V.S. Varadarajan

Reflections on Quanta, Symmetries, and Supersymmetries

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In memory of Mackey and Harish-Chandra

Contents

Preface

The essays in this book collect some of the lectures I have given at various places. Although the lectures ranged over a wide array of topics, they had a single theme which reflected my deep interest in the role of symmetry and supersymmetry in quantum theory. As such they are mathematical as well as personal.

Symmetry and supersymmetry, especially in quantum theory, are expressed in the language of the theory of unitary representations. This is a subject of great intrinsic beauty and enters other parts of mathematics at a very deep level. Two of the greatest figures in its history are *Mackey* and *Harish-Chandra*. Their work (to use the words of Weyl) affords shade to large parts of present day mathematics and high energy physics. It is to their memory that this volume is lovingly dedicated.

The essays should perhaps be viewed like a stroll through a garden of ideas: quantum algebras, super geometry, unitary supersymmetries, differential equations, non-archimedean physics, are a few of the topics encountered along the way. My own mathematical education evolved out of interactions with Mackey and Harish-Chandra, and I conclude this volume with brief portraits of their work, embedded in the context of personal reminiscences.

Quite a large number of people have had a hand in shaping my views expressed in these essays. In the essays themselves I have made an effort to mention some of them. But I must acknowledge my deep gratitude here to my longtime friend and collaborator Don Babbitt for the hundreds of discussions on the topics discussed here, spread literally over a lifetime.

I am also indebted to Don Blasius, who, despite many demands, personal as well as professional, on his time, took a deep interest in these essays and made many suggestions for improvement. A first draft of a substantial part

of this book was written while my wife Veda and I were vacationing in the Dordogne, living in Don's house in May of 2009. The delights of rural France were a big inspiration for me and so I should thank Don and Peter, as well as Barry and Julie (not to mention Sukie!), for making life so pleasant in Chez Blasius.

Finally, I wish to thank Ann Kostant for the help and encouragement she has given me throughout this enterprise. It was her enthusiastic acceptance of my suggestion, made in the Spring of 2009, that Springer publish a book of essays collecting together some of the lectures I have given at various places, that was the genesis of this book. I really cannot thank her enough.

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Chapter 1
Prologue

The world is built with a great harmony but not always in
the form which we expect before unveiling it.
Goro Shimura

Some thoughts on reality and its description, as well as some personal recollections.

1.1 Reality and its description

I have always admired the profound insight behind the remark of Shimura quoted above. He was actually referring to the world of mathematics[1] but I have modified his remark to mean the physical world since it makes even more sense in that context. The essays that follow, which are essentially reworkings of lectures I had given at various places, give, among other things, a highly personal view of the quantum world that has evolved in the twentieth century as an offspring of the genius of many great physicists and mathematicians. My discussions are neither complete nor entirely objective, but there are aspects to them that I feel are rather compelling. This prologue is something like an introductory discussion that sets the stage for what is to come and gives a preview of the themes to be explored. I have mixed it with some personal reminiscences of how these ideas got sorted out in my own mind.

It is generally agreed that it was *Galileo Galilei* who first insisted that the description of physical reality must be in the language of mathematics. However, the phrase *physical reality* in the above statement should not be taken in any absolute sense; I use the phrase to mean just what we are able to perceive *under current circumstances*. Thus it certainly has a provisional character, with a more comprehensive meaning to us than to Galileo. Based on

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our past experience it appears that physical reality is *layered*; as our abilities to make observations improve, we peel off the existing layers to peer inside the new layer and try to come to an understanding of it. The new perceptions often force us to invent new mathematics to describe them, as well as new ways of setting up the dictionary between mathematics and phenomena, which may be completely different from the earlier ones.

I want to emphasize that this is not a question of setting up an axiomatic scheme and showing that the new theories are just natural consequences of these axioms. Indeed there is "no functor in the sky" that will reveal the secrets of the physical world to the mathematician. Rather, it is the creation of a new way of interpreting phenomena that is mirrored in the mathematical models we propose. These models seem perfect till we penetrate to the next layer, when their inadequacy is revealed, and we are confronted with new problems and new dilemmas.

Nothing illustrates this better than the transition from classical to quantum mechanics which is one of the most dramatic in the entire history of science. Many serious mathematicians and physicists believed, at the turn of the twentieth century, that the task of the natural philosopher was finished, and all that remained was the computation of the fundamental constants of nature to ever greater accuracy. But in just a short time, spectroscopic observations overwhelmed the ability of classical electromagnetic theory to explain and predict them. Indeed, according to the classical electromagnetic theory applied to the model where the electrons are circling around a nucleus, the moving electrons will radiate and continually lose energy; this would imply that the spectral lines would be continually shifting, and ultimately the electrons will fall into the nucleus. This is in stark contradiction to what is observed: *the existence of sharp spectral lines*. Thus the stability of matter could not be accounted for on the basis of classical electromagnetic theory. Clearly, deeper explanations were required. A great step was taken by Niels Bohr when he created what is now called the *Bohr atom*. For a certain period of time it was enough to work with this hybrid model where the classical structure was buttressed by a set of rules (*quantum conditions*) that took into account the new phenomena. After spectacular initial successes discrepancies began to appear and cloud the picture, as new phenomena could no longer be explained except by making artificial and *ad hoc* modifications of the theory of Bohr. Eventually it was recognized that new principles had to be introduced in describing atomic phenomena. The first step in this transition, which was indeed a gigantic one, namely the creation of a fundamentally new mechanics, was taken by Heisenberg. He started with the basic principle that the new mechanics should be based on only quantities that are physically observed. In the case of atomic transitions this meant to him that each physical quantity should be represented by an *infinite matrix* indexed by the energy levels E_a whose entries x_{ab} are the complex amplitudes associated with the transition from E_a to E_b. In an absolutely mysterious manner he realized that the *algebra* of matrices entered into the picture because he stipulated that if X is the matrix associated with the physical quantity x, then X^n is the matrix corresponding to the quantity $x^n (n = 1, 2, \ldots)$.

The idea that matrices represented physical quantities, with its mystic origins in Heisenberg's mind, introduced a strange new complication in the mathematics because of the fact that *matrix multiplication is non commutative*. Indeed, Heisenberg believed at first that this was a weakness of his theory. But it was Dirac who understood that instead of being a weakness it was the *key to the description of the quantum world*. To Dirac the set of physical quantities, which is represented in classical mechanics by an algebra of functions, and hence a commutative algebra, is represented in quantum mechanics by a non commutative algebra, such as an algebra of matrices. Dirac called this algebra the *quantum algebra*. He made the further remarkable discovery that in the quantum algebra the commutator $ab - ba$ of two physical quantities a, b, really corresponded to the classical Poisson Bracket of the quantities interpreted classically, although there would be elements of the quantum algebra which had no classical analogs, such as spin. The structure of the quantum algebra depended on $\hbar$, Planck's constant, in such a way that when $\hbar$ could be neglected, the quantum algebra became commutative.

It must be noticed that the change in point of view from Heisenberg to Dirac, is dramatic and yet subtle. The Heisenberg matrices are very concrete and tied very intimately to very physical aspects: atomic transitions, energy levels, transition amplitudes, and so on. The Dirac algebra is very abstract, with a structure dependent on $\hbar$ that becomes commutative when $\hbar \to 0$. This extra abstraction became so characteristic of Dirac's entire approach to the description of Nature that it eventually acquired a special name: the *Dirac mode*.

If one looks back at the description of the pre quantum world, and compares it with the new ideas that were needed to describe the quantum world, it becomes clear that what had happened was a revolution in thought, in the way we describe the physical world, and in the dictionary we set up between the abstract concepts and concrete aspects of the world. This is what I meant when I remarked earlier that we had penetrated to a new layer of reality.

The fact that in the quantum description old pictures based on spacetime such as electron orbits were thrown out made the connection between reality and the mathematics that described it very remote from intuition and experience. The consistent physical interpretation of the mathematical schemes presented deep problems, many of which were philosophical and epistemological. Bohr and Heisenberg contributed enormously to this aspect of quantum theory. With characteristic brilliance, Heisenberg realized that there are truly fundamental reasons why the orbits are unobservable: classical physics puts severe restrictions on the precision with which we can observe *both* the position and the momentum of an electron. This is because the process of measurement of the position, which typically involves a collision with a photon, introduces uncontrollable changes in the momentum of the electron. Heisenberg's analysis involved his famous thought experiment using a gamma ray microscope. He then elevated this argument into a far-reaching principle: *we cannot separate the observer from the phenomena that are being observed so that measurements of atomic systems introduce uncontrollable disturbances to them.* His famous *uncertainty principle* was a quantification of this qualitative assumption. Bohr's contribution was to emphasize the notion of *complementarity*, most clearly illustrated by the *wave-particle duality of all matter*. The difficulties of constructing a consistent theory of quantum measurement were analyzed brilliantly by von Neumann whose work revealed the *thermodynamic nature of quantum measurement*.

The state of our description of the world of elementary particles and their interactions is another illustration of what I have said, perhaps even better, since it is still unfinished and beset with problems. In the quantum mechanics of Heisenberg, Dirac, Schrödinger, Pauli, and others, there was no possibility of deriving the Bohr transition rules with (what Weyl calls) the *magic formula*

$$E_a - E_b = \hbar \nu_{ab},$$

nor could one derive the Planck radiation formula which started the quantum revolution. The reason for this is easy to understand: these are features that result from the interaction of the atom (matter) with the electromagnetic field (radiation). To derive these one has therefore to set up a scheme in which *both* the atom and the radiation field are treated quantum mechanically. This was at an entirely new level because the *radiation field is already an infinite* *dimensional system classically*. Dirac was the first person to ascend to this new level, and his treatment obtained not only all these formulae but also a resolution of one of the most vexing problems of classical physics, namely the *reconciliation of the wave and particle aspects of light*. In Dirac's theory, the classical radiation field, which was a wave field, acquired particle properties on quantization. Attempts to generalize the Dirac theory into a systematic theory of quantum fields which combine quantum theory with special relativity then encountered new problems, such as divergences. Eventually many of these difficulties (but not all) were resolved. Even quantum electrodynamics, as currently understood, which is regarded as the most accurate physical theory ever constructed, is not fully acceptable to some because of the *renormalization rules* which are just recipes for hiding ugly divergences in the mathematical models that are used. People like Dirac never accepted them and one cannot entirely give up the feeling that the current status of this and other even less accurate theories is similar to the hybrid theory of the Bohr atom with its quantum rules. The standard model does summarize what is known accurately but its esthetic ungainliness appears to suggest strongly that it is provisional, and that new phenomena (perhaps coming out of the new collider at CERN) might be strong enough to suggest more radical models which are less divergent.

The string theorists try to solve all the problems by a pure intellectual effort, akin to what Dirac did when his equation predicted anti-matter, or Einstein did when he invented his theory of gravitation. Only the future can tell if string theory will join these two as a decisive way, free of singularities, of looking at elementary particles and their fields, which is as beautiful and elegant as these two theories.

Quantum theory inaugurated a striking departure in the way things are described. Before the quantum era, mathematical descriptions of physical phenomena followed ordinary experience and there was nothing mysterious about it. But quantum theory changed this in a dramatic fashion. The fact that the phenomena to be described were remote from ordinary experience forced one to invent completely new mathematical schemes that were different from anything that preceded them. In basic nonrelativistic quantum theory one needs Hilbert spaces, self-adjoint operators and their spectral resolutions, quantizations, spin structures, and so on. Later on, when quantum mechanics was combined with special relativity, the framework was enlarged to admit spacetime symmetry groups and their unitary representations. Quantum electrodynamics brought its own characteristic features: Fock space, creation and annihilation operators, and as a culminating flourish, the concept of a *quantized field* and renormalization rules. The singularities that arise in the adaptation of these very deep mathematical theories to understand the physical world ultimately drove the physicists to enquire into the very foundations of spacetime, and change its micro-structure from that of the familiar manifolds to a supercommuting manifold, and even, a noncommuting manifold.

Some people are uncomfortable with this foray into the deepest parts of mathematics to explain Nature. They feel that the beauty of mathematics is very seductive and leads the physicist away from his true quest of describing Nature. I remember an occasion when we had invited Julian Schwinger to speak in our department. His lecture was on *the Epistemology of Modern Physics*. In a long and far-reaching discussion after the talk, Schwinger explained to us his view that the *phenomena themselves should force us to the mathematical schemes that will best explain them*. At that time he was referring to his work on the quantum measurement algebra in which he had shown, quite brilliantly, how the modern way of explaining quantum theory via vector spaces and operators is literally forced on us by the analysis of the Stern-Gerlach experiments (see the essay on quantum algebra). This is however quite opposite to the approach of the string theorists and the supersymmetricians. For them the idea is to perfect the mathematics first and *only after that* look for physical interpretations that tie it to the real world. This is the *Dirac mode* mentioned earlier (see also the essay on super geometry).

There is also some misunderstanding, mainly on the part of mathematicians, about the way mathematics is used by the physicists. It is about the role of *rigor* in physical calculations. I happen to think that rigor is not that crucial. The essential issue is whether the particular type of mathematics is the right language to use. In some sense therefore physics is more concerned with the *formal* rather than the *analytical* structure of things, more with the question whether we are using the right language, rather than in the minute details of fitting the language with reality. The situation is quite different from what it is in mathematics. Here is what Hermann Weyl says about the role of mathematics in physical sciences: *Men like Einstein or Niels Bohr grope their way in the dark toward their conceptions of general relativity or atomic structure by another type of experience and imagination than those of the mathematician, although no doubt mathematics is an essential ingredient.*

There is also a philosophical aspect to this quest that has been examined by many people such as Weyl, Schrödinger, Schwinger, and others. The creations of the mathematician reflect an esthetic that is purely internal and yet, miraculously, these very same constructions are precisely the ones we need when we try to understand the physical world. The examples are many and well known:

(a) the theory of ordinary differential equations and their uncanny applications to the Newtonian theory of celestial motion and universal gravitation;
(b) Riemannian geometry and its use in Einstein's general theory of relativity;
(c) fiber bundles, connections, and their emergence as the basic tools in gauge theories like Yang-Mills;
(d) the use of spin geometry in Dirac's equation for the electron and in unified theories;

and so on. In several conversations I had with Harish-Chandra he often used to refer to this phenomenon as the *coincidence of the inner and outer realities.* With so many examples like these, there has emerged a point of view that *what is not forbidden must be true* and that *only beautiful theories have a chance of being also true.* Weyl, Dirac, and perhaps even Einstein, in his later years, were among the foremost believers in this principle. It is in the spirit of this philosophy that one should view the discussions in my essay on nonarchimedean physics. There it is not a question of immediately connecting p-adic mathematics with physical reality but one of building a structure that *may* have some aspects of reality in it.

In Newtonian mechanics, and even in classical electrodynamics, the phenomena studied are for the most part close to experience, and there is no difficulty in understanding the forces acting on the bodies and setting up the dynamical schemes. But in quantum mechanics and quantum field theory this is no longer true. As a consequence one is often forced to use *symmetry* as a guide in the choice of the Lagrangian or Hamiltonian to set up the dynamical equations. Consequently underlying all the discussions in this book is the massif of *representation theory*. It is the essential ingredient in all descriptions of symmetry and is present in all the essays in this book. My own understanding of physics and mathematics is built upon this theory whose power to influence mathematics and physics is unmatched. As an illustration of its power even in purely mathematical theories I discuss in one of the essays its application to the theory of differential equations.

1.2 A quantum education and evolution

My first brush with quantum theory came in 1954 when I was an undergraduate in my home town, Madras (now called Chennai, with good justification), studying in the Presidency College for a master's degree in Statistics. It was during that year that Dirac came to Madras and gave a lecture at the University. I had heard of him of course but had absolutely no idea of what he had done or why he was revered so much. I went to the talk in a big hall which was overflowing. I understood nothing, but one remark of Dirac still stands out in my mind: *the world is non commutative*. My subsequent progress in my studies had no direct connection with quantum mechanics. It was mainly probability theory at the Indian Statistical Institute in Calcutta. After my degree I went to the US, first as a post doctoral fellow to Princeton University in 1960. In the Fall of 1960 I went to the University of Washington at Seattle due to the kindness of professor Edwin Hewitt who liked some of the mathematics I did in my thesis. Seattle was a wonderful place for me. I made the acquaintances of a number of young people—Ken Ross, Albert Frodeberg, Roger Richardson, Bill Woolf, Albert Nijenhuis, to mention a few. Harish-Chandra's suggestion that I make myself thoroughly familiar with the Chevalley volumes was always ringing in my years, but they were difficult to penetrate, especially the first volume. By the greatest of good fortune, Richardson, who came from the University of Michigan, had his own handwritten notes of Hans Samelson's courses on differentiable manifolds and Riemannian geometry. They were wonderful for a beginner like me and I worked through these and finally began to understand what the global theory of Lie groups was all about. At the same time I also carefully read Dynkin's famous paper on the classification of simple Lie groups through what we now call Dynkin diagrams, as well as Weyl's proof of his famous character formula, given in his *Classical Groups* and *Group Theory and Quantum Mechanics*.

I cannot speak enough of the courtesy and accessibility of Richardson. He was already a mature mathematician, with a regular position. His beautiful work with Nijenhuis on deformations of Lie algebras was still to come. He would come almost daily to the part of the department where Woolf and I had offices and talked about mathematics. Nijenhuis and Woolf were collaborating on their extension of the Newlander–Nirenberg work to the $\mathbf{C}^{1+\alpha}$ case, and Nijenhuis would come almost daily to our little alcove. So I was in daily contact with beautiful mathematics, and these influences were pivotal for my career.

In Seattle two things happened: the first, a minor event, was that I picked up a used copy of Dirac's book, almost new, for one dollar (I still have it); the second was that in the summer of 1961, Professor George Mackey came to Seattle as a Walker Ames Professor and delivered a series of lectures on unitary representations and quantum mechanics. I had by this time become very interested in group representations and had studied, in a desultory fashion, the famous Murray–von Neumann papers on Rings of operators and some parts of Weyl's *Classical Groups.* But the connections of representation theory with quantum theory were not there in my mind until I heard Mackey explain them. For me these lectures were a revelation, a first glimpse of the noncommutative world. I fell in love with quantum theory, and from then on, quantum theory and representation theory were to be my most beloved interests.

That summer in Seattle with its customary lovely rain-free weather, was a most idyllic time in my life. I had no duties except for teaching one summer course. Mackey lectured every day for four weeks. The lectures were a tour de force, connecting all kinds of things with a unified philosophy that was profound and inspiring. I attended the lectures, and talked with him about almost all aspects of his work. His philosopy, his perspective in both mathematics and physics, and his passion, all made the deepest impression on me. It was the single sustained personal learning experience that I have ever had in my entire scientific career.

Inspired by Mackey's lectures I hastened to read Dirac and von Neumann. From my point of view as a probabilist, I was most struck by von Neumann's wonderful conception that in quantum theory an orthocomplemented partially ordered set replaced the Boolean algebra of events of classical probability theory. This structure, whose members are the *experimental propositions*, was called by von Neumann the *quantum logic*, and, for the standard models in physics, was just the *projective geometry* of the closed subspaces of a complex Hilbert space. The concept of an observable as a self-adjoint operator arises naturally in this context and is the proper substitute for the classical notion of a random variable. But, and this was the essential difference in the quantum case, classical calculations involving more than one random variable could not be carried out in the quantum world unless the variables are *simultaneously measurable*. It was a beautiful theorem of von Neumann that quantum observables in the standard Hilbert space model are simultaneously measurable if and only if their operators commute with each other, or equivalently, if and only if they are all functions of a single observable. It occurred to me to ask if this theorem of von Neumann on simultaneous observability could be proved in the abstract setting of a more or less general quantum logic. I was able to do this in the summer of 1961. Then I went to the Courant Institute, where I met Warren Hirsch who was very friendly to me and we became close friends. He encouraged me to publish this work and I submitted it to the Communications in Pure and Applied Mathematics, where it appeared.

Warren and I became close friends for we shared many common interests. I was very young, far away from home, knew nobody. Warren took me under his wing and cheered me up tremendously. I will always remember his infectious laugh, sense of humor, and supreme courteousness. I shared an office with Hermann Hannish; and Warren, who was collaborating with Hermann, would come in every morning and we would chat briefly of many things. I remember his telling me that the main qualification of Byron "Whizzer" White to be a supreme court justice was his ability as a football player!! I did not know if he was joking or serious but I enjoyed it!!

Of all the people I met during my year at Courant (1962), Warren was the most vivid, the most humane, and the most interesting, to me. In retrospect I realize that he was extraordinarily generous, to have been so accessible to an unknown young mathematician (I was 25) from nowhere. He was a probabilist, but eventually branched into mathematical epidemiology, where he made fundamental contributions and became a seminal figure.

I returned to the Indian Statistical Institute at Calcutta in 1962. I have spoken elsewhere[2] about these years and the efforts of a few of us to build something lasting at the Institute. From this group only Varadhan remained that year. We ran a seminar where we discussed whatever came to our minds: quantum theory, markov processes, representation theory, and so on. Markov processes were Varadhan's great love but I was more interested in representation theory, and so we switched to that topic. As a first step(!!) we started a project of specializing Harish-Chandra's work on infinite dimensional representations to *complex* semisimple Lie algebras [2].

For me this was the start of a long period of working on Lie groups and their representations, and represented a turning point in my mathematical education and growth. The work of Harish-Chandra and the immense effort required to understand it, led me into the world of semisimple groups. No one who has not entered it can understand its incredible beauty. Here are objects which are special but are beautiful to an almost limitless degree, and which have moved so many people to spend extraordinary amounts of time investigating them.

Why are semisimple groups so fundamental and so all-pervasive? This question seems to have no satisfactory answer. I remember vividly the Williamstown conference on representations of semisimple groups in 1972 when Harish-Chandra was giving a series of lectures on his work on the Plancherel formula for p-adic semisimple groups. The lectures were built around what he called *the Lefschetz Principle*, to the effect that *all primes must be treated on an equal footing*. He had used this principle to guide him in his work on p-adic groups by starting from his immense insight on real groups (at the infinite prime) and seeing how the theory of real groups led him to a substantial part of the harmonic analysis of the p-adic groups. He was of course aware that this approach may not give everything in the p-adic case. To buttress his own conviction perhaps, but certainly to take the audience into his confidence, he recounted a story that Chevalley had told him. The time of the story was that of the Genesis, when God and his faithful disciple, the Devil, were creating the universe. God told the Devil that he (the Devil) had a free hand in creating whatever he wanted, but there were a few things that He (God) will take care of Himself; According to Chevalley, semisimple groups were among these special things(!). Harish-Chandra, after reciting this story, added, that he hoped that the Lefschetz Principle was also among these special things!

My interest in the world of semisimple groups would last a long time, during which period the world of physics was on the back burner, so to speak. However events changed all this very abruptly. In 1983 Harish-Chandra died of a heart attack and that was an event that changed my entire intellectual landscape. The loss of someone who was a close personal friend as well as a great mentor was too much to bear and to understand. When I recovered from it eventually, I started to work on other themes in which I could create my own paradigm. The idea that I should revive my original interest in quantum theory was a natural one and an opportunity to bring it to the front arose in 1988 when I was invited to go to Genoa, Italy, to work with the physics group there, led by Enrico Beltrametti, and his younger associates Gianni Cassinelli, Piero Truini, and their students, especially Ernesto De Vito and Alberto Levrero. We called ourselves the quantum group of Genoa for fun and that visit was the first of many since then. We worked on fundamental questions of quantum theory, relativistic wave equations, extension of the

Mackey theory to orbit schemes, and so on. The interest in physics remained unabated after that.

I had, by then, also started talking about physics with Don Babbitt and among the topics we wanted to understand was the spectral theory of first order Schrödinger systems. Gradually we realized that there was no systematic treatment of the foundations of first order systems, and that there was a group theoretic approach that appeared tantalizingly new. This was the beginning of my interest in the group theoretic view of differential equations and the starting point of a very long collaboration with Don Babbitt, my longest and most sustained with anyone. It led to many long papers [3] on differential equations with *irregular singularities*, and to very happy and deep interactions with the great masters of the theory such as Yasutaka Sibuya, Bernard Malgrange, Tosihasu Kimura, Jean-Pierre Ramis, Werner Balser, and above all, Pierre Deligne. Their interest and comments [4] were responsible for some of our best work and deepest insights, although our work touched only a small part of the beautiful theory of ordinary differential equations in the complex domain. The local reduction theory and moduli of irregular systems touches in a surprising manner on semisimple groups and their orbit spaces, not only over **C** but also over *function fields*.

In UCLA I had the privilege of becoming friends with a great master, Bob Finkelstein, and learning, not only physics but also the attitudes and approaches of physicists to physical problems. The situation became even better for me in the 1990s when Sergio Ferrara, one of the world's leading authorities on super symmetry, accepted a permanent part-time position at UCLA and started to come regularly to Los Angeles. We started discussing physics and mathematics and that was how I began to get interested in Picard-Fuchs equations, regular singular differential equations, their moduli and monodromy, and most importantly, supersymmetry. Attending Sergio's lectures gave me the idea to investigate the mathematical underpinnings of unitary representations of super Lie groups, especially super Poincaré groups. This I did with the Genoa group of Gianni Cassinelli, Alessandro Toigo, and Claudio Carmeli. I must mention that the work of Kostant [5] was a great pioneering effort in super geometry. To me however, personally it was the paper of Deligne and Morgan [6] that was truly inspirational and started my own excursions into super geometry.

The p-adic story represents an entirely different direction. Of course representations of the p-adic semisimple groups have long been of interest to mathematicians. Already in the early 1960s, Gel'fand, Mautner, Bruhat, Harish-Chandra, Jacquet, Langlands, and others saw that unitary representations of p-adic semisimple groups had deep arithmetic significance. But no one has thought much of the relevance of nonarchimedean models to physics except Beltrametti and his collaboratiors in the early 1970s. Their view was that the singularities that were the plague of quantum field theory could have arisen from the fact that the micro-structure of spacetime was radically different from what is usually assumed, for instance, that it is a real manifold. They had the idea that one should investigate the possibility that it is a p-adic manifold. This idea was revived in a big way in the late 1980s when Igor Volovich [7] proposed that in regions of spacetime whose sizes are of the order of the *Planck units*, no measurements are possible and so spacetime geometry in such regions could not satisfy the archimedean axiom. From the Volovich point of view it is a reasonable assumption that the micro geometry of spacetime is non archimedean. I came across thse ideas in 1990s when I was already aware of the work of Beltrametti and his collaborators, of Weyl's work on the commutation rules over *finite rings*. and Manin's paper on adelic physics. My interest received a big stimulus when I went to Dubna and Moscow and had the opportunity of spending time with Igor Volovich and Anatoly Kochubei. The group-theoretic connections were (and are) especially fascinating to me. Although the groups involved are *not* semisimple, the physically interesting cases of the Poincaré groups and the associated quantum field theories over p-adic fields and adelic rings present interesting challenges [8].

All of these topics are discussed in these essays. In this sense the essays must be read like a travelogue, an account of an intellectual journey. I mean this in the simplest and most naive sense: an account of some of the things that I came across which interested me, and which I hope will interest some others. The musical analogy with what I am going to say is that it is like a string quartet or quintet, a recital of some voices, which I, as a moderator, try to blend and present.

It should be clear to anyone who has read what I have to say in this prologue that I have a great indebtedness to a huge collection of friends. But above all the greatest debt I owe is to George Mackey and Harish-Chandra. In the last essay I write about their work and how my personal interactions with them allowed me a glimpse into what may be called, without any controversy, *the right way of looking at things*.

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Chapter 2
Quantum Algebra*

The ways of gods are mysterious, inscrutable and beyond
the comprehension of ordinary mortals.
Julian Schwinger

Quantum algebra was created by Dirac. Its evolution also bears the imprint of the genius of many great mathematicians and physicists such as Weyl, von Neumann, Schwinger, Moyal, Flato, and others. It has inspired developments in deformation theory, representation theory, quantum groups, and many other mathematical themes.

2.1 The quantum algebra of Dirac

The quotation above is Julian Schwinger's tribute to Weyl on the occasion of Weyl's birth centenary*, but it applies with even greater force to the mysterious way in which Dirac and Heisenberg slew all the dragons of classical physics and let quantum theory emerge. We can understand almost all of their thought processes but there will always be a residue of mystery to the moment of creative genius when things suddenly go to a new level of perception and imagination, and everything falls into its place as if by magic.

The term *quantum algebra* appeared for the first time in a paper of Dirac[1a, 1b] which has now become famous. Just a few months earlier Heisenberg [2]

* This essay and the next are based on lectures given at Howard University, Washington D.C., sponsored by my friend D. Sundararaman, in the 1990s.

V. S. Varadarajan, *Reflections on Quanta, Symmetries, and Supersymmetries*,
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had come up with the startling and revolutionary idea that in quantum theory physical observables must be represented by hermitian matrices which are in general of infinite order; and that if the physical observable x is represented by the matrix X, then the observable x^n is represented by the matrix $X^n (n = 1, 2, \ldots)$, the n^{th} power of X. However Heisenberg had realized that the matrices do not obey the commutative rule of multiplication that the classical observables did, and felt that this was a serious flaw in his scheme. Heisenberg had sent the proof sheets of his article to Fowler at Cambridge; and Fowler, who was at that time the thesis advisor to Dirac, passed them on to Dirac. After a study of Heisenberg's paper Dirac realized that the noncommutativity of the quantum observables, which had appeared to Heisenberg as an unwelcome aspect of the new mechanics, was in fact one of its central features, and led to a structure for the new mechanics which was a beautiful and far-reaching generalization of classical mechanics, and which had the classical mechanics as its limiting case in the correspondence limit when $\hbar \to 0$. Dirac's great conceptual insight was that the quantum observables belong to an algebra which is noncommutative but in which the *commutator*

(1) $$xy - yx =: [x, y]$$

of two elements x, y, which measures the departure from their commutativity, *corresponds to*

$$i\hbar\{x, y\}$$

where

$$\{x, y\} = \sum_{1 \le r \le k} \left(\frac{\partial x}{\partial q_r} \frac{\partial y}{\partial p_r} - \frac{\partial x}{\partial p_r} \frac{\partial y}{\partial q_r} \right)$$

is the classical *Poisson Bracket*:

(1a) $$[x, y] = i\hbar\{x, y\}$$

Implicit here is the assumption that the observables in both the classical and quantum theories are denoted by the same symbols but have different multiplicative structures. It is not clear from Dirac's discussion whether the commutator is *exactly equal to* the Poisson Bracket, although for the position and momentum observables

$$q_1, \ldots, q_k, p_1, \ldots, p_k$$