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# SOME MATHEMATICAL REMINISCENCES*

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*To Raghu, with affection and admiration.*

**1. Introduction.** There are two ways to approach what sort of contribution one should make on an occasion like this. One is to look forward, to discuss the various ways in which our two friends have influenced their subject and the people who work in it, and to indulge in some scientific speculation; the other is to look back and talk about how it all began. My choice is closer to the second. However we must realize that looking back, pleasurable as it is, is also tinged with a little melancholy because we know that the times of our youth, indelibly etched in our hearts and minds, will never return.

Varadhan and I go back more than 40 years. We graduated out of the same institution, the Presidency College which was affiliated to the University of Madras. We majored in statistics but three years apart. We both went to the Indian Statistical Institute at Calcutta for graduate studies. I joined the Institute in 1956 and he came in 1959. But by the time he came in I had already done my Ph.D and was leaving Calcutta to spend what turned out to be three years in the US. I came back in 1962 when he had finished his thesis and we worked for a year together before he went to the Courant Institute where he has remained ever since and has had an illustrious career.

I think it was in 1968 that I met Dan Stroock for the first time. I was visiting the Institute for Advanced Study at Princeton and had come to New York to touch base with Varadhan. I distinctly remember that first meeting with Dan. We were walking towards a small local cafe for lunch when Varadhan turned to Dan and said that he knew how to overcome the difficulty they had encountered, or something to that effect. I was startled to hear Dan then say exactly the same thing. I had already worked with Varadhan a few years back and knew what it was to run with him and I did not remember many occasions when I had a come back like Dan's! I looked up with a little more interest at this young man who appeared to be running well with my friend, and made a mental note that he will make a big splash in the mathematical scene. This of course was what happened and the present occasion is a celebration of their joint work as well as their individual accomplishments on the occasion of their turning 60. What makes it so nice is that they are modest even though they have done wonderful things that have been widely recognized and admired, and more importantly, have maintained their gentlemanly approach to science, something that is not as frequently encountered as one would wish, in the competitive world of science today. To modify Leo Durocher's famous remark, this is a pair of nice guys who finished first!

Everyone knows how fundamental and pioneering the Stroock–Varadhan theory of martingales and markov processes is. They did this when they were both very young; however they did not stop with this but went on to do many other beautiful

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things. I think Samuel Johnson's tribute to Oliver Goldsmith would perfectly fit this pair of gentlemen: *they touched nothing they did not adorn.*

I am by no means an expert on the mathematical themes that have energized my two friends, and so it was not clear to me at first what I could say on this occasion. Then suddenly the idea occured to me that I could tell a few things about some of the mathematical interactions I have had with Varadhan. These took place at two different times; first, in Calcutta when we were both very young and starting on our careers, and a second time, when we were both much older and well established. You will see that our interactions were over a wide mathematical scene: they touched on Lie group representations, infinite dimensional algebras, quantum theory, and, last but not least, probability theory and analysis. I hope to make you understand through these remarks why I admire him so much. His power, insight, and willingness to think about anything mathematical are admirably balanced by his patience and inexhaustible good humor when dealing with people. He is a true master and I feel it is a great good fortune of mine to have known him very closely. A great deal of what I know about doing mathematics has come from him.

**2. Graduate studies in Calcutta at the Indian Statistical Institute.** Let me begin my story with an account of what it was like to be an aspiring graduate student interested in studying mathematics in the 1950's in India. You must keep in mind that our country had become independent in 1947, just a few years back. Although in ancient times India had been prosperous and very creative in mathematics, she had entered a long period of darkness and was only slowly coming out of it in the 1950's. Although there were isolated flashes of achievements like those of Ramanujan, Raman, Saha, and a few others, there were very few places with a tradition of doing good science; moreover, there was a feeling among most people, unstated and implicit but nevertheless very real, that under those conditions, it was only by going to some place in Europe or the US that one would be able to do good work. However there were two places which were very attractive for young people to get into. One was the Tata Institute of Fundamental Research in Bombay, and the other was the Indian Statistical Institute in Calcutta. I joined the Statistical Institute in 1956 as a research fellow. A little later R. Ranga Rao and K. R. Parthasarathy also joined the Institute. Varadhan came to the Institute in 1959.

In the mid 1950's and early 1960's, the Statistical Institute was a place with an extraordinary ambiance for doing creative work in many areas. It had been founded in 1931 by P.C.Mahalanobis with the goal of developing statistical methods and techniques in an environment that was deeply concerned with actual applications. In the 1950's the need for planning the economic development of India forced the Institute to the forefront of statistical and planning activities. Mahalanobis, the director of the Institute, and C. R. Rao, the head of the research and training school of the Institute, had the vision to realize that it was essential to create and maintain a high level of scientific atmosphere at the Institute to foster the discovery and development of new ideas. The main focus of the Institute was on statistics and its many applications, mainly to economics and planning, but there was activity in many other areas such as psychometry, biometry, and genetics. There was a constant stream of visiting scientists, such as Oskar Lange, Leo Boron, Charles Bettelheim, Norbert Wiener, John Galbraith, R. A. Fisher, Jerzy Neyman, A. N. Kolmogorov, N. N. Bogolyubov, R. Vaidyanathaswamy, J. B. S. Haldane, Nathan Keyfitz, and scores of others. I think, for young students like us, this exposure to so many different areas of interest was very beneficial. It allowed us to put what we wanted to do in its proper place in the big scheme of things and gave us an overview that would prove to be crucial for our development and growth as scientists.

For those of us who wanted to work in Statistics, sample surveys, genetics, or other areas closer to applications, there were plenty of people around who were doing world class work in those subjects and so guidance was available. Many of our friends, like J. Sethuraman, Vasant Korde, G. P. Patil, went in that direction. But our small group was interested in probability theory and mathematics and there was almost no one available to guide us. S. D. Chatterji, who joined the Institute a year before me, left to work in the US a few months after I came in. G. Kallianpur, who was a central figure in the probability group at the Institute, at that time, had left the Institute to go to the US permanently. It is a tribute to C. R. Rao and Mahalanobis that we were not pressured in any way to do things we did not like to, and so we were left pretty much to ourselves. But this isolation did not bother us very much. We were a very confident bunch of youngsters and felt we did not need anyone to tell us what we should do! Because of our deep interest in probability theory we started studying measure theory and topology. The books of Halmos on measure theory, finite dimensional vector spaces and Hilbert space, the book of Kelly on set topology, and the great classics of Gnedenko and Kolmogorov (limit theorems for sums of independent variables), Cramer (mathematical methods of Statistics), Doob and Feller constituted the background for everything we did. At some point R. A. Gangolli joined our group but only for a few months; he left for MIT for further work.

Of course merely reading books and discussing them in seminars, while a very necessary activity, is hardly sufficient to take the next step, namely to do something new. The impulse for this came suddenly. In 1956 C. R. Rao had gone to Moscow for a conference on probability and information theory where he had spent some time with Kolmogorov. On his return he had told me that in Kolmogorov's view information theory was going to be a very basic field in the years to come; he had also given me a reprint that Kolomogorov had given him of a paper of Kolomogorov and Prokhorov that was presented by them in a conference in Berlin in 1954. This paper treated weak convergence of probability measures in complete separable metric spaces(polish spaces). I became quite excited and began studying the topological aspects of the space of probability measures on topological spaces, especially metric spaces and polish spaces. When Ranga Rao and Parthasarathy joined me later, I communicated to them my excitement. This intervention of C. R. Rao gave a real boost to our solitary efforts and started us on a path from which we never looked back. In the meantime, R. R. Bahadur had joined the Institute faculty. With his interest in the more mathematical side of things and his great interest in us, the atmosphere for our group improved considerably. Ranga Rao worked on uniform approximations of measures and obtained deep and far reaching generalizations to higher dimensional spaces of classical uniform approximation theorems in probability. Parthasarathy started looking into measure and information theoretic aspects of dynamical systems. We had, in some miraculous way, formed a nucleus of a modest school and were looking at real research problems of some current interest. At that time there was no other place in India except the Tata Institute of Fundamental Research where young people were doing real mathematics.

Some of my work on convergence of measures on metric spaces had much overlap with work of Prokhorov in Moscow and Le Cam at Berkeley. At about the same time Patrick Billingsley at Princeton was also doing things similar to what I had been doing. Prokhorov was the examiner for my thesis. This fact requires an explanation. Although I worked at the Institute, the Institute had not yet been given the power to grant the doctoral degree and we had to submit our theses to the Calcutta University. Indian Universities, operating on an ancient system that was based on total distrust of the candidate's supervisor, insisted on having a foreign examiner for any Ph. D thesis, and Calcutta was no exception. But surprisingly they accepted my suggestion that in my case Prokhorov should be the foreign examiner.

This was the situation when Varadhan joined the Institute as a research fellow in 1959. He had had a spectacular career in the Madras University. His total of marks for the masters degree was the highest in the history of the University. In spite of this he was very modest, willing to do anything and look at any question that people asked him. Everyone who came into contact with him was immediately impressed by his quickness and his power of analysis of problems. Haruki Morimoto, who was a statistician from Osaka, Japan, and who was a frequent visitor to the Institute, told me years later that a conversation with Varadhan at that time changed his entire perspective on a whole series of problems and led to many papers of which he is very fond of. Unfortunately I did not have much chance to interact with Varadhan in this period. I had received an offer of a postdoctoral fellowship from Princeton University and was about to leave. In the weeks and months before my departure, Ranga Rao, Parthasarathy, and myself had been running a seminar on topological groups (essentially studying Pontryagin's famous book). In view of our background and also of our being a part of the prevailing culture of the Institute, it is not surprising that that the idea came to us that it would be nice to prove limit theorems for sums of independent variables with values in general locally compact abelian groups and even Hilbert space. This problem is of course closely related to the determination of the structure of infinitely divisible distributions on those groups.

After I left for Princeton Varadhan joined this group. Their very first problem was to examine whether a typical probability measure on a metric group was decomposable as a convolution of two other non trivial probability measures. Parthasarathy has told me how one day Varadhan came in and showed them the argument that produced many indecomposable measures; building on this they then succeeded in showing that on a complete separable metric group the set of indecomposable measures was a dense $G_\delta$ in the space of probability measures on the group equipped with the weak topology, a result that surprised Bochner (to whom they had communicated this work to ask for his opinion). They then went on to work out completely the theory of infinitely divisible distributions and the associated limit theorems in all locally compact abelian groups. But the extension to *Hilbert space* proved quite difficult. This is due to the fact that infinite dimensional Hilbert space is not locally compact, and so to prove the tightness of a family of probability measures, i.e., to find compact sets whose complements have small probability uniformly in the family, is always technically difficult because compact sets are harder to locate in spaces which are not locally compact. Varadhan succeeded in extending the locally compact theory to the case of independent random variables with values in a Hilbert space. This was a substantial achievement, not only because of the infinite dimensionality of the context, but also because the techniques he used were quite original and were based on a deep use of Levy's concentration function but adapted to the context of Hilbert space. This work, done about 1962, was more or less his thesis for the Ph. D degree.

Our group was changing once again. Ranga Rao had gone to Urbana, Illinois, Parthasarathy had gone to Moscow and I myself had returned from the US. By this time the Institute itself had begun to grant the Ph. D degree. However the system of going to foreign examiners was still retained. For Varadhan's thesis the foreign examiner was Kolmogorov. His report was in Russian. Since I was one of the few people in our department who was familiar with the Russian language, the report eventually found its way to me.

I have since tried to trace a copy of that report but without success. But I still remember two sentences which stood out in that report. Kolmogorov wrote that *this thesis is not that of a student but that of a mature master... the thesis deserves the second degree in the Soviet Union*. I should remind you that the first degree in the (then) Soviet Union is called candidate's degree and is roughly the same as a Ph.D in the US, while the second degree, Doctorate of Sciences, is given only for distinguished work, usually several years after the candidate's degree. For instance, Prokhorov's famous 1956 paper was essentially his thesis for his second degree.

**3. Collaboration: representations of complex semisimple Lie groups.** As I mentioned above, Varadhan and I were now the only ones left in Calcutta from our group. We shared the same office and were discussing mathematics all the time. Varadhan tried to interest me in markov processes and gave many penetrating expositions (just for the two of us) of Feller's classification of diffusions in one dimension. It was all very beautiful but my heart was not in it. Already at the time of my leaving for the US in 1959 my interests had turned to quantum theory and unitary representations. In the US I had come into contact with Mackey and Harish–Chandra and so, by the time I came back to Calcutta, I was filled with an overwhelming desire to work in the theory of representations of Lie groups. After returning to the Institute in 1962 I gave several lectures there on foundations of quantum theory and the role of unitary representations in it. I like to think that I must have passed on to Varadhan some of my excitement about these things. Soon we agreed that we should try to get a better understanding of quantum theory and unitary representation theory.

That was the first time I worked with him and I still remember those heady days. There was no teaching to do so that we could concentrate on whatever we wanted to do completely. We literally worked all the time. We finished discussing everything I knew about quantum theory in a month(!) and then started on unitary representation theory.

I had learned representation theory from Hermann Weyl's famous books and knew that the heart of representation theory was in the semisimple groups. I knew of Harish–Chandra's monumental work but had just started to explore it. Although the deepest results of Harish–Chandra's theory were analytical, their underpinnings were algebraic; indeed, his initial papers were almost completely algebraic. The algebraic theory was very attractive to us as a beginning because it required very little preparation; it was mostly linear algebra, although in the context of infinite dimensional algebras. So I suggested to Varadhan that we take up the paper[1] where Harish–Chandra had made an algebraic construction of infinite dimensional irreducible representations, in fact essentially *all* of them, for all *real* semisimple groups, and try to see if we can understand it at least for the *complex* groups, like $SL(n, \mathbf{C})$.

[1] Trans. Amer. Math. Soc. **76**(1954)26; Collected Papers, Vol I, 450.

The attempt to look at complex groups needs a little explanation. The point is that the theory of semisimple Lie groups and their finite dimensional representations is usually done over **C**. Since the correspondence between compact groups and complex groups, discovered by Weyl, was one to one, this foundation was adequate for the compact theory also. Only in Elie Cartan's work on symmetric spaces did the real groups appear in their own right, but his work on the real forms of complex groups was very much unknown. It was only when Harish–Chandra started his quest for the theory of unitary representations of all real semisimple Lie groups that the real theory came into the foreground. In their pioneering work, Gel'fand and Naimark restricted themselves to the complex groups, and even Harish–Chandra began his work on infinite dimensional representation theory by first looking at complex groups. However Bargmann had made a deep study of $SL(2, \mathbf{R})$ and had discovered phenomena that were not present in the theory of the complex groups, such as the occurrence of irreducible summands in the regular representation, the so called *discrete series* of representations. By the time we started on our journey, Harish–Chandra had moved on immeasurably beyond, and had already constructed the entire discrete series (his announcement in the Bulletin of the American Mathematical Society **69** (1963), 117–123, has the date of communication as September 13, 1962). We knew all this but felt that our modest starting point was reasonable; what was good for Gel'fand, Naimark, and Harish–Chandra when they started out, was good for us too!

Before imposing the restriction to complex groups let me describe the framework in the context of real groups. Let $G$ be a *real* connected semisimple Lie group and let $K$ be a maximal compact subgroup of $G$; if $G = SL(n, \mathbf{C})$ or $SL(n, \mathbf{R})$ we can take $K = SU(n)$ or $SO(n, \mathbf{R})$. I shall assume that $G$ has finite center to avoid some mild irrelevancies; this hypothesis is always satisfied for complex groups. The choice of $K$ does not matter since all maximal compact subgroups are mutually conjugate by a classical result of Elie Cartan. Let $\mathfrak{g}_0$ (resp. $\mathfrak{k}_0$) be the Lie algebra of $G$ (resp. $K$) and let $\mathfrak{g}$ (resp. $\mathfrak{k}$) be its complexification. We also need in what follows the centralizer $\mathfrak{Q}$ of $\mathfrak{k}$ in the universal enveloping algebra of $\mathfrak{g}$. In a paper preceding the one we are talking about, Harish–Chandra had shown that the correspondence between representations of $G$ and those of $\mathfrak{g}$, which plays a powerful role in the theory of finite dimensional representations, extends also to the category of representations of $G$ in Banach spaces (with a slight restriction that I will not go into here). This allowed him to reduce the study of the representations of $G$ to the study of those representations of $\mathfrak{g}$ which decompose, when restricted to $\mathfrak{k}$, as a direct sum of finite dimensional irreducible representations of $\mathfrak{k}$, namely the $\mathfrak{k}$*–finite* representations of $\mathfrak{g}$, or the $(\mathfrak{g}, \mathfrak{k})$–modules for $\mathfrak{g}$. In particular the basic problem became the problem of determining all irreducible $(\mathfrak{g}, \mathfrak{k})$*–modules.*

Very soon it became clear what Harish–Chandra's strategy in his paper[1] was. He looked at all finite dimensional representations of the group and obtained a view of them based on a special way of looking at their restrictions to the maximal compact subgroup, so that the data determining them were parametrized by certain sets of integers. He then gave complex values to these parameters and obtained the infinite dimensional representations. But because he was dealing with *all real groups,* the construction was quite complicated. However, when the group was complex, things became somewhat simpler because in that case the imbedding of $K$ in $G$ could be described *entirely algebraically*. I shall explain this point a little later.

Let me now give a brief description of Harish–Chandra's approach in[1]; it was based on three fundamental results.

I. The restriction to $\mathfrak{k}$ of a $\mathfrak{k}$–finite irreducible representation $\pi$ of $\mathfrak{g}$ has the property that each irreducible of $\mathfrak{k}$ occurs with finite multiplicity. Moreover, let $V$ be the space of $\pi$, and for each irreducible equivalence class $\vartheta$ of $\mathfrak{k}$, let $V_\vartheta$ be the subspace (the so called *isotypical subspace*) which is the span of the irreducible subrepresentations in the class $\vartheta$ when $V$ is viewed as a module for $\mathfrak{k}$. Let us introduce $\mathfrak{Q}$, the centralizer of $\mathfrak{k}$ in the universal enveloping algebra of $\mathfrak{g}$ that we mentioned earlier. Then $\mathfrak{Q}$ stabilizes $V_\vartheta$ and $V_\vartheta$ is irreducible under the joint action of $\mathfrak{k}$ and $\mathfrak{Q}$. We can therefore write $V_\vartheta$ as $U_\vartheta \otimes W_\vartheta$ where $\mathfrak{k}$ and $\mathfrak{Q}$ act on $U_\vartheta$ and $W_\vartheta$ separately, and $W_\vartheta$ is irreducible under $\mathfrak{Q}$. We denote this representation of $\mathfrak{Q}$ on $W_\vartheta$ by $h_{\pi,\vartheta}$.

II. The representation $h_{\pi,\vartheta}$ determines $\pi$ up to equivalence. More precisely, if $\pi_1, \pi_2$ are two $\mathfrak{k}$–finite irreducible representations of $\mathfrak{g}$, not necessarily finite dimensional, such that both of them contain a common irreducible $\vartheta$ of $\mathfrak{k}$ upon restriction to $\mathfrak{k}$, and if the representations $h_{\pi_1,\vartheta}$ and $h_{\pi_2,\vartheta}$ are equivalent, then the representations $\pi_1$ and $\pi_2$ are equivalent[2].

III. The irreducible representations of $\mathfrak{g}$ containing a given class $\vartheta$ form (roughly speaking) a *variety* in which the finite dimensional representations containing $\vartheta$ form a *dense set in the Zariski topology.*[3]

There is no need to emphasize that these are remarkable results. Notice that according to II, the knowledge of just a *single* type $\vartheta$ together with the corresponding $h_{\pi,\vartheta}$ is enough to determine $\pi$ completely.

It was by the use of these results, especially the density theorem described informally in III above, that Harish–Chandra obtained his famous subquotient theorem, the main theorem (Theorem 4) of the paper[1], namely that all irreducible representations of $G$ are subquotients of the principal series representations. However his description of the pair of subspaces that determine the subquotient was very opaque to us. Our restriction to complex groups was based on the hope that in this case we would be able to clarify this point a little more.

I now take up the point mentioned earlier, namely, that for complex groups the reduction to $K$ of finite dimensional representations simplifies considerably. Suppose that $G$ is a *complex* group. Then its Lie algebra $\mathfrak{g}_0$ has the structure of a *complex* Lie algebra, being the Lie algebra of the real group underlying a complex group, and with respect to this complex structure, $\mathfrak{k}_0$ is a *real form* of it; it is in fact the *compact form* of $\mathfrak{g}_0$, discovered by Weyl which is the basis of his "unitarian trick". So we have $\mathfrak{g}_0 = \mathfrak{k}_0 \oplus J\mathfrak{k}_0$ where $J$ is the multiplication by $(-1)^{1/2}$ in the complex structure of $\mathfrak{g}_0$. It follows from this that the complexified pair $(\mathfrak{k}, \mathfrak{g})$ is isomorphic to the pair $(\mathfrak{g}, \mathfrak{g} \times \mathfrak{g})$ where $\mathfrak{g}$ is *diagonally* imbedded in $\mathfrak{g} \times \mathfrak{g}$. The representations of $\mathfrak{g} \times \mathfrak{g}$ that are of interest are those that decompose into direct sums of finite dimensional representations of $\mathfrak{g}$ upon restriction to $\mathfrak{g}$. The irreducible *finite dimensional* representations of $\mathfrak{g} \times \mathfrak{g}$ are (outer) tensor products of irreducible finite dimensional representations of $\mathfrak{g}$, and their restrictions to $\mathfrak{g}$ are then the usual tensor products of the representations of $\mathfrak{g}$. Another way to see this is as follows. If $\rho$ is any irreducible finite dimensional

[2]See Theorem 2 of the paper referred to in footnote 1.

[3]See Theorem 1, loc. cit. I shall speak of this as the *density theorem.*

representation of the complex group $G$, then

$$\rho = \pi \otimes \overline{\pi'}$$

where $\pi$ and $\pi'$ are *holomorphic* representations of $G$ and the bar means conjugation. By Weyl's unitarian trick the restrictions of $\pi, \pi'$ to the maximal compact subgroup continue to be irreducible and the complex conjugation disappears on restriction:

$$\rho\big|_K = \pi\big|_K \otimes \pi'\big|_K$$

Thus the restriction of irreducible representations of $G$ to $K$ comes down to the study of tensor products of finite dimensional irreducible representations of $\mathfrak{g}$. We thus have a very straightforward procedure of viewing all the finite dimensional irreducible representations of $\mathfrak{g} \times \mathfrak{g}$ in terms of their restrictions to $\mathfrak{g}$.

Suppose that $\pi$ is an infinite dimensional irreducible representation of $\mathfrak{g} \times \mathfrak{g}$ whose restriction to $\mathfrak{k}$ contains a class $\vartheta$. Clearly, the construction of $\pi$ through $h_{\pi,\vartheta}$ would become simpler if we knew that $\vartheta$ occured with multiplicity 1; in that case $h_{\pi,\vartheta}$ would be a *homomorphism* into $\mathbf{C}$. Obviously this should happen at the finite dimensional level too. From our study of Harish–Chandra's work specialized to the complex groups we began to suspect that actually a particular constituent $\vartheta$ of the tensor product always appeared with multiplicity 1 in $\pi = \pi_1 \otimes \pi_2$, and furthermore, that the associated homomorphism $h_{\pi,\vartheta}$ of $\mathfrak{Q}$ into $\mathbf{C}$ was *explicitly computable.* Indeed, it appeared that one can write down a homomorphism of $\mathfrak{Q}$ into a polynomial ring (even though $\mathfrak{Q}$ is highly nonabelian), and the homomorphism into $\mathbf{C}$ determined by a finite dimensional representation was obtained by evaluating the polynomials in the ring at the integer parameters of $\pi_1, \pi_2$ of the representation; and by making these evaluation points complex, infinite dimensional representations were to be obtained. This is the meaning to the density theorem described a little earlier. We had thus come down to the following question: if $\pi_i (i = 1, 2)$ are two finite dimensional irreducible representations of $\mathfrak{g}$, does the tensor product $\pi = \pi_1 \otimes \pi_2$ contain a canonical irreducible $\vartheta$ with multiplicity 1 such that the homomorphism $h_{\pi,\vartheta}$ of $\mathfrak{Q}$ can be written down explicitly?

Let us see what happens if $G = SL(2, \mathbf{C})$. Its finite dimensional irreducibles $\mathcal{D}_j$ are parametrized by integers $j \geq 0$ and we have the famous Clebsch–Gordan series

$$\mathcal{D}_j \otimes \mathcal{D}_{j'} = \mathcal{D}_{|j-j'|} \oplus \mathcal{D}_{|j-j'|+2} \oplus \cdots \oplus \mathcal{D}_{j+j'}$$

Thus *every* type occurs with multiplicity one. However it became clear from our study of[1] that the canonical type we wanted was $\mathcal{D}_{|j-j'|}$, the *minimal* type.

Indeed, what happens is the following: if we denote by $v_j, v_{-j'}$ the highest and *lowest* weight vectors of $\mathcal{D}_j$ and $\mathcal{D}_{j'}$ respectively, the vector $v_j \otimes v_{-j'}$ is *cyclic* for $\mathcal{D}_j \otimes \mathcal{D}_{j'}$ even though this representation is far from irreducible, and its projection on the subspace corresponding to $\mathcal{D}_{|j-j'|}$ is an *extremal* (lowest or highest depending on the sign of $j - j'$) weight vector of that representation; this fact then allows one to calculate in a natural and explicit manner the action of $\mathfrak{Q}$ on that subspace. Since $j - j'$ is a weight of *all* the types and is an extremal weight of the type $\mathcal{D}_{|j-j'|}$, we have an invariant characterization of the minimal type. Also, $j - j'$ is not necessarily nonnegative and $|j - j'|$ is the nonnegative one in the Weyl group orbit of $j - j'$; the Weyl group here is $\{\pm 1\}$ and it acts as $k \longmapsto -k$ on the weights.

If we now fix an integer $k$, the finite dimensional irreducibles with $k$ as their extreme weights correspond to a pair $(j, j')$ with $j - j' = k$. If we substitute for $(j, j')$ a pair $(\lambda, \mu)$ of complex numbers with $\lambda - \mu = k$ in the formula for $h_{\pi,|k|}$, we obtain homomorphisms of $\mathfrak{Q}$ which should come from infinite dimensional representations containing $|k|$ as a minimal type. This turns out to be the case, the representation being a *quotient* (rather than a subquotient as in Harish–Chandra's theory) of a principal series representation, thus justifying our expectations that in the complex case one could say more.

To generalize these calculations even for $SL(n, \mathbf{C})$ looked to be a formidable problem. A word here about multiplicity formulae in finite dimensional representation theory that were then known. The most famous of these were the formulae due to Kostant (for the weights) and Steinberg (for the types). But these are difficult to use as they depend on calculating partition functions which become exponentially harder to calculate; moreover they contain no geometric information.

These ideas, which took us quite some time to formulate and understand, made it clear that we were looking for a very explicit $K$–type in the decomposition of the restriction to $K$. I shall come to its complete description presently. At the moment we shall simply call a $K$–type *minimal* if its highest weight occurs as a weight in all the types entering the restriction. Clearly minimal types are unique if they exist. But we did not have a clear idea of how to describe the highest weights of the minimal types. Calculations with some cases in $SL(3, \mathbf{C})$ showed what the minimal types were in these cases. But the general case remained murky. However, as soon as a minimal type was found we could compute the action of $\mathfrak{Q}$ on the corresponding space and our method would lead us to some interesting infinite dimensional representations.

To find out whether the minimal type existed and what their highest weights were, we decided to use the Weyl character formula for the irreducibles, which for the case of $SL(n)$ that we were working with, was nothing but a ratio of two determinants, the denominator being the same for all representations. So in this method it was a question of multiplying the two determinants and expanding the product as a linear combination of some other determinants of the same type, and looking out for a particular class $\vartheta$ that occured as a minimal type, in analogy with the case of $SL(2, \mathbf{C})$.

This was clearly a combinatorial problem of some complexity and after a few days' work one morning Varadhan came to my office and told me how to determine what the minimal type was and how to show that it occurred with multiplicity 1. It was a remarkable argument. Since we knew already how to do all the other steps, our work was essentially complete. Unfortunately time had run out and he left for the Courant Institute. It was virtually impossible to continue our work after he left–you must remember that all this was in 1963, decades before electronic mail and other technological advances that have made collaborative work so much simpler these days. If my memory is correct, even photocopying equipment was very primitive; I still have my handwritten copies of certain papers that I wanted to read carefully! Of course it is easy to predict what happened to my friend when he left Calcutta; he simply went back to his great love, the theory of markov processes, and all of you know what he did with them, by himself and with Stroock.

I have always treasured that period of one year we worked together, for many reasons: it was my first collaborative effort, we were really naive and optimistic, and when we were looking at things together no mountain seemed too formidable to climb.

I would also like to point out that there were not many people anywhere at that time who understood what Harish–Chandra had done. Therefore, this adventure, which for us was completely new and thrilling, was an audacious effort, one that could be attempted only because of our scientific innocence. This is something I am very proud of and I am sure Varadhan has the same feeling about it too.

After Varadhan's departure for the Courant Institute I continued to think about the problem that we had been spending our time on and how to approach it when $G$ was an *arbitrary* complex semisimple group. Instead of the integer parameters that describe the finite dimensional representations of $SL(n, \mathbf{C})$ we now have a linear function $\lambda$ on a Cartan subalgebra which has a distinguished lattice on which a certain finite group (Weyl group) $W$ operates; this lattice is equipped with a positive cone whose elements are the highest weights of the irreducible holomorphic finite dimensional representations of $G$. The question slowly became clear in my mind: given irreducible finite dimensional representations $\pi_\lambda, \pi_\mu$ of $\mathfrak{g}$ with highest weights $\lambda, \mu$ respectively, to prove the existence of the minimal type in the decomposition of $\pi_\lambda \otimes \pi_\mu$ and determine it in terms of the highest weights of $\pi_\lambda$ and $\pi_\mu$.

A few months after Varadhan's departure, Ranga Rao and Parthasarathy returned to Calcutta. We started to discuss the problem among ourselves. We soon realized what the minimal type should be. Let $v_\lambda$ be the highest weight vector of $\pi_\lambda$ and $w_\mu$ the *lowest* weight vector of $\pi_\mu$. One knew that the weight of $w_\mu$ was $s_0\mu$ where $s_0$ is the longest element in $W$ (just like the permutation group in $SL(n)$ each element in $W$ is a product of basic reflections and the number of constituents in a minimal expression of an element as a product of the basic reflections is called the length of the element). So let us write $v'_{s_0\mu}$ in place of $w_\mu$. Then one can show that the vector

$$v_\lambda \otimes v'_{s_0\mu}$$

is *cyclic* for

$$\pi_\lambda \otimes \pi_\mu$$

and so has a nonzero projection in every isotypical subspace which generates that subspace. The weight of

$$v_\lambda \otimes v'_{s_0\mu}$$

is

$$\lambda + s_0\mu$$

The functional $\lambda + s_0\mu$ need not be in the positive cone I mentioned above but there is a unique positive element in its Weyl group orbit, which we denote by

$$[\lambda + s_0\mu]$$

When $G = SL(2, \mathbf{C})$, we have $\lambda = j, \mu = j', s_0\mu = -j'$; $j - j'$ need not be $\geq 0$ and so we have to replace it by $|j - j'|$, so that $[\lambda + s_0\mu]$ is $|j - j'|$ in this case.

We now had a well formulated conjecture: to show that

$$\pi_{[\lambda+s_0\mu]}$$

occured with multiplicity 1 in

$$\pi_\lambda \otimes \pi_\mu$$

We eventually found the general arguments to prove this and then went on to the subsequent results on infinite dimensional representations that this led to. This minimal type has since then acquired some importance in invariant differential operator theory on vector bundles. In our paper[4] we wrote : *At an early stage of the work, the last named author had the opportunity of detailed and stimulating discussions with Dr. S. R. S. Varadhan of the Courant Institute of Mathematical Sciences; he would like to express his indebtedness to Dr. Varadhan for these discussions which determined the entire approach of the present work ...* In retrospect I think this acknowledgment should have included the remark that the discovery of the minimal type and its multiplicity 1 property in the case of $SL(n, \mathbf{C})$ go back to my work with Varadhan in 1962–63; it was an omission I regretted when it turned out subsequently that this component was often referred to as the *PRV component.* I really think it should be called the *PRVV component*!

The key point in the general form of the result, already remarked on above, is that although $\pi_\lambda \otimes \pi_\mu$ is not irreducible, the vector

$$v_\lambda \otimes v_{s_0\mu}$$

is *cyclic.* If we replace $s_0$ by an arbitrary element $s$ of $W$, the vector

$$v_\lambda \otimes v_{s\mu}$$

is no longer cyclic–for instance, if we replace $s_0$ by 1, we get $v_\lambda \otimes v_\mu$ which generates the irreducible $\pi_{\lambda+\mu}$–but one may certainly consider the *cyclic submodule generated by it.* Kostant conjectured that in this cyclic module the type

$$\pi_{[\lambda+s\mu]}$$

occurs with multiplicity one. If I remember correctly Ranga Rao also had made essentially the same conjecture. The conjecture nevertheless came to be known as the PRV conjecture and was proved some years later by Shravan Kumar[5].

**4. New York and Los Angeles: Invariance principle and the finite approximations of Schwinger and Weyl to quantum mechanics.** I have already mentioned that Varadhan left for the Courant Institute in 1963. I myself went to UCLA in 1965 and our mathematical paths really diverged after that although we kept in close touch and knew what the other was doing at least roughly. I remember how he explained to me once his treatment of the problem of the heat semigroup on a Riemannian manifold for small time. He soon became the leading figure in the theory of markov processes. He was also very influential in local quantum field theory as a friend and collaborator of Symanzik who was at the Courant Institute for a number of years.

We met off and on and he used to tease me occasionally that I could not regard myself as a probabilist till I did something on markov processes. I agreed with him

[4]Ann. Math. **85** (1967), 383.
[5]Invent. Math. **93** (1988), 117.

but it was not that simple a matter to redress this deficiency! In any case, the missed opportunity (simply due to our physical separation and the subsequent divergence of our interests) of writing the minimal component paper with him was always at the back of my mind and I wanted to write at least one paper with him. However, as time went on, this appeared less and less likely. But events took a different direction and the chance to do this came up suddenly and unexpectedly.

It was around 1991 that I was visiting Trondheim in Norway, lecturing on the foundations of quantum mechanics. Trond Digernes, who had arranged the visit, mentioned to me that he would be interested in looking at some problems that were different from the problems in the theory of operator algebras that he had always been working on. I then remembered the finite models of Weyl and Schwinger as approximations to actual quantum mechanics on the line (or euclidean space). This is just what is called lattice quantum physics and has been explored deeply in quantum field theory; but in the simpler context of quantum mechanics it had been somewhat neglected. The idea consists in replacing $\mathbf{R}$ by the cyclic group $\mathbf{Z}_N$ (integers mod $N$) with $N$ elements where $N$ is large, the cyclic group being identified with a grid $X_N$ in $\mathbf{R}$, namely,

$$\{r\varepsilon \mid r = 0, \pm 1, \pm 2, \ldots, \pm (N-1)/2\} \qquad (N \text{ odd }, \varepsilon = (2\pi/N)^{1/2})$$

Here I am following the treatment and notation of Schwinger[6]. This type of approximation was familiar to Fourier himself and after him Riemann; in more recent times it has been the context for what is called *fast Fourier transform* and is a beautiful and active field. One can take the configuration space to be $X_N$ to define the position operator $q_N$ in the obvious way, the Hilbert space of the particle being

$$L^2(X_N) \simeq L^2(\mathbf{Z}_N)$$

Now one's inclination would be to replace the differential operator defining the momentum by a difference operator in the finite model. Schwinger's very original idea was to define the momentum operator $p_N$ as the *finite Fourier transform* of $q_N$, the Fourier transform being taken in the *finite* group $\mathbf{Z}_N$. It is not a local operator on the grid but more like a Fourier integral operator. In the continuum limit it becomes local. His object in doing this was to retain the symmetry fully in the finite model also. His idea then was to replace the Hamiltonian $H(q, p)$ by the *finite* Hamiltonian

$$H_N = H(q_N, p_N)$$

and see how closely the dynamics of the finite model approximates that of the continuum limit. I had always felt that a further exploration of this theme was an interesting problem and should be taken up.

I suggested to Trond that we take up these questions and try to do something with it. We started out with the harmonic oscillator and did numerical work that indicated that the Schwinger approximation was stunningly close. *For even low values of $N$ like $11, 21, 41$, the energy levels of the finite harmonic oscillator were identical with those of the actual oscillator to several decimal places at the lower end of the spectrum.* We were really blown off our feet by this and realized that it was time to prove a strong limit theorem. We tried but did not have much success and I mentioned to Varadhan that this was something he may like to look at.

[6]See *Proc. Nat. Acad. Sci. USA*, **46** (1960), 570 .

Then one day he called me from New York and said that he knew how to do it. He sent me a fax the next day containing the essence of his arguments, as well as a quick exposition of the Feynman–Kac formula (including three different ways of proving it!). He formulated the problem of justifying the Schwinger approximation as a statement that the semigroup

$$e^{-tH_N} \qquad (t > 0)$$

converged to the semigroup

$$e^{-tH} \qquad (t > 0)$$

in a strong norm, say the uniform operator norm. It turned out that the Schwinger Hamiltonian $H_N$ could be replaced for our purpose by another Hamiltonian $H'_N$, a sort of *stochastic* Hamiltonian, in whose free part the Schwinger Laplacian is replaced by the usual *stochastic* Laplacian on the lattice involving second order difference operators. This step allowed the introduction of path integral methods. Varadhan's idea then was to obtain the limit theorem by showing that the propagator $K'_N$, the kernel function of the operator $e^{-tH'_N}$ (which is an integral operator), converges to the propagator $K$ for the semigroup $e^{-tH}$, and to do this by the use of the Feynman–Kac formula applied to the random walk approximation to brownian motion in function space. Of course this approximation is classical, a case of the invariance principle; but in our case there was a twist because the propagators were functions of two points $x, y$, an initial and a final one, and so the invariance principle needed to be applied to the *conditioned processes* that are required to start at $x$ and exit at time $t$ through $y$.

Perhaps it may not be out of place to describe the situation with a little more precision. Let $x, y \in \mathbf{R}$ and $t > 0$. Let $x_N, y_N \in \varepsilon\mathbf{Z}$ vary in such a way that they converge to $x, y$ as $N \longrightarrow \infty$. Both the random walk and the brownian motion may be viewed as probability measures on the Skorokhod space $D[0,t]$ of right continuous functions with discontinuities only of the first kind. Let $P^{(N)}_{x_N,y_N,t}$ be the conditional probability measure for the random walk on $\varepsilon\mathbf{Z}$ that starts from $x_N$ at time 0 and ends at $y_N$ at time $t$. Similarly, let $P_{x,y,t}$ be the conditioned brownian measure that starts from $x$ at time 0 and ends at $y$ at time $t$. Then we have, as $x_N \to x, y_N \to y$, $\Longrightarrow$ denoting convergence of probability measures in the space $D[0,t]$,

$$P^{(N)}_{x_N,y_N,t} \Longrightarrow P_{x,y,t}$$

uniformly in $x$ and $y$ as they vary over compact sets. From this one gets the convergence of the propagators and thence the convergence

$$\mathrm{Tr}\ (e^{-tH_N}) \longrightarrow \ \mathrm{Tr}\ (e^{-tH}) \quad (N \longrightarrow \infty)$$

It is not difficult to show that this implies that $e^{-tH_N} \longrightarrow e^{-tH}$ in the weak topology of the Hilbert space of Hilbert–Schmidt operators. Since $\mathrm{Tr}\ (e^{-tH}) = ||e^{-(t/2)H}||_2^2$, we deduce that

$$||e^{-tH_N} - e^{-tH}||_2 \longrightarrow 0$$

where the suffix 2 means the Hilbert–Schmidt norm. Some simple arguments then allow us to sharpen this result to

$$||e^{-tH_N} - e^{-tH}||_1 \longrightarrow 0$$

where the suffix 1 means the trace norm, which is the natural norm for this problem. In particular we have the convergence in the *uniform operator norm,* and as a consequence, one can conclude that the spectra of the finite Hamiltonians converge in a very strong fashion to the spectrum of the actual Hamiltonian.

I wrote up the paper containing these results but we had some misgivings because Varadhan felt that the techniques would be familiar to specialists. So we added a remark to that effect as well as an additional remark that the paper has a semi–expository character. I could not have been happier. I sent the paper to my friend Moshe Flato who was an editor in the Reviews of Mathematical Physics and the paper was published there[7].

**5. Some personal remarks.** I would like to end these reminiscences on a personal note. Varadhan is of course a great mathematician but what is unusual is the affection with which he is regarded by the members of the probability community. This is undoubtedly due to his willingness to help and share his insights with those who come to him, both students and colleagues, without any condescension and always with unbounded generosity. The above story of my interactions with him is a perfect illustration of this. In all the years we have known each other there have been very few real disagreements and none that had any impact on our friendship, a friendship of which I am very proud. After my heart surgery in 1992 he came to spend a few days with me. One day while we were chatting of this and that, there was a discussion as to what I should be doing from that point on. I remember very well his advice that I should simply do what gives me pleasure. I have followed his advice since then. Sharing these reminiscences is one of the things that has given me great pleasure.

[7]Rev. Math. Phys. **6** (1994), 621.