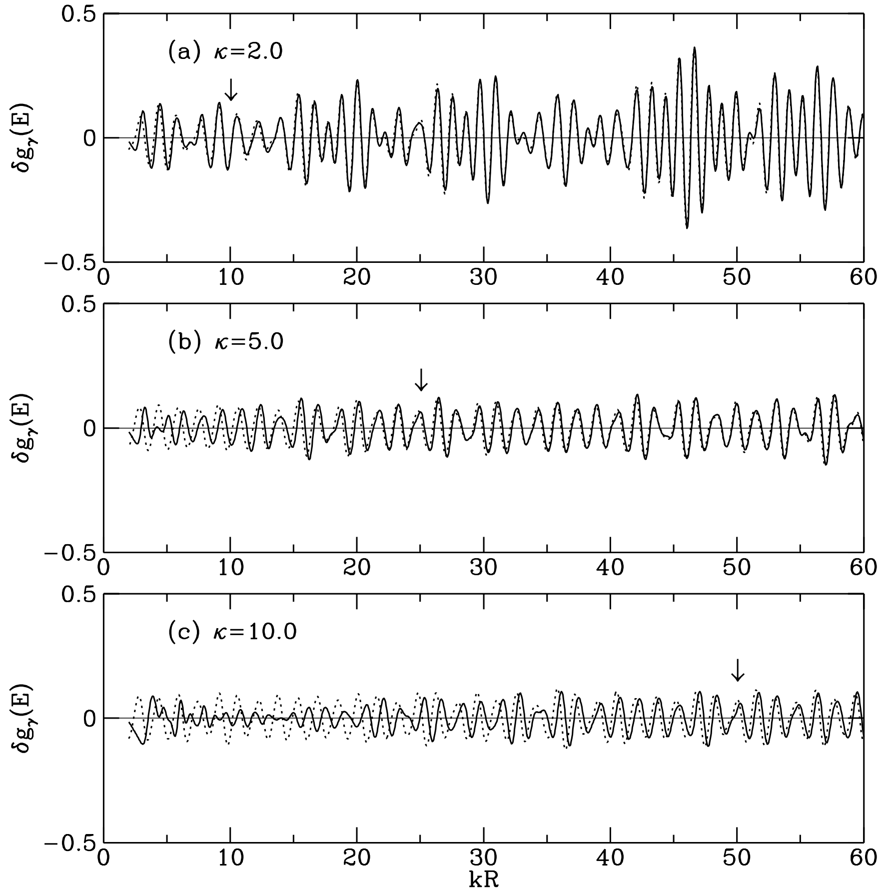

Figure 5: Comparison of the quantum (solid curves) and semiclassical (dotted curves) oscillating part of the density of states, $\delta g_\gamma(E)$, as a function of $kR = \sqrt{E}$ for (a) $\kappa = 2.0$, (b) $\kappa = 5.0$, (c) $\kappa = 10.0$. The smoothing width γ is $0.4\sqrt{E}$.

self!). Well, I must admit that I have not worked very hard on these things yet, but I am planning to start trying them soon. He also introduced me to real Indian cooking, and along with Manjudi, he taught me how to make various kinds of curry dishes (and I always "enjoyed" cutting onions). Moreover, I am grateful to Rajatda, Manjudi, Ronnie, Ranju, Tukun (Mallika), and Sharmila for giving me nice times at their house.

I wish Rajatda and his family all the best, and I am sure that he will enjoy even more and more physics from now on.

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17 THE RIEMANN CONJECTURE AND BOSE CONDENSATES

While I am writing my contribution to Rajatda's "Birthday Book", he is on his way home to Canada – probably somewhere above Greenland (and I bet, in-flight working hard on bosons!). How happy I was that I could convince him to visit me so far north in central Finland. We spent a wonderful week together, and I already miss his company.

When both Andreas and I picked him up from the airport, even after more than 24h of travel he was all fresh and full of energy. We went home for dinner and naturally ended up discussing physics until long after midnight - Rajatda didn't show any signs of jet-lag after the long travel, but his well-known appetite for new adventures in physics. The first day in Jyväskylä, Rajatda suggested to cook chicken curry for us (see the back cover), and I realized that everything was very well-planned: he had a big bag with spices in his suitcase which he brought from India only a week before he came to Finland. The curry was wonderful, and the Finnish Sauna later on provided the right tropical climate. In the following days we soon found out that what we both liked best after work was to go home in the evenings, have a Scotch and then cook various Indian meals with Finnish ingredients. (Of course I did cut the onions. But I soon realized that the biggest advantage of not being a Ph.D. student any longer was that I didn't *have to.*[1]) Over a Scotch or sometimes a bottle of wine, we shared many thoughts.

My first impression of Rajatda goes back to 1990, when I was a student at the University of Regensburg and following a course on nuclear physics, held by Matthias. The syllabus included parts of the book by Preston and Bhaduri, as well as Bohr and Mottelson's "Nuclear Structure". I still remember how much I studied those books. I could never have imagined at that time that I would later on be so fortunate to collaborate with Rajatda, Matthias, and Ben. After this term, I began my "Masters" and joined Matthias' group. Not only that he had impressed me in his lectures, I also knew him as a brilliant pianist. From sharing the fun in physics (and music), friendship originated, and the same happened later in my relation to Rajatda. Towards the end of my graduate studies, Matthias decided to send me to Rajatda at McMaster for a couple of weeks, before I would finish writing my thesis. How excited I was: I should go and work with the man whose book I had studied so thoroughly. Above all, it was my first trip to the American continent. Rajatda received me at the airport, and we immediately got along very well. I still remember that very first evening: he took me to his home, and for the first time I had Indian chicken curry.

Rajatda excitedly told me about the Riemann zeta function which he had been working on with Avinash and Jimmy [60]. The zeta function, originally introduced by Euler, knows all about the prime numbers, and is written in the simple form of a Dirichlet series, $\zeta(t) = \sum_{k=1}^{\infty} 1/k^t$ with integer k and real $t > 1$, or equivalently, as a product $\zeta(t) = \prod_{p\in\mathcal{P}} 1/(1-p^{-t})$ over the primes $p = 2, 3, 5, 7, ... \in \mathcal{P}$. In his treatise "*Über die Anzahl der Primzahlen unter einer gegebenen Größe*",[2] which was a milestone in number theory, Riemann analytically continued this function to the complex plane.[3] Defining $s = \sigma + it$ with real σ and t, the Riemann zeta function $\zeta(s)$ has "trivial" zeros at negative

[1] cf. Matthias', Jonny's and Kaori's contributions

[2] B. Riemann: *Gesammelte Werke* (Teubner, Leipzig, 1892; reprinted by Dover Books, New York, 1953)

[3] H. M. Edwards: *Riemann's zeta function* (Academic Press, New York, 1974)

integers for $\sigma < 0$ and is non-zero for $\sigma > 1$. Riemann conjectured that inside a "critical strip" $0 < \Re(s) < 1$ *all* non-trivial complex zeros can be found on the half-line $\sigma = 1/2$, and that there are infinitely many of them. The latter part of the conjecture was proved by Hardy[4] already in 1914, while for its first part, there is only numerical evidence and a proof is still lacking. Along the line $\sigma = 1/2$, the phase angle $\theta_\sigma(t)$ of the zeta function for a given σ, defined by the polar form $\zeta(\sigma + it) =\mid \zeta(s) \mid \exp(-i\theta_\sigma(t))$ along the imaginary axis t for fixed $\sigma = 1/2$, is a smooth function unless $\zeta(s)$ changes sign: it then shows a discontinuous jump by π. Off the 1/2 axis, the zeta function memorizes its zeros, but this memory is fading away with increasing distance to the 1/2 axis. Like Fermat's theorem (to which it actually is connected), the Riemann conjecture is one of the holy grails for a mathematician. Armed with much physical insight, Rajatda, Avinash and Jimmy did not hesitate to challenge the "queen of number theory": they could link the smooth phase of the zeta function to the quantum scattering phase shift of a one-dimensional inverted harmonic oscillator [60]. Rajat passed his excitement to me, and I soon found myself pondering about the prime numbers and getting lost in unknown territory. It can be really dangerous when physicists catch the math bug: we were so much infected that we did not even hesitate to do thermodynamics with the Riemann zeros! But we had lots of fun and thrill – and, as Rajatda keeps telling me, that is what counts. We started to examine the phase of $\zeta(s)$ more closely and found that its derivative with respect to t for fixed $\sigma > 1/2$ is just the Lorentz-smoothed oscillating part $\delta g_\sigma(t) = g_\sigma(t) - \widetilde{g}_\sigma(t)$ of the density of the Riemann zeros, $g(t) = \sum_n \delta(t - t_n)$, at the half line: it contains all the information about the zeros. For $\sigma = 1/2$ and assuming the Riemann conjecture that the only zeros are at $1/2 + t_n$,

$$-\frac{1}{\pi}\frac{d\theta}{dt} = \delta g(t) = \sum_n \delta(t - t_n) - \frac{1}{2\pi}\ln\left(\frac{t}{2\pi}\right). \tag{1}$$

This discontinuous function gets smoothed by Lorentzians of widths $(\sigma - 1/2)$ as the derivative of the phase is computed along the imaginary axis at $\sigma > 1/2$. The Lorentz-smoothed density is expressed as

$$g_\sigma(t) = \frac{\gamma}{\pi}\sum_n \frac{1}{(t - t_n)^2 + \gamma^2} \tag{2}$$

with a smooth part

$$\widetilde{g}_\sigma(t) = \frac{1}{2\pi}\ln\left[\frac{1}{2\pi}\left((\sigma - \frac{1}{2})^2 + t^2\right)^{1/2}\right]. \tag{3}$$

The density of states of the Riemann zeros was thoroughly examined by Berry[5] who wrote it in terms of a (convergent) Gutzwiller trace formula, with one primitive periodic orbit for each prime number, having a classical action that is proportional to the logarithm of the prime $p \in \mathcal{P}$

$$\delta g_\sigma(t) = -\frac{1}{\pi}\sum_{k,p}\frac{\ln p}{p^{k\sigma}}\cos(kt\ln p). \tag{4}$$

(We set $\hbar = 1$ and identify t with the energy variable.) Far away from the half line, only the "shortest paths" are important and the contributions of larger primes get severely

[4] G. H. Hardy, Comptes Rendus CLVIII, 280 (1914)

[5] M. V. Berry, Proc. Roy. Soc. London Ser. A **400**, 229 (1985)

damped. This is illustrated in Figure 1 below: it shows the derivative of the phase of the zeta function at $\sigma = 2$ as an example. On the basis of Eq. 4 one is tempted to speculate that the zeros s_n behave as eigenvalues of a dynamical Hamiltonian.[6] For very large σ, unstable harmonic motion in one direction, perched on the edge of an inverted harmonic oscillator in the transverse direction, serves as a caricature of the Riemann zeta function [67]. The classical motion perpendicular to the saddle has only one isolated, unstable periodic orbit and its repetitions. Clearly, such a toy model does not correspond to any dynamical system that could be a candidate for describing the phase of the zeta function even outside the critical strip. Focusing on the semiclassical density of states and its asymptotic connection to the Riemann zeta function, however, one notices that for very large distances from the half line, the curvature of the inverted potential at the saddle is directly proportional to σ.

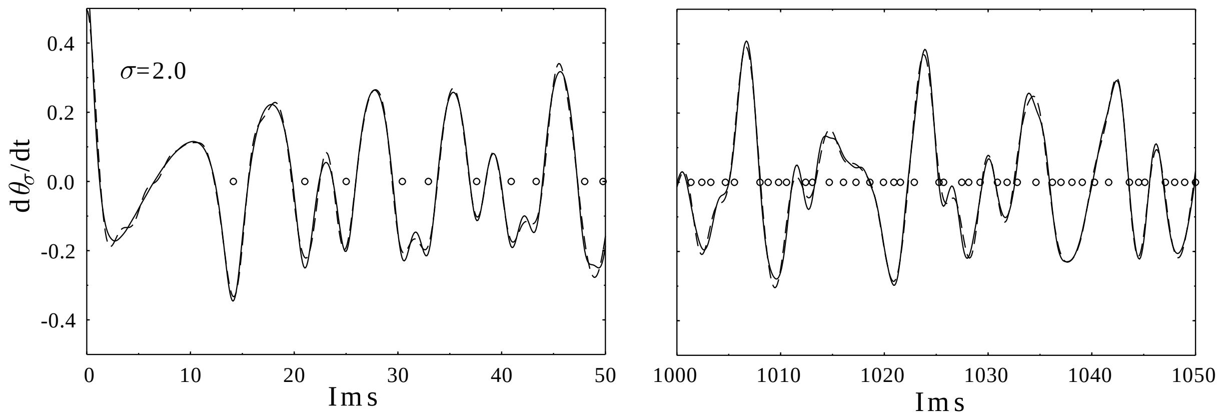

Figure 1: Derivative of the phase of the zeta function $d\theta_\sigma/dt$ at $\sigma = 2$, calculated by using the first 10 primes and $k_{max} = 10$ (solid line), and the first 100 primes, $k_{max} = 100$ (dashed line).

Rajatda's basic curiosity and sense of adventure is best described by telling about one of those days at McMaster, when we went down to the store in the basement of the physics department and lost our way, ending up in the chemistry section: What wonderful stuff they had there! We walked around and showed each other the most bizarre glass bulbs, bottles and tubes. Actually, we got a bit depressed that we were no chemists. What fun these guys must have in their labs! We silently returned to our desks and continued our fight with the ζ-function. Of course, many cooking events accompanied our mathematical expedition. I remember Ed being very sophisticated: he would always ask his computer for advice while cooking, having his laptop with all the recipes among a pile of chilies, garlic and other stuff. The outcome was really hot.

Being back home, instead of my working on my thesis, we decided to write a paper on our ideas about the Riemann zeros. We submitted it to Physical Review A, very soon got rejected, and I was very disappointed. Rajatda suggested to submit again to Annals

[6] M. V. Berry, in *Quantum Chaos and Statistical Nuclear Physics*, Lecture Notes in Physics 263, ed. by T. H. Seligman and H. Nishioka (Springer Verlag, Berlin, 1986), p. 1

of Physics. I, a real pessimist, exclaimed: 'I bet a bottle we get another reject!' Rajatda just loved it – he was sure the Scotch was his. And, indeed, it was [67].

When we resubmitted the paper, we realized through discussions with Poul Erik Lindelof from NBI in Copenhagen that our mathematical toy actually has its experimental counterpart in nanostructure physics: the electrostatic potential of a split-gate quantum point contact in a mesoscopic structure[7] has a similar shape than the oscillator saddle potential that we had studied in connection with the Riemann zeta function. Transmission through such a saddle potential may take place in quantized channels that correspond to the bound states of the one-dimensional harmonic potential.

It would take several years before we met again (apart from a very short visit of Rajatda in Regensburg, where he was busy writing his book with Matthias). I was then as a post-doc at NBI in Copenhagen and had turned away from semiclassics and periodic orbits to semiconductor quantum structures and their microscopic description. Many analogies between such finite-size condensed matter systems and nuclei – such as, for example, shell structure – called for bringing together nuclear and condensed matter physicists for a workshop at ECT* in Trento. Curious characters full of energy like Rajatda were exactly what we needed for a successful meeting, and I knew that he would be all in favour for such an interdisciplinary enterprise. Rajatda, Ben and I stayed another week after the workshop, in order to collect our ideas and summarize the outcome, and had a week of exciting discussions and lots of fun. The workshop had drawn some connections to the physics of Bose-Einstein condensates, which interested both Rajatda and Ben very much. For me as a green post-doc, everything was very new. The atomic condensates constitute a new playground for many-body physics. One can even make them rotate – no wonder that both Ben and Rajat love this new toy. Cooled below a critical temperature, a large fraction of the atoms of a bosonic gas condenses in the lowest quantum state as a consequence of quantum statistics.[8] The wavefunctions can overlap if at low enough temperature the de Broglie wavelength of the (indistinguishable) atoms becomes comparable to the average inter-atomic distance. At this critical temperature, the bosonic atoms form a coherent cloud of atoms with a macroscopic population of the same lowest quantum state – the Bose-Einstein condensate. As the range of the inter-atomic forces is much smaller than the de Broglie wavelength of the atoms, interactions can be modeled by an effective contact interaction $U_0\delta(\mathbf{r}-\mathbf{r}')$, with $U_0 = 4\pi\hbar^2 a/m$, where a is the s-wave scattering length. Rajatda had noticed an astounding analogy to his earlier work with Diptiman Sen [59] on a non-interacting Haldane gas[9] and speculated that interacting bosons in quasi two dimensions could be mapped on non-interacting particles obeying Haldane's fractional exclusion statistics. We then considered a dilute Bose gas in an oblate three-dimensional trap and took the quasi two-dimensional limit in which $\omega_x = \omega_y \ll \omega_z$, being well aware of the fact that the delta-function interaction in this effectively two-dimensional Hamiltonian is not to be regarded as a pseudo-potential from scattering in two dimensions, but rather as the dimensionally reduced form of a three-dimensional pseudo-potential. For temperatures above the critical temperature T_c of the interacting system, there is no condensate and $n(\mathbf{r}) = n_T(\mathbf{r})$, where n_T is the density of particles

[7]M. Büttiker, Phys. Rev. **B 41**, 7906 (1990); R. Taboryski, A. Kristensen, C. B. Sørensen and P. E. Lindelof, Phys. Rev. **B 51**, 2282 (1995)

[8]V. Bagnato, D. E. Pritchard and D. Kleppner, Phys. Rev. **A 35**, 4354 (1987)

[9]F.D.M. Haldane, Phys. Rev. Lett. **67**, 937 (1991)

occupying states other than the ground state. For $T < T_c$ one writes $n(\mathbf{r}) = n_0(\mathbf{r}) + n_T(\mathbf{r})$, with the condensate density $n_0(\mathbf{r})$. In the Thomas-Fermi approximation,

$$n(\mathbf{r}) = \int \frac{d^2p/(2\pi\hbar^2)}{\exp[(p^2/2m + m\omega^2 r^2/2 + (2\pi\hbar^2/m)gn(\mathbf{r}) - \mu)\beta] - 1}, \tag{5}$$

where $\beta = 1/(k_B T)$. The dimensionless coupling constant g of the effective two-dimensional interaction is $g = a\sqrt{2\pi}/b_z$ with the oscillator length $b_z = \sqrt{\hbar/m\omega_z}$ in $z-$direction. This equation, together with the constraint $N = \int d^2r\, n(\mathbf{r})$, can be solved self-consistently to obtain $n(\mathbf{r})$. Without interaction, $g = 0$, the condensate density n_0 is macroscopic below T_c which results in a discontinuity in the chemical potential μ at T_c (see Fig. 2).

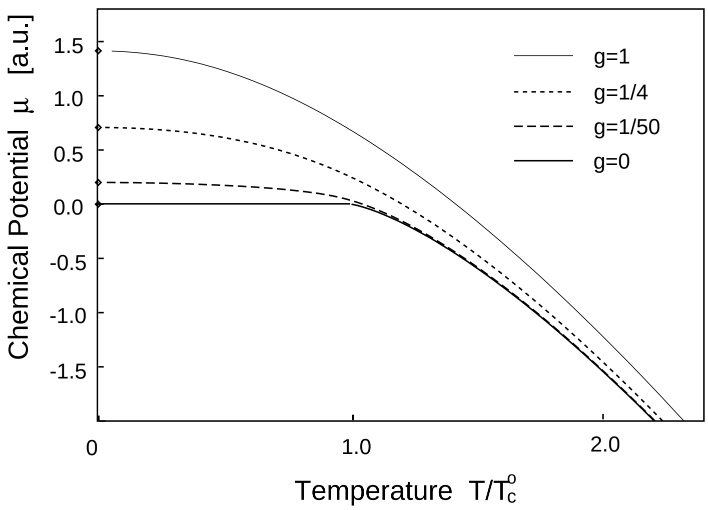

Figure 2: Chemical potential μ versus temperature T/T_c for interacting bosons in a quasi two-dimensional trap at various values of the interaction strength g, obtained from a selfconsistent solution of the Thomas-Fermi equations.

Rajatda showed me a paper[10] which claimed that for an *interacting* quasi two-dimensional system there was no selfconsistent solution of the TF equations below a certain non-zero critical temperature T_c. He was very upset, as this did not seem to fit together with the above mentioned “Haldons”, and asked me to repeat the numerics to see what is going on. It was a sunny Sunday morning in Italy, and I really did not plan to spend the whole day in the institute. But I knew Rajatda wouldn’t get any peace until the work was done: *Run, rabbit, run, dig that hole, forget the sun ...*

For non-zero positive g, the above equations can be solved all the way down to $T = 0$. We finally saw that within the finite-temperature Thomas-Fermi method, there is no strict phase transition with a repulsive zero-range two-body interaction, no matter how weak the repulsion of the particles. As the temperature is lowered the chemical potential rises smoothly, and only at zero temperature does it match the lowest energy level of the trap.

[10] W. J. Mullin, J. Low Temp. Phys. **110**, 167 (1998)

After we left Italy, we continued our work via the internet – what would we do without it. Trying to demonstrate the mapping of interacting bosons on non-interacting "Haldons", I remember night-long searches for a bug in the numerical code. Rajatda was so sure that he was right, the bug had to be on my side. And of course it was – a simple factor of two missing at a delicate point. (Later on, when Rajatda visited Jyväskylä, Susanne Viefers joined us and this derivation was done analytically.) We celebrated on the phone, immediately finished the paper and submitted it to the "Physical Review Lottery" [70]. This time, however, if we had made another bet, the bottle would have been mine.

Rajatda and I manage to regularly confuse each other but usually, some answer originates later from it. Rajatda guides me through the wonderland of physics, always drawing my attention to many beautiful sights along the way, which I otherwise certainly would have missed. I deeply admire his never-ending curiosity. Rajatda is a truly dynamical *Hamilton*-ian: he attacks a new problem that crosses his path with immense intensity and ingenuity. He then passes his excitement to anyone who is around, and he would always let us share his ideas.

I would like to take this opportunity to thank both Rajatda and Matthias for being my teachers and friends, and for always being a source of fun with doing physics.

I wish to extend my thanks to Manjudi and the whole family for always so warmly welcoming me to their home. How much I enjoy their company on all those evenings, when we come home after a long working day and have a good Indian meal, or when on some occasions I meet their friends for a big party.

Happy Birthday, Rajatda!

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INDEX