

A Glance Back at Five Decades of Scientific Research

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Abstract. I review my scientific research career for the last 50 years, with emphasis on the issue of “Poincaré recurrences”: I stress some ideas of mine which became so popular that they have been taken up (recurred) by others, sometimes forgetting the original source.

1. Introductory Remarks at the Symposium

This symposium “Particles and Fields, Classical and Quantum”, where so many of my friends in research areas that I was (and am) involved are in, gives me an opportunity to recall and recollect how and why I shared these topics with others, and their interconnection. And the happy chance connections which brought them about. It is a remarkable fact that the more one reviews the circumstances in which a particular item of research came about, the more one is amazed at the interconnections of items of knowledge. To the uninitiated, the fact that these areas of research are apparently unconnected may compare my style of work with the eating habits of a sheep jumping from one bush to another every few minutes, in contrast to others who share the grazing habits of a cow, confining themselves to one area. Cows are most people's favorite, but sheep are more of a challenge. That is probably why my father had the saying that every boy should have a sheep or two to tend.

Interconnections between topics are often confusing. Once, reading my (elder) brother's college textbook on physics, I came across the formula for the period of a simple pendulum where it stated, “ The derivation of this formula is beyond the scope of this book”. I was amazed that a major college text would admit such a fact; and that there is so much more of physics which would enable one to calculate it. The same was true of the formula for the focal length of a thin lens. One thing leads to another, and soon you are searching for answers to basic questions.

Another time during lectures on Classical Logic, we were introduced to an “experimentum crucis”. It was illustrated by the deciding experiment of Fizeau on the speed of light in water as compared to its speed in air. Since wave theory predicts that speed in water is less, and corpuscular theory (with point particles) predicts it would be faster, this is supposed to have selected the wave theory is correct. But then how would one accommodate the photoelectric effect? Then it turns out that if the “corpuscle” of light had a finite size, corpuscular theory also predicts lower speed of light in water. But then one can ask how come photoelectric emission being prompt even in feeble light, how could the energy of a photon spread over $\pi(\lambda/2)^2$ act as a whole and liberate a single photoelectron! This leads us to question the square of the amplitude

being interpreted as the probability of the particle being formed in the immediate vicinity. How do probabilities enter quantum mechanics? Thus the questions (and the quest) go on.

2. Poincaré Recurrences. Markov Processes. Dynamical Maps

The analysis of celestial motion under periodic perturbations often gave secular terms; but these are not consistent with perturbed periodic motion. H. Poincaré in his brilliant analysis [1] showed that these violations of periodicity coincided with resonances. Earlier he had shown [2] that for any motion in a compact region, a collection of nearby trajectories may diverge and tangle up, but sooner or later the configuration should reappear more or less intact. These are called “Poincaré Recurrences”.

The reappearance of the same theme in different contexts is somewhat similar. In the following outline a few such instances occur. The first piece of research I did was to study the multiple Coulomb scattering of charged particles in special photographic emulsions as a means of determining pv/c for the particles [3]. The arithmetic mean of the second differences is a measure of this quantity. But this suffers from noise as well as curvature of the tracks due to the distortion of the emulsion itself. In the course of this work, done in the group led by Bernard Peters, I had to learn the correlated distribution of the transverse displacements. This study taught me how to determine the third differences of the displacements [4]. While the scattering due to many random collisions is akin to Brownian motion in two dimensions, and the distinction of both direction and position is a Markov process, the distribution in transverse positions alone is not a Markov process [5]. This was my first exposure to open system dynamics. About this time a group at the University of Madras was studying classical stochastic dynamics of finite level systems. The theory was simple and elegant. A three level system has 'states' described by points in the interior of an equilateral triangle of unit height, all curves spiraling or directly approaching a local point. R. K. Bansal and I wrote a short note [6] on it. These ideas prompted me to think of the nature of stochastic processes for finite dimensional quantum systems. These investigations led to a paper, “Stochastic Dynamics of Quantum Mechanical Systems”, in which the main results of the theory were carried out explicitly [7]. In this paper we showed that a dynamical map can be obtained as the contraction of unitary transformations of an extended system. There are cases in which the dynamical matrix has only non-negative eigenvalues, and the map has the form $\rho \rightarrow \sum \zeta^{(n)} \rho \zeta^{(n)\dagger}$ with $\sum \zeta^{(n)\dagger} \zeta^{(n)} = 1$. Such maps [7] are now called completely positive. We also showed how to construct such a map by contraction of a unitary transformation. The classification of such maps and their parameterization were given in another article [8] I wrote for the Yuval Ne'eman *Festschrift*.

Despite the early work being published in the Physical Review, more than a decade after the results were reproduced by another scientist whose major contribution to the topic was to change Greek letters in the formulae to Latin characters and vice versa. So many people refer to my work as the “Kraus representation”. It is amazing to me that either people did not read the literature or do not recognize (and know) that the work, including the reproduction, is identical to and was lifted from my work [9]. My second article has not yet been 'lifted' by anyone. Yet!

In the meantime, Vittorio Gorini joined me in Austin, and we examined the differential form of the map to obtain a stochastic semigroup. Along with Andrej Kossakowski, who joined us, we obtained the generic form of the completely positive semigroup of a stochastic map [10], following a paper by Kossakowski on the semigroup structure [11]. But once again the “Poincaré recurrence” occurred. A mathematical physicist in Sweden with whom I had

discussed these developments, wrote a paper [12] on the same subject without any reference to my discussion or to Kossakowski's pioneering paper. Now half the world refers to this as the “Lindblad Formalism”.

There are many people who claim that the not completely positive maps are unphysical. Yet the dynamical evolution of an extended system with entanglement leads to such evolution. Many people were quite dissatisfied with the positive maps which were not completely positive, some going so far as to claim that they violated the basic principle of quantum mechanics. Some of the concerns originated from misunderstanding: the definition of a not completely positive map as being m -positive if $\varrho \times I_m$ is positive, but not $\varrho \times I_{m+1}$. They then thought our inert “cuteness” with an $(m+1) \times (m+1)$ most degenerate matrix would lead to violations of quantum probabilities. This absurd claim was based on the misinterpretation of the mathematical definition of a not completely positive map by M. D. Choi [13].

3. From Convex Sets of Density Matrices to Axiomatic Field Theory

The study of density matrices and mappings had an unexpected dividend. Since the density matrices form a convex set we could extend their properties to other convex sets. A particularly interesting, but unexpected, application is to Axiomatic Field Theory, as formulated by A. S. Wightman [14], who worked with vacuum expectation values of the (unordered) product of field operators. He could reconstruct the field operators from the vacuum expectation values using the Gelfand-Segal construction. The convex sum of two (or more) sets of vacuum expectation values is again a set that satisfies all the Wightman axioms. But the corresponding field theory has two (or more) vacuum states. The existence of one and only one state invariant under the Poincaré group $U(a, \Lambda)$ should be added to complete the Wightman axioms [15]. I also know the quantity $\tau(x) =: \Phi^2(x) :$ (normal ordered square of a free field) is also a Wightman field; but it has realizations which contain an odd number of Φ quanta in a field theory with no vacuum state! But the axiomatic field theorist never acknowledged my work showing that the original Wightman axioms were incomplete.

4. The V-A Interaction

During my days in Tata Institute of Fundamental Research in Mumbai, India, we had many distinguished physicists visiting us. One of them was Maria Göppert Mayer. In preparation for her lectures, I studied many papers on nuclear beta decay along with my colleague, S.D. Soman, as well as Louis Michel's paper on the electron spectrum from muon decay, but it was only after I became a graduate student at the University of Rochester that I began the serious study of weak interactions. The discovery of parity violation in 1956 made beta decay a part of particle physics. Robert Marshak suggested that I study the field of weak interactions. C. S. Wu gave a lecture at the 1955 Rochester High Energy Physics International conference with the preface: “I am here talking to you on the strength of weak interactions”. I had already gotten captivated by symmetries in particle physics. During the period of Spring 1956 I studied every relevant paper that came in, and found that the experimental data were not consistent so I concluded that some of them must be wrong! On the basis of the work on angular correlation in beta decay, the decay of the muon, and pion decays, as well as parity violation experiments, on the longitudinal polarization of beta decay electrons and the decay asymmetry of electrons from polarized nuclei and muons, it was abundantly clear by Christmas 1956 that the only beta decay theory consistent with a Universal Fermi Interaction was V-A with chiral projections of

all four fermions. I could not convince Marshak that this was a sound conclusion even with the incontrovertible analysis of all existing experiments; and so I could not get even five minutes to present my conclusions at the Rochester conference!

That summer Marshak was at the Rand Corporation in Santa Monica, California; Ronald Bryan and I went to Los Angeles to work in consultation with Marshak. After an informal discussion with Murray Gell-Mann (who was also visiting Rand), Marshak decided that I should present the theory to Gell-Mann. Bryan and I paid our share for a lunch in Santa Monica, where I gave a full presentation. Gell-Mann was convinced, and so was Marshak. Gell-Mann told us that he did not plan to write a paper on these ideas. Marshak asked me to write up the material, which I promptly did. Instead of sending it for publication immediately, he said that he would present the theory at the forthcoming conference on “Mesons and other newly discovered particles” in Padua-Venice in September, so my manuscript [16] “rested in peace” from June through September 14. I am very pleased with this paper, which is an ideal paper in that it formulates the problem, marshals all the data, identifies the crucial experiments which have to be redone, and formulates an interaction with a great deal of symmetry. The journal paper came a bit later [17].

As luck would have it, apparently Gell-Mann talked to Richard Feynman and they also created a paper [18] with no analysis but with the declaration that it was the V-A interaction. Despite the clear evidence of our priority and the systematic analysis, everyone quoted our V-A theory [16], but ascribed it to Feynman and Gell-Mann. Among the people who did not quote our paper was Robert Oppenheimer, but he admitted that though he had received it in August, he did not read it for a year!

Many people did not read our paper, and found it convenient to quote the paper of Feynman and Gell-Mann. If they had read and followed the arguments and analysis in our paper, they could no doubt recognize that the primary discovery was in our paper. But now, fifty years later, those who do read both papers could have no doubt as to who discovered the chiral V-A interaction.

5. Symmetries in Particle Physics and Superluminal Motions

In particle physics, the fifties and sixties were the golden years. Symmetry principles had much to predict (or relate) particle properties. Susumo Okubo, Robert Marshak and I obtained many of these consequences [19]; other people, including Sydney Meshkov, were also doing so. So it is not surprising that there were many “Poincaré resonances” during this period. A special case of Okubo's mass formula [20] was independently discovered by Gell-Mann for the baryon Octet [21].

During the summer of 1959, which I spent at Rochester, one of the students asked me why is it that special relativity forbids particles traveling faster than light. I thought about this and finally came to realize that while such particles were not yet found, there was no insurmountable difficulty for them to exist. The “rest mass” had to be imaginary to get real momentum and energy. The relativistic addition law guaranteed that if particles traveled faster than light in one frame, they did so in every other frame. The speed of light is a limit for these particles also, and it is approached when the energy and momentum increased indefinitely. Our study was rejected by three referees of Physical Review: one said it was “all wrong”, the second that it was “correct but well known”, and the third said he agree with “both” the earlier referees! I

discussed these results with my (late) friend Gerald Feinberg at Columbia in detail. Some years later my colleague at Rochester, Olexa-Myran Bilanuck rewrote the paper for the American Journal of Physics [22] so we got it published and it generated much correspondence in Physics Today.

Poincaré recurrence strikes again! About seven years after this paper and about ten years after my discussions with Feinberg, he wrote a paper and got it published right away [23] in the same Physical Review which treated my paper somewhat shabbily. The New York Times wrote a long article about it, but almost all quotes were from our American Journal Paper! Time Magazine had also a short article on it; they, too, lifted sentences from our paper, and attributed it to Feinberg. Several letters were sent by different people to New York Times pointing out these incorrect citations. Needless to say, none of them got published. The subsequent years made clear that we had done the work, many years prior to Feinberg. Some years later Walter Sullivan wrote a lengthy article also in the N.Y. Times about our work with no mention of Feinberg (or their misallocation of the origin).

6. Light on Quantum Optics

Misattribution of work (including specific sentences) was even more glaring in the formulation of Quantum Optical Coherence. Emil Wolf and Leonard Mandel had studied the statistical states in optical photocounting and autocorrelation functions. This was done in terms of expectation values of quadratic Gaussian functions of the optical wave amplitude [24], very much in the style of G. I. Taylor, Theodore von Karman, and the book by G.K. Batchelor [25] for turbulent flow, rather than in terms of a probability functional. Denis Gabor had introduced the notion of “analytic signal” which has only positive frequencies. All problems of conventional optics like reflection, refraction, double refraction, interference, and diffraction, need only bilinear functionals. What modifications are introduced by using quantum electrodynamics? I have studied the question and came to the conclusion that there is, in fact, no difference. For every quantum field there is a classical wave field which gives identically the same results for these standard optics phenomena [26]. So when much fuss was made by Roy Glauber [27] that “There is no substitute for quantum theory” and that “all classical coherence optics should be abandoned” at some gatherings where Emil Wolf was present, I told him that for these phenomena there is absolutely no difference. Glauber ought to have known it from my lectures on phase space descriptions of canonical variables at the Brandeis Summer School years earlier where he was also lecturing.

I gave the one and only formulation of quantum optics by showing that all quantum fields could be represented as a diagonal coherent state distribution

$$\rho = \int \Phi(z) |z\rangle \langle z| d^2z$$

but with a weight function which is real but not necessarily nonnegative. (The coherent states were introduced by Schrödinger about the time I was born!). I gave the diagonal weight in terms of the density matrix and gave an explicit formula. It shows, in particular, that the one-photon states do have a highly singular indefinite weight distribution [28]

As far as distributions are concerned, some people only consider distributions appropriate for Riemann integration or Stieljes integration. Others like tempered distributions. But I had given the explicit form which in many cases is indefinite [29]. (To stick to some preconceived class of distributions is like the man looking for his lost key bunch at the foot of the lamppost

because that is the only place where there is light!). Glauber promptly rechristened my diagonal representation [30] (with Greek and Roman letters interchanged) and presented it as his “original” contribution (but his heart was not in it). However, he kept insisting that the distribution weight must be non-negative. Obviously he did not calculate the weight for a one-photon state, already given in my paper! Nor did he calculate the diagonal weight for any state, including squeezed states.

Chandra Lal Mehta and I obtained the diagonal weight as the solution of a simple integral equation [31]. In my 'big' paper [32], I developed the subject in great detail. In the book [33] written by John Klauder and me, the matter is also spelled out in its entirety.

During my Tata Institute days, P. A. M. Dirac gave a course of lectures on quantum mechanics, the first formal course on the subject that I ever had. In those lectures, he introduced the notion that given a “standard state”, we can reach other states by acting with suitable operator since one can go from any vector to any other vector by a continuous path, so we can get an over-complete set of states. So when I got the manuscript of John Klauder's papers on continuous representation for review, I recognized it and recommended immediate acceptance. In the meantime, the continuous representation in terms of the family of Weyl operators

$$W(\lambda, \mu) = \exp i(\lambda p + \mu q)$$

which were bounded operators, and which generated Schrödinger's minimum uncertainty states for the ground state as λ and μ are considered as the real and imaginary parts of a complex variable z ; these states may be identified, apart from normalization, as entire functions. These entire functions constitute a Hilbert space with a suitable norm. Valentine Bargmann [34] and later Irving Segal [35] studied these Hilbert spaces. So these representations were well known long before 1963. The bilinear expectation values of normal ordered variables were shown to be the same as in classical theory in my Brandeis lectures [36]. So the slogan, “No substitute for Quantum Theory”, was only a slogan. It was also known from my lectures that all states can be represented by Wigner-Moyal functions, almost all of which are indefinite.

When the weight distribution is not positive definite, we have true manifestations of the quantum nature of the distribution. For example, the photocount distributions would show antibunching of photons. This was experimentally demonstrated by Kimble, Mandel, and Digenas [37] from the University of Rochester. So there is also the possibility of negative Hanbury-Brown and Twiss intensity correlation [38]. States with squeezing of over 60% were also obtained by Jeff Kimble at the University of Texas, Austin [39]; these have truly quantum distributions.

Despite all this material being available in print, the Swedish Academy publication (the Press Note [40] issued by the Swedish Academy) on the web, attributed all these non-classical effects to Roy Glauber: I was mentioned as providing “Some mathematical contributions”. When evidence is clear as to what was the work that I did, it was attributed to Glauber. I am truly amazed and puzzled how all these quantum effects can be attributed to Glauber's work and my work mentioned as merely mathematical contributions! “There is no Prosperity without Publicity” [41]. That may be the reason for such darkness over quantum optics.

7. Quantum Zeno Effect. No Recurrence?

Since quantum mechanics works in terms of amplitudes rather than probabilities, one would expect to get transitions from a metastable state, the survival probability of which goes as a Gaussian in t rather than the exponential law (familiar to us from the observations of decay in radioactivity). Dirac showed that if we have a continuum of states into which the decay could go, it can be approximated by an exponential [42]. But if we did not have the condition and approximations that Dirac outlined, should we see the decay probability corresponding to the

Gaussian law? Baidyanath Misra and I called this effect the Quantum Zeno Effect [43]. This work has been followed up by many scientists like Misra and Charles Chiu, and most significantly Severio Pascazio [44]. The effect has been experimentally verified in a beautiful experiment by Wayne Itano and collaborators [45] at NIST (Colorado), and by Mark Raizen at Austin [46]. This vanishing of the transition rate is important in efforts at stabilizing qubit states in quantum computing [47]. It would be relevant for understanding of collective mode-mediated processes in physical chemistry [48].

8. Still More Recurrences: Quantum Measurements

There are other areas of my research that have manifest Poincaré recurrences. About 25 years ago I wrote a paper on implementing the only consistent model of a quantum measurement [49]. A good measuring instrument should be stable except for a sensitive pointer that responds to the quantity that is being measured. To avoid an infinite regress, the measuring instrument should be classical. But then, how to couple a classical instrument to a quantum system? This problem could be solved by making the classical measuring instrument a special case of a quantum system with double the numbers of degrees of freedom at the classical level by introducing for any classical variable ω (canonical or otherwise), the quantum companion $-i\partial/\partial\omega$ but decree that these conjugate operators are not measurable [50]. We could then couple this quasi-quantized system with the true quantum system to be measured. By suitably choosing the interaction we could make the measurement decisively.

If $-i\frac{\partial}{\partial\omega}$ are to continue to be unmeasurable, we could only measure a compatible set of (primary) quantum observables. If we have to measure the quantum variables, that do not commute, the commutation of their measurement interaction leads to the existence of terms $-X\frac{\partial}{\partial\omega_1}$ and $Y\frac{\partial}{\partial\omega_2}$ in the hamiltonian. The commutation of these interaction terms contains the term

$$[X, Y]\frac{\partial^2}{\partial\omega_1\partial\omega_2}$$

But this would act on dynamical variable $f(\omega)$ and yield a term involving $\frac{\partial}{\partial\omega}$. But this was to be non-measurable. So all X, Y, \dots in the measurement interaction should commute. This gives us the requirement that only a commuting system of quantum observables can be simultaneously measured. This does not have to be inserted as a postulate.

Along with Tom Sherry and Sulaksh Gautam I applied it to the Stern-Gerlach type experiment to measure the magnetic moment and hence the spin of particles in a molecular beam [50]. F. J. Belinfante was the referee, and he offered numerous suggestions for improvement of the papers, which were published in the Physical Review [51]. My primary paper published in Pramana – Journal of Physics [52] had a very patronizing referee report to the effect that she could not find anything specifically wrong with the manuscript, but she did not think it would have any physical significance! But unknown to her, (the late) Asher Peres reproduced the model calling it a quantum-classical “hybrid” [53]. Though he did not take the correct path to make it a model of measurement, he pronounced it to be impossible. Such a wise person as Peres could not be expected to deliberately ignore my earlier work, but like many other people, he did not bother to read and/or understand what I did. Thus I conclude that this, too, is a case of Poincaré recurrence. But then, this was the fate of the work on dynamical maps also with his group, another recurrence.

9. Concluding Remarks

Why do these things happen? The simplistic views that honor is no longer valued amongst scientists, and that we are in an age of robber barons as far as scientific discoveries are concerned. But it is tempting to think of the possibility that ideas are like bundles of trajectories undergoing complicated evolution; but since the domain of these ideas are in a compact space, Poincaré recurrences occur. But it seems to be a malevolent system that gets Poincaré recurrences to my loss.

It is to be emphasized that it is the duty of scientists to prevent such misappropriations and to try to set right what is wrong. This involves not only goodwill, but also moral courage to take the proper responsible stand.

I wish to conclude by telling you how gratified I am to have such a celebration for my birthday. I am not unaware of the responsibility and effort involved in arranging this conference. Special thanks are due to our hosts, Professors J.F. Cariñena, M. Asorey and Dr. J. Clemente, and particularly Professors Luis J. Boya and Giuseppe Marmo. I also want to thank all other contributors, some of whom have traveled great distances. Finally, I am pleased to be with such good friends.

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