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Personal recollection

Some people and some problems met in half a century of commitment to mathematical physics

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Abstract. Personnal recollection of half a century of Mathematical Physics.

Beginnings

February 1946. Nine months after the end of World War II in Europe the doors of the "Technische Hochschule" (now "Technische Universität") in Stuttgart opened again and I enrolled as a student of physics. The town was still in ruins. In my mind I see Dr. Alfred Kochendoerfer – lecturer and distinguished solid state physicist – marching up the hill with a load of bricks he had dug out from downtown ruins, maybe ten at a time, to mend some walls of the physics buildings. The scarcity of many things, in particular space and laboratory equipment, demanded improvisation and rendered many regulations about the curriculum ineffective. This suited me very well. Perhaps I missed for the rest of my life some lessons of great educational importance but I survived without them and I gained adequate time to occupy myself with topics of central interest to me. Since I had entered the university with some previous knowledge in mathematics and physics, acquired by reading books, I concentrated from the beginning on theoretical physics. I listened to all the lectures of the two professors: Uz Dehlinger and Erwin Fues and I worked out each lecture in detail on the same day at home. In this way I wrote my own text books which were very useful to me later when I had to give lectures myself.

I enjoyed the lectures of Professor Dehlinger very much. They were not always reliable in details but comforting in their down to earth approach and lack of awe in front of great intellectual pinnacles. Thus I never forget his introduction to the Dirac equation: "Let's try to figure out what was on the mind of Dirac when he developed these equations. Well, one cooks with water everywhere...".

Professor Dehlinger had a small laboratory and agreed that I could work there to acquire the necessary credit for advanced laboratory work. Much to the dislike of the Institute for Experimental Physics which felt that I was cutting corners (which was probably true). While I felt quite at home in the jovial atmosphere surrounding Professor Dehlinger I was really fascinated by Professor Fues. His tall but frail, somewhat oblique figure (one wing of his lungs had been amputated in 1935 and in view of the prognosis of the surgeon it was a miracle that he was still alive), his well measured way of walking and of arriving at balanced judgments on any questions posed created the feeling of natural authority of a man who has thought deeply about many things and whose fairness was beyond question. He engaged me as his auxiliary assistant and suggested the problem I should solve in my diploma thesis. So I saw much of him and his family. In June 1948, his daughter Kaethe and I got married, just a few weeks before I received my diploma.

There was only one dark blot for me in those days: the final exam in experimental physics. The examiner, Professor Regener, famous for his experiments in early days of cosmic ray physics, was sore that I had not done any work in his laboratory and decided to teach me a lesson. After some twenty minutes of rather nasty cross examination he got me into a situation where my brain stood entirely still and I could no longer think about the simplest things. At the end he summed up: "Looking at your other grades I guess I should let you pass. There is no point in suppressing such one-sided talents. But I hope you recognize that you understand nothing about physics. Otherwise you might invent again a particle without charge and without mass which can never be detected." Prof. Regener's sarcasm was directed towards Wolfgang Pauli and his neutrino hypothesis which he regarded as a hoax as long as no possibility of detecting neutrinos was visible.

I left Stuttgart in 1949 to work for a Ph.D. degree at the University of Munich under the guidance of Professor Bopp. Fritz Bopp was one of the youngest professors in theoretical physics in Germany and the fact that he occupied now the chair which once had been the chair of Arnold Sommerfeld was evidence of the high reputation and great expectations placed on him. Sommerfeld was still around and quite active. We had just celebrated with him his 80th birthday, an impressive affair since most of the professors of theoretical physics in Germany had been his students at some time. Sommerfeld had retired soon after Hitler's grasp of power and his immediate successor was an affront to him. Sommerfeld found himself in the shooting line of the movement "German physics", led by Philipp Lenard and Johannes Stark, which was fostered by the ideology of the regime. These were people who believed that the growing abstraction in physical theory as manifested by the relativity theories and quantum mechanics was evidence of the poisonous influence of the Jews. In the files of the institute I found a letter of recommendation for the successor: "He is a real physicist, not an atom-dogmatic like Sommerfeld..." After the war the "real physicist" disappeared from the scene and I have not heard of any contribution by him to physics.

The main research project of our institute was – what Bopp called – "field mechanics". Starting from a modification of Maxwell's equations by the introduction of a non-local form factor the equations of motion of a charged particle involved additional degrees of freedom originating from the expansion with respect to the retardation. It was Bopp's hope that these degrees of freedom could provide a model for the spin of the particle. For quite some time this looked hopeful. We could cast the equations of motion in canonical form and found that the new degrees of freedom produced quantities whose Poisson bracket relations were the structure relations of SO(4,2) and that these in turn were the commutation relations of the matrices appearing in the wave equation of a particle with arbitrary spin (generalized Dirac-Kemmer... matrices).

The project of "field mechanics" showed signs of sickness and was buried quietly in the summer of 1951. Still, the two years I had devoted to this project were not wasted. The joint paper by Bopp and myself "On the possibility of spin models" [Bopp 1950] found wide interest world wide. In it we showed that the differential operators of angular momentum in the case of a top (where all three Euler angles enter) had systems of eigenfunctions to half integer angular momentum. The reason is: the commutation relations of the angular momentum operators define the Lie algebra of SU(2). In the case of the top we deal with the regular representation of the group whereas in the case of a particle, where only two Euler angles enter, we have a subrepresentation. The regular representation contains all irreducible representations, the other one only those with integer angular momentum. Thinking about "field mechanics" I was forced to consider unfamiliar aspects of the canonical formalism and quantization rules. Thus I treated rather exhaustively the degenerate Lagrangians where the velocities could not be expressed in terms of the canonical momenta. This problem was treated at about the same time by Dirac and his version known as the "Theory of Constraints" plays an important role in relativistic quantum theory. So I am sorry that probably nobody read my paper on this subject [Haag 1952]. The main reason was that it appeared in a journal which was read by engineers who were not likely to meet such degenerate situations; the other reason was that I was not Dirac who could afford to publish in any obscure journal and still reach an audience.

Copenhagen

On a high level, hidden from my view, the plans to create a great research center for high energy physics by a joint European effort had entered into a decisive stage. In 1952 Niels Bohr who had been one of the fathers and a most enthusiastic promoter of this idea arranged a conference in Copenhagen as a step towards this goal. The participants all fitted easily into an old seminar room at Blegdamsvej 17; so the number could not exceed fifty. But it was a wonderful mixture. There was a good dozen of real experts knowing the latest results in cosmic ray or accelerator experiments, of recent theoretical attempts in quantum field theory or elementary particle classification. But there was an equal number of eager young people at the post doc level from different European countries, and there were the great masters of quantum physics Bohr, Heisenberg, Pauli who did not try to dominate the scene. From Germany the young delegates were Gerhard Lueders and myself. This was not so much due to our achievements but to the fact that Lueders came from Heisenberg's institute in Göttingen while I came from Munich. These were the places with most prestige in fundamental physics in Germany. Needless to say, for a person like me, coming from a scientifically rather provincial atmosphere this conference was an absolutely fabulous experience. The step to the great world.

I recall the amusing controversy between Wightman and Pauli. Arthur Wightman was in Copenhagen on a sabbatical and he carried with him from Princeton the wisdom of Wigner and Bargmann. Pauli was the chairman of the discussion after a talk on the Møller-Kristensen proposal of a non-local quantum field theory. Pauli wrote three items which he wanted to be discussed on the blackboard. I have forgotten what they were. Wightman got up and said he wanted to discuss the second and the third together. Pauli: "That is not possible. They have nothing to do with each other." Wightman: "I want to discuss them together anyway." Pauli: "I object." Wightman: "After you have objected I may now proceed." On the next day Pauli agreed that Wightman was right after all.

Of great importance for my future work was a tea party at the castle where Niels Bohr lived. I got hold of Wightman and we walked many times around the lawn. We saw that we had many aspirations in common and when I asked him about infinite dimensional representations used by Wessel (which had interested Bopp) he told me that he did not know about Wessel but recommended in the strongest terms that I should read the 1939 paper by Wigner on the irreducible representations of the inhomogeneous Lorentz group (nowadays called the Poincaré group). After my return from the conference to Munich I dug out this paper [Wigner 1939] and indeed, it was a real revelation.

Here was a paper which formulated precisely what special relativity implied for quantum physics and answered basic questions. First of all Wigner recognized that the symmetry group in special relativity is the Poincaré group which is a semidirect product of translations and Lorentz transformations. It is not enough to consider Lorentz invariance and translation invariance separately. Secondly he recognized that in quantum theory where pure states are rays rather than vectors in a Hilbert space we need only representations up to a factor, not true representations of the symmetry group. It is this fact which allows the half integer values for the spin. After these preliminaries Wigner derived a complete classification of all irreducible ray representations of the Poincaré group. He found that those with positive energy were labelled by two parameters whose physical interpretation was mass and spin. Thus the simplest mathematical objects, (irreducible representations) correspond to the simplest physical systems (single particles). I could not understand why this paper, published in 1939, had remained virtually unknown to almost all theoretical physicist for thirteen years. Even in 1955 when I had to give a talk in Paris (due to a magnificent collective invitation of the Faculty of Munich University by the Sorbonne) I was introduced by Louis Michel with the words: "He is one who has read Wigner's 1939 paper." This was apparently enough of a claim to fame at that time. The reasons for this ignorance of Wigner's paper were twofold. For the down to earth theoretical physicists the paper appeared as an unnecessary mathematical escapade just describing things he knew all along in another language. For others the paper appeared to be impossibly difficult to read. That both attitudes were due to prejudice became apparent a few years later when the paper had become a piece of household equipment for almost every theorist working in quantum field theory.

I shared with Prof. Bopp a deep dissatisfaction with the standard interpretation of quantum theory as it was spelled out in the writings of Bohr, Heisenberg, or the books by Dirac and von Neumann. On our way home from the institute in the evening we often got so entangled in the discussion of such questions that we walked back and forth for an hour or more while our wives waited with the evening meal. The questions which bothered us ranged from the reality problem: "What happens if there is no observer?" to the unwillingness to accept the superposition principle as basic: Why are pure states described by rays in a linear space over the complex numbers? And why are probabilities given as absolute squares of complex amplitudes? Some of these questions have accompanied me throughout my life. I made some progress, changing aspects and localizing essentials, but I am still not satisfied with my or anybody else's understanding. Luckily I realized that the occupation with such questions can become a fatal disease and my sense of self preservation was strong enough so that I decided one day that first I must understand all applications of the quantum theoretical formalism before delving into questions about the interpretation. I talked to Fritz Bopp that it was our duty as teachers to present some more practical research program to the students. Fritz Bopp took his duty very seriously. After a long pause he answered "You are perfectly right but I see the solution of some questions right around the corner, so I cannot quit now. But next year I shall certainly change to some other subject." Unfortunately, the way around the corner was a long one and so Fritz Bopp spent his best years in a rather futile battle.

The 1952 conference in Copenhagen gave the starting signal for the "CERN Theoretical Study Group"[1] which was hosted by Bohr's institute in Copenhagen for a few

[1] CERN = « Centre Européen de Recherche Nucléaire ». Actually this is a misnomer. The plan was to build an accelerator yielding protons with an energy of 20 GeV which is a thousand times larger than the typical energy transfers in nuclear reactions. The objective

years until the construction of the laboratory in Geneva had progressed sufficiently to take over. The idea was that each member state of CERN should delegate one young physicist for one year to this group so that by the time the accelerator in Geneva was working there would be a stock of competent young theoretical physicists in Europe.

Again Gerhard Lueders was the first delegate from Germany and I was the second, starting my term in April 1953. The time in Copenhagen stands out in my memory as a particularly happy one. For the first six months we rented a summer cottage in walking distance from the shores of the Baltic Sea.

The task of directing the activities of the study group was assigned to Christian Møller. He handled it with great delicacy. The level of knowledge differed widely between the members of the group but everyone was allowed to work or study at his level. Officially we were supposed to concentrate on nuclear physics and on non-pertubative methods, specifically the Bethe-Salpeter-Equation and the Tamm-Dancoff-Method. In the first months I dutifully followed this line and studied nuclear physics by reading the book by Blatt and Weisskopf. But in the summer vacation when I stayed mainly at the cottage I fell from grace and returned to my hobby or obsession: the synthesis of quantum theory and special relativity beginning with the work by Wigner. In fall when I told this to Møller he was quite interested and suggested that I should give a couple of seminars on the subject. It soon became apparent that this topic met with wide interest. Thus my seminar talks continued on a weekly basis till spring 1954. Edith Abrahamson, secretary of the study group, devoted a lot of time and care to the production and distribution of my lecture notes, entitled "The Relativistic Quantum Theories of Interacting Particles" [Haag 1954]. I am deeply grateful for all the help I received from her.

The lecture notes had quite an impact. Apart from giving a somewhat popularized exposition of Wignerism[2] they contained some original contributions. Among them the demonstration that in quantum field theory (or for any system of interacting particles) the equivalence class of the representation of the Poincaré group is independent of the interaction. It depends only on the types of stable particles described and is explicitly known. This result was at first sight rather counterintuitive since the Hamiltonian – which is one of the generators of the group – contains a term characterizing the interaction. But this is due to the choice of variables in terms of which the Hamiltonian is written. It is not a purely group theoretical feature. The result killed a project of Wigner and Moshinski on which Wightman had reported in the 1952 conference: to construct a representation of the Poincaré group describing interacting particles.

Another topic concerned the construction of the appropriate Hilbert space of states in quantum field theory. Since one has an infinite number of independent variables, the algebraic relations (typically canonical commutation relations) do not fix the equivalence class of their representation by operators in Hilbert space. This had been noticed by several authors[3].

But the practitioners of quantum field theory, if they knew about it at all, regarded the "strange representations" as a mathematical curiosity of no relevance to physics. Thus it came as something of a shock when I showed that simple algebraic

was "elementary particle research" rather than nuclear research. I suspect that the name was chosen to persuade governments to give money for the project because nuclear physics seemed important due to the bomb and the atomic energy.

[2] The work by Wigner on symmetries, their representation and consequences such as the limits of localizability of relativistic particles.

[3] First by [von Neumann 1938], in this book by K.O. Friedrichs "Mathematical Aspects of the Quantum Theory of Fields" it appeared in a chapter entitled "Myriotic Fields" [Friedrichs 1953]; a coarse classification had been given by L. Gårding and A.S. Wightman [Gårding 1954a; 1954b].

substitutions change the class of the representation and that the customary treatment in Fock Space was inconsistent in quantum field theory with interaction. This was later called "Haag's theorem" by Wightman who mended my incomplete proof.

The notes also contained my first steps in collision theory replacing the then prevailing ideology of "adiabatic switching off of the interaction" by an appropriate form of convergence of sequences of operators or state vectors, mirroring the expectation that in a typical collision process the finally emerging stable particles will move farther and farther apart so that the interaction between them vanishes asymptotically. I called this expectation "the asymptotic condition". For the case of non relativistic quantum mechanics I claimed correctly the strong convergence of the Møller operator if the interaction potential decreases faster than r^{-1}. But I was too careless in attempting to transfer this result to quantum field theory by analogy and arrived at a wrong formulation of the asymptotic condition which as I noticed much later was even in conflict with other parts of the notes. The first correct form was given in the celebrated paper by Lehmann, Symanzik and Zimmermann [Lehmann 1955]. Their form of the asymptotic condition used weak convergence of field operators averaged by solutions of the Klein-Gordon equation. This led immediately to their elegant reduction formulae and the expression of S-matrix-elements in terms of vacuum expectation values of time ordered products of field operators. I first met Wolfhart Zimmermann in spring 1954 at a meeting of young German theoretical physicists in Oberwolfach, at that time a small idyllic village in the Black Forest. Years earlier the Mathematical Seminar of the University of Freiburg im Breisgau used an opportunity to buy a house there to give scientists the possibility of retiring for quiet reflection or meet other colleagues in small workshops. Gradually the site developed to a conference center. Affinity between mathematics and music was respected from the beginnings. There was a usable piano and a cello together with adequate literature. In later years this was raised to a luxurious level. An extra music room with a grand piano and space for a whole string quartet was added. The idea of organizing meetings where all young theoretical physicists in Germany could get acquainted with each other was put in action in 1951. From this time on there were regular meetings of young physicists lasting 10-12 days in Oberwolfach each spring.

In 1954, Wolfhart Zimmermann showed me their conjectured form of the asymptotic condition and asked me whether I had any objections. I had none but I still did not realize that my version was wrong.

Bohr's institute was a great attractor for theoretical physicists all over the world. This was due in parts to the scientific reputation, the glorious past in the twenties when this institute had been the workshop for the development of quantum mechanics as a coherent theory. But the attraction was also due to the very particular atmosphere of the place generated by the human side of Niels Bohr who considered all scientists as his friends and the members of his institute as his family. The Bohrs also had the means for hospitality on a large scale because the Carlsberg Foundation had stipulated that the greatest living scientist of Denmark should reside in the castle they owned and that he should benefit of all the services needed. On Christmas Eve the Bohrs invited all bachelors of the institute lest they felt lonely and on Christmas Day all the young families with children. There was a huge, richly decorated Christmas tree in the hallway and on the floor under it a large toy elephant with wheels on which Niels Bohr lifted my three year old son Albert and pulled him around the Christmas tree.

I shared the office with Nico Hugenholtz who came from Utrecht. He had been a student of Kramers and was an expert in statistical mechanics. Like many of his countrymen he had an aversion against Germans, stemming from experiences during the occupation of the Netherlands in the war. But the ice between us melted soon. Every day after lunch we took an extended walk together talking about anything from physics to politics. We became friends for life. It was many years later that we started our first scientific collaboration which turned out to be very fruitful.

On the top floor of Bohr's institute there was a cafeteria where one could get hot and cold beverages at lunch time. The food one bought across the street in a little shop which prepared individual sandwiches according to your order. Around noon you could meet most of the population of the institute in the cafeteria. Once when Wolfgang Pauli was visiting Copenhagen we talked at lunch about his reputation as a scavenger of theoretical physics who destroyed by his biting remarks every seminar speaker who was not absolutely sure of himself. Dr. Alder from Zurich who had been Pauli's assistant reported that recently Prof. Kronig from the Netherlands had given a talk in Zurich when Pauli attacked him so viciously that Kronig almost started to cry. I was outraged and said: "Kronig is a distinguished scientist with important work to his credit. Why should he take Pauli's criticism so serious?" Mrs. Hellmann, the very resolute chief secretary of the institute was sitting at the neighbouring table. She got up, looked sternly at me, and said: "Young man, when you get older I hope for you that you take the opinion of Prof. Pauli very very serious. Even Prof. Bohr listens to Prof. Pauli." This had a funny sequel. Next afternoon we were all invited for tea to Niels Bohr's residence, the Carlsberg castle. It so happened that while most guests were still walking in the garden Pauli and I entered the tea room and sat down at the same table. Pauli was in a benevolent mood and we had a pleasant conversation till Mrs. Hellmann approached with the rhetorical question "May I join you?".

In civil life outside the boundaries of science Pauli could be pleasant, even charming. But when problems in physics were concerned he pushed the demands of politeness and tolerance aside. I guess there were several reasons for such aggressiveness. First of all he did not suffer a fool lightly and felt it his duty to keep the temple of physics free from imposters and mediocre contributions. This was probably healthy for the field. Looking at the swamp of irrelevant papers being produced now, one might be inclined to wish for a person of the strength and sharpness of Pauli. Another aspect was his understanding of rhetoric battles with sharp insulting wit as a sportive effort, offering his opponent the chance for revenge at a forthcoming talk by Pauli. But he must also have had a diabolic urge to insult famous people if he disagreed with some recent paper. Some letters to Albert Einstein or to Hermann Weyl were full of insulting sarcasm. Once Niels Bohr told me chuckling: "The only person Pauli was afraid of was his old teacher Arnold Sommerfeld."

Pauli's favourite among the younger theoretical physicists was Gunnar Källén and in the subsequent years also Harry Lehman. Källén was something like a star in the scientific community of Sweden and shared the quick grasp of problems, the thorough knowledge as well as the fighting spirit with Pauli. As a Professor in Lund he was not far from Copenhagen. So he came often to lecture on his work on quantum electrodynamics. I did not understand much of these lectures and got from them mainly a dislike for the state of the art. When Pauli was around, Källén and Pauli would sit in the cafeteria discussing the best strategy for dismantling the next seminar speaker. But Gunnar Källén was fair and honest. He never attacked younger, insecure persons but tried to help them. In subsequent years we were on very friendly terms. We met at many conferences or workshops in various countries till his tragic untimely death.

One day at lunch Niels Bohr approached my table and said "I have again received a manuscript on the foundations of quantum mechanics by Prof. Bopp. I cannot understand why he is worrying about issues that have been clarified long ago while there are so many fascinating new problems." Thinking of my many discussions on this subject with Bopp I answered somewhat rashly: "Maybe these things are not as clear as you think." A few weeks later I got a call from Niels Bohr's philosophical assistant Dr. Pedersen: "Prof. Bohr would like to speak to you. Could you come over to his office?" Niels Bohr greeted me cordially and, after a few general remarks he said: "But now you should tell me what your problems are." Unprepared as I was I mumbled a few sentences about reality till Bohr interrupted me: "Twenty years ago Dirac came to me with exactly the same questions and I had to talk to him for a whole week to get it out of him." Then he proceeded to give me a condensed version of his discussion with Dirac walking up and down and around the table which stood in the middle of the room. I did not understand much and remember only the words "of course you can change the mathematics but this changes nothing in the fundamental aspects like complementarity." He ended by saying "nothing you told me has given me the slightest indication of where your problems really lie." I could not resist saying "Perhaps that is because I did not say anything in the last hour." Then I asked him to let me speak for half an hour without interruption and reply afterwards. Of course this was again highly inappropriate and Bohr justly declined. Again weeks later we met in the morning at the bicycle stand and Bohr said "I have been thinking about it and we shall have that discussion in form of a seminar." The seminar took place. It was not helpful. I was nervous and Bohr was annoyed. I tried to argue that we did not understand the status of the superposition principle. Why are pure states described as rais in a complex linear space? Approximation or deep principle? Niels Bohr did not understand why I should worry about this. Aage Bohr tried to explain to his father that I hoped to get inspiration about the direction for the development of the theory by analyzing the existing formal structure. Niels Bohr retorted: "But this is very foolish. There is no inspiration besides the results of the experiments." I guess he did not mean that so absolutely but he was just annoyed. Still it indicated the difference between a true physicist and a mathematical physicist. Dr. Alaga the member of our group from Yugoslavia summed it up: "You have been talking about different things in different languages." Five years later I met Niels Bohr in Princeton at a dinner in the house of Eugene Wigner. When I drove him afterwards to his hotel I apologized for my precocious behaviour in Copenhagen. He just waved it away saying: "We all have our opinions."

Munich again

Returning to my old assistant's position in Munich in spring 1954, I began to condense my Copenhagen lecture notes to a "Habilitationsschrift" in German language. The next task was to rewrite the matter for publication in the Danish Academy, now again in English language. It appeared there finally in 1955 [Haag 1955a].

A different problem which attracted my attention in those days concerned the classical theory of interaction between an electron and an electromagnetic field. The conventional wisdom is that the electron radiates when accelerated and that the acceleration is given by the Lorentz force exerted by the "effective field" which is the total field minus the "self-field of the electron." When the electron is in uniform motion the self-field is just the Coulomb field in the rest system. But what is it when the electron is accelerated? In a beautiful paper in 1938 Dirac had treated this problem in a relativistically covariant fashion [Dirac 1938]. The equation of motion for the electron he obtained was a third order differential equation which seemed to lead to some paradoxes. In particular it had unphysical solutions, so-called "run away solutions", which had to be excluded by a "finality condition". This had led to some mystification in the subsequent literature. I found [Haag 1955b] that, while all the tough calculations could be taken over from Dirac's paper, the proper formulation of the initial condition for electron and field (the "asymptotic condition at $t \to -\infty$") removed most of the paradoxes. Instead of the third order differential equation one has an integro-differential equation which has a unique physically reasonable solution to prescribed initial data consisting of the asymptote to the world line of the electron at $t \to -\infty$ and the incoming field, provided the latter is not excessively strong and fast oscillating. So it is a theory covering most practical purposes but there exists probably no classical theory for the coupled system of electron and field which covers all possible cases. The problem has been followed up by Fritz Rohrlich who devoted a whole book to a detailed study [Rohrlich 1965].

Fritz Bopp and Wilhelm Maak, one of the younger professors of mathematics at Munich University, decided to activate the dialogue between theoretical physics and mathematics. The result was a weekly meeting whose attendance quickly dwindled until finally there were only Bopp and myself representing theoretical physics and Maak with two assistants on the mathematics side. For me these meetings were of essential value. I was introduced there by Dr. Thoma to the work of Gelfand and Naimark in Russia on involutive algebras as well as to the work by von Neumann and Murray on Rings of Operators. I bought a book which was a translation into German of several survey articles by Naimark. This became my bible in functional analysis for many years.

Quite a number of famous scientists from foreign countries visited Bopp's institute in those years (1954-1956). Most exciting for me was the visit by Eugene Wigner. He shared with Bopp the idea of representing impure states by a pseudo-probability distribution in phase space. They had both published some paper about it. This is nowadays called the Wigner function. Wigner told me that he had read my Copenhagen lecture notes and liked them. I asked him whether there was a possibility for me to visit Princeton for a year. He answered: "That is why I am here." The result was an offer of a visiting professorship at Princeton University for the academic year 1957/58. A fabulous offer indeed for me at this time.

Due to the initiative of Wolfhart Zimmermann I received an invitation to work for the academic year 1956/57 at Werner Heisenberg's institute in Göttingen. Fritz Bopp was very helpful arranging a leave of absence for me from Munich University starting in fall 56 and being prolonged again and again till it was clear that I would not return to Munich in the foreseeable future. For the institute my leaving was no loss. Bopp was able to attract excellent replacements to whom he could offer the chance to obtain the Venia Legendi at Munich University. There was Karl Wildermuth. He built up a nuclear physics group at the institute. As a former assistant of Heisenberg he emphasized physical intuition rather than mathematical refinement. There was Gerhard Höhler, a former assistant of Richard Becker in Göttingen who was an expert in the many body problem. Finally there was Georg Süssmann whose always active mind jumped on every intellectual puzzle. He had come from Carl Friedrich von Weizsäcker with whom he shared the wish for universal knowledge.

Wanderjahre
Göttingen – Princeton – Marseille

In the "good old days" prior to World War I, a strictly observed tradition among handicraft men like carpenters, tailors demanded that after finishing their apprenticeship they should move around for several years, working in different places and see something of the world till, when returning, they could open their own shop as masters settling down and raising a family. In our fast moving times this excellent tradition could not last among handicraft men. But in my generation it prevailed among scientists with one difference: most of us were married and had children before starting to move around. My wife Kaethe and I had two boys at that time, the younger one being born in 1954 in Copenhagen. Therefore the Wanderjahre were possible for me only because Kaethe considered the family as the center of her life and the creation of a home for the children under sometimes rather make-shift conditions as the most important task, coupled with a certain pioneer spirit enjoying the challenges and experiences.

The first station was the Max-Planck-Institute for physics in Göttingen, specifically the part within it directed by Werner Heisenberg. This was where a few years earlier the trio consisting of Harry Lehmann, Kurt Symanzik, and Wolfhart Zimmermann, dubbed by Pauli the "Feldverein", had formed and produced their work on the formulation of quantum field theories which had quickly become a classic. Now Zimmermann was orphanized. Symanzik had gone to the USA on a fellowship, Lehmann had spent the year 1955/56 in the CERN study group at Copenhagen and, immediately after his return, he was installed as a full professor in Hamburg. This was a rather singular case since Lehmann had not even bothered to obtain a habilitation. Instead he had the decisive support by both, Heisenberg and Pauli. In order to fill the gap in the quantum field theory group in Göttingen the institute had invited me and Kazuhiko Nishijima, a highly distinguished young physicist from Japan. Of course there were many other groups in the institute engaged in areas such as plasma physics, cosmic rays, astrophysics…

Heisenberg was working at this time rather isolated on an extremely ambitious project: a fundamental theory of elementary particles explaining the multitude of known stable or metastable particles in terms of a single basic field. There was a big difference in approach between him and the younger people at the institute. For him who before the age of thirty had become one of the world's most famous scientists and who had continued to produce important ideas for many years the direction of quantum field theory appeared a dead end road. Like Dirac he was repelled by the development of renormalization theory which he called a tactical advance but a strategic disaster, deviating attention from the fundamental problems. The young people on the other hand believed that this was not the time for great ideas supported by hand-waving arguments but that a lot of careful consideration of the strengths and weaknesses of existing quantum field theory was called for, an analysis of concepts, mathematical tools, as well as technicalities.

But also outside of Göttingen in the older generation Heisenberg found little support. Bohr who believed that, like at the advent of quantum mechanics, one needed "crazy ideas", felt that Heisenberg's approach was not crazy enough. Møller deplored that while he had always profited much from Heisenberg's talks in the past this was no longer so. Pauli complained that he had to listen again to Heisenberg's "swamp blossoms". I was essentially of the same opinion as my friends of the "Feldverein" but had some more sympathy with Heisenberg's ideas and was looking forward to discussions with him. I shared the office with Zimmermann and – as long as my family had not yet come to Göttingen – I slept in the guest room across the floor from our office.

In these weeks when both Nishijima and I were temporary bachelors we often went out in the evening together. I remember us walking on the remnants of the old city wall often circling the town several times talking about customs and attitudes in our countries, rarely about physics. We did not anticipate then that we would be spending the next ten years always in the same places, sometimes even sharing the office and that our chance meeting would grow to a solid friendship with the years.

At this time an idea occurred to me which at first I considered to be mainly of aesthetic value but which turned out to be so fertile that its elaborations and applications determined the direction of my work for many years. It started from the widespread discomfort about the role of quantum fields as the basic observables. It appeared to be far removed from experimental practice in high energy physics. This had induced Heisenberg 14 years earlier to search for the truly observable quantities in elementary particle physics. It led him to the concept of the S-matrix and the attempt to determine the S-matrix-elements directly from basic principles. He had abandoned this project long ago. Now in 1956 he used to say: “The S-matrix is the roof of the theory, not it’s foundation.” But what are the basic observables? Obviously the essential instruments in high energy physics are detectors. The task of a detector is to give a signal from a specified region in space at some time. It does not measure a field or an S-matrix-element. It just indicates some property within a definite space-time region and a priori we do not even know which property. It is a long process to improve the sensitivity and selectivity of detectors and to gain information by testing different geometric arrangements of detectors. My conclusion was that the theory must give us for each region of space-time an algebra corresponding to the set of all observables or operations pertaining to the region. This correspondence between space-time regions and algebras is the content of the theory; nothing more nor less. Relativistic causality demands that the algebras of two regions which lie space-like to each other should commute. In the case of a field theory the algebra of a region is generated by the fields “smeared out” by test functions with support in the region. But there may be other possibilities of construction.

Heisenberg’s ideas about the mathematical structure of his prospective fundamental theory had become somewhat more concrete. Instead of an unspecified “Hilbert space II” which previously had served as a Deus ex Machina to absorb difficulties, he now settled on a Hilbert space with indefinite metric. The problem was then to define “the physical states” so that transition probabilities between them remained positive and the physical S-matrix remained unitary. A simplified model of a quantum field theory had been devised by T.D. Lee. It could be decomposed into a set of sectors with increasing complexity but each with a finite number of degrees of freedom. A modification of this, living in a Hilbert space with indefinite metric, had been presented by Glaser and Källén [Glaser 1956]. It could serve as a testing ground for the problems mentioned. Heisenberg asked me to look into it. This was the beginning of a brief but very intense collaboration between us. I saw quickly that there was no problem in the lowest sector. A unitary S-matrix for the physical states existed there. But my method could not be extended to the higher sectors. So I felt that the major part of the work was lying ahead of us. Heisenberg on the other hand was optimistic and impatient saying “We know now how it goes. The rest is cold coffee.” We could not agree on the status of the cold coffee and so Heisenberg continued alone, writing up a paper for publication in a few weeks [Heisenberg 1957].

At roughly this time I made decisive progress on the formulation of a general theory of collision processes. The question was: how do we get from single particle states to the states describing configurations of incoming or outgoing particles? In the many discussions with Heisenberg we had come across this question. Heisenberg said “We just multiply the single particle states.” I objected: “There is no natural product of vectors in Hilbert space. One can multiply the creation operators but the choice of a creation operator is highly non-unique. You can add an arbitrary destruction operator which gives zero when applied to the vacuum but not when applied to other states.” Somewhat later, thinking about the physical interpretation of my algebras of local observables, it occurred to me that the choice of a creation operator for an essentially localized single particle state must be restricted to elements belonging (essentially) to the algebra of the region of localization. Furthermore this restriction suffices to yield a unique definition of asymptotic many particle states because at asymptotic times the relative distance between various particles tends to infinity and an essentially local destruction operator commutes with the (local) creation operators of other particles so that it can be shifted till it hits the vacuum where it gives zero.

In late spring 1957, I met Leon Van Hove for the first time. I had made a short trip to the Netherlands, mainly to see Wigner who spent a term in Leiden but also to visit Utrecht where my friend Nico Hugenholtz was a lecturer and Leon Van Hove was the boss of the theoretical physics. When I told Wigner that I was going to give a talk on the treatment of collision processes in quantum field theory in Utrecht Wigner made one of his slightly enigmatic remarks: “You know, poor John von Neumann is dead. But Van Hove is as good as anybody.” I remember this so clearly because I kept wondering whether he wanted to emphasize that von Neumann had been a singular light, incomparable with anybody or whether he wanted to put Van Hove in the same class as von Neumann. Anyway I gave my talk at Utrecht and Van Hove was quite unhappy with it. In the discussion at the end we could not straighten out our differences. So after half an hour the audience became restless and wanted to go out for lunch. So Van Hove closed the seminar and we decided to continue our discussion at the Hugenholtz home where Nico’s wife Ankie had prepared an excellent meal. Van Hove started: “Now let us go very carefully through it point by point from the beginning.” He did and it did not take him longer than 10 minutes to realize that his objections had been unfounded. So we could eat lunch in full harmony. Later in the day when I went for a walk with Nico he suddenly started laughing: “This is the first time I have ever seen Van Hove admitting a mistake. But then he rarely makes one.”

Not much later there was the International Conference On Mathematical Aspects Of Quantum Field Theory in Lille, France. It was probably the baptism of mathematical physics as a discipline distinguished from theoretical physics. Among the participants there were pure mathematicians like Laurent Schwartz who had become very famous recently as the creator of the theory of distributions legalizing Dirac’s δ-function and other generalized functions. He was a member of the French Bourbaki group, engaged in condensing all mathematical knowledge in an optimised form in encyclopaedic fashion. – There was K.O. Friedrichs, from the Courant Institute in New York who had emigrated together with Courant from Göttingen after Hitler’s ascent to power. He had written a book on mathematical aspects of quantum field theory and he told me the motivation for that. He had been fascinated by the articles about renormalization in the Physical Review, feeling like an archaeologist coming across the hieroglyphs. “Obviously these were messages from highly intelligent people, but what did they mean?” – And there was Irving Segal from Chicago who had developed the theory of involutive Banach algebras (called C*-algebras) parallel to the Russian mathematicians Gelfand and Naimark. In his contribution to the conference Segal proposed to ignore the problem of inequivalent representations of the canonical commutation relations or whatever other algebraic relations were used and to consider instead observables as elements of an abstract C*-algebra rather than operators in Hilbert space. The physical states corresponded then to positive linear forms over the algebra. We physicists, as practitioners of quantum field theory, regarded this as the typical brain child of a mathematician who had never stooped to do any calculations in quantum field theory. Indeed the physical interpretation was missing and Segal’s remark that the S-matrix could be viewed as an automorphism of the algebra led astray since we do not need an S-operator, let alone an S-automorphism but a matrix in a very specific basis given by the configurations of incoming particles. Looking back now I see that our arrogant rejection of Segal’s proposal was not warranted. Had I thought seriously about it then I could have supplied a good deal of the missing interpretation, namely the algebraic simulation of detectors and coincidence arrangements. Six years later Daniel Kastler and I came back to the idea of using abstract algebras. Now of course with a lot more insight into the structure of the theory.

For the theoretical physicists at the conference this was the time for injecting the theory of distributions and that of analytic functions of several complex variables into the discussion. The opening address was given by Arthur Wightman who presented his program of an axiomatic approach to quantum field theory, taking from traditional quantum field theory all features which could be cast in precise mathematical language regarding these as "axioms" and working out the consequences. There was the hope that with the progress of understanding more information could be injected so that the scheme could be narrowed. At the time there was one important consequence: the vacuum expectation values of products of field operators, subsequently called "Wightman Functions", which for real arguments were tempered distributions in the sense of Laurent Schwartz, turned out to be boundary values of analytic functions of the complexified arguments. Thus entered the theory of analytic functions of several complex variables and with it the task of extending the domains of holomorphy to natural domains. Gunnar Källén reported on joint work with Wightman, trying to find the most general form of the 3-point function. He took pains to stress that he had an entirely different philosophy of physics than Wightman but that their collaboration worked anyway quite well. – Of more immediate practical interest were the analytic properties of retarded functions in momentum space since they led to dispersion relations of the S-matrix which could be checked by experiments. Res Jost and Harry Lehman reported on their collaboration to combine the x-space and p-space analyticity and to extend the existing proof of dispersion relations. – Van Hove discussed lessons to be drawn from a model devised in Utrecht. – I presented my ideas on local algebras and collision theory. At the end I could not resist saying: "It is a shame that till now nobody has thought properly about the general formulation of collision theory without introducing the notion of a "free Hamiltonian", awkward in rearrangement collisions and impossible in quantum field theory." Somebody in the audience said: "There is a recent paper about this by Hans Ekstein in the Physical Review." [Ekstein 1956]. Back in Göttingen I looked up the paper by Ekstein and indeed he had proposed essentially the same procedure. I had already written up my contribution [Haag 1959] for the conference so I could just add one footnote referring to the paper of Ekstein. Half a year later Ekstein was visiting me in Princeton and said: "If you had not written that footnote I would have thought that you were a crook." As it was we became good friends and I spent many evenings in his home in Chicago in later years.

The conference had been organized by Louis Michel and supported financially by French sources. Therefore the official language was supposed to be French. Since most of the talks were given in English this implied a lot of work for Louis Michel who had the unpleasant task of translating all contributions into French. He accomplished this without complaining in a most unselfish way.

Meanwhile in Göttingen some remarkable development cast its shadows. Pauli's attitude towards Heisenberg's projected theory had changed. Heisenberg showed me some letters by Pauli in which he pointed out various things which should be done and asked questions. I could not make too much sense out of these proposals but registered that Pauli had abandoned his negative attitude and his jokes about Heisenberg.

In fall 1957, just before I left for the USA, we had a star studded conference in Oberwolfach, organized by Gerhard Höhler. There were Heisenberg, Pauli, Heitler, Jost, Lehmann, Källén, and many others. I was supposed to justify Heisenberg's use of the indefinite metric. After the first five minutes of my talk Pauli asked a question and I replied that I did not know. Pauli said: "Then I am not interested in the rest" and he wanted to get up and leave. But Lehman and Källén sitting at his sides were pushing him down by his shoulders saying: "You stay here." So Pauli had to suffer through the whole of my talk but at the end when I had a silly slip of the tongue he jumped up exclaiming: "You see! Everything you say is wrong and the more clearly you want to say it the more wrong it becomes." This makes me think of a few other conversations with Pauli. The next one was: "Everything you say is right but it just does not interest me." Then in 1958, a few months before his death, at the conference in Varenna he said: "What you do I do not understand anyway. I give it directly to Res Jost. So after all I like you quite well." This was most comforting.

I had gone to Princeton with the idea of profiting from the wisdom of Wigner, possibly entering in a collaboration with him in an area of common interest. In addition I intended to join forces with Arthur Wightman in his program of an axiomatic approach to quantum field theory. Both of these intentions did not really materialize. On the personal level I felt quite close to Wigner. He treated me as a young friend, inviting Kaethe and me often to his house. And he left his proverbial politeness aside when talking to me. Thus, when I iterated my complaint that it was a shame that nobody had looked properly at the general formulation of collision processes prior to 1956 he countered: "Surely somebody must have done that. Perhaps it was Mr. Mott. But that was a few years before you were born." Or, when he pointed at the heap of preprints cluttering all tables in his office: "Look at that! If I was sure that the person who sends me such a manuscript had thought about the problem, maybe half as much as I thought about it, I would be willing to read it. But since it is usually not the case there is no point..." Once he came to join Valja Bargmann and me at our weekly meeting. Valja greeted him with "welcher Glanz in meiner Hütte". I talked about my plans and at the end I said to Wigner: "I guess these things do not interest you very much." His laconic answer was: "That is an understatement." It was many years later when I found out the reason for this disinterest. At a talk I gave he remarked at the end: "Many years ago we had shown that the idea of strict locality cannot be upheld in relativistic quantum theory." Walking with him to a coffee shop after the talk I asked him: "Do you refer to the paper by Newton and Wigner?" He was surprised: "Oh, you know that paper?" "Yes, of course. But why do you want to mark a point in space-time by the position of a particle?" He said "How else do you want to do it?" I argued that it could not be done by a single particle because of the restriction of the energy-momentum to the mass shell. The point in space-time is approximated as an attribute of a high energy event. – I do not know whether I could convince Wigner or whether it was my fault that no fruitful scientific exchange between Wigner and me developed during my time in Princeton. Perhaps I was too much living in my own ideas and not enough prepared to listen to others. But maybe it was just that Wigner was busy with too many other chores.

Somewhat better was my interaction with Arthur Wightman. I took part in advising one of his Ph.D. students (J. Lew) and we had discussions about many things. But our paths did not join. In fact I felt rather aside from the trends of the time which concentrated on the analytic properties of various functions (Wightman functions, retarded functions, S-matrix elements). I had no talent for the intricacies of the theory of functions of several complex variables and I had an instinctive dislike for the use of such methods in physics because they were too fine grained and did not lend themselves to qualitative statements. Anyway, apart from the lectures I had to give, there were some mopping up operations. One was the finishing of a review paper in collaboration with Wilhelm Brenig, a young, extremely bright assistant of Richard Becker in Göttingen [Brenig 1959].

The other was a paper about the treatment of collisions of "composite particles" in quantum field theory based on my talk at the Lille conference (see [Haag 1959][4]). Nishijima had written a paper on this problem some years earlier and Zimmermann had recently devised another method generalizing the LSZ formalism. Curiously enough Nishijima, Lehmann, and Zimmermann had come to Princeton in the same year as guests of the Institute for Advanced Study while I was at Princeton University. So I thought it would be a good idea if we three got together and wrote one authoritative paper on the subject. However in our discussions about this project it turned out that Zimmermann and I had quite different opinions on what was important while

[4] The English original is available as an historical document in [Haag 2010].

Nishijima did not take any sides in this controversy. So ultimately we wrote three separate papers [Haag 1958; Nishijima 1958; Zimmermann 1958].

Meanwhile the news had spread that Heisenberg and Pauli were about to present a fundamental theory of elementary particles. Among the people who were in Princeton at this time I was the one who had had the most recent contact with Heisenberg. So Oppenheimer asked me to give a seminar and tell what I knew about this. I did not know much and told Oppenheimer that I could tell all I knew in about half an hour. Actually the seminar lasted with all the questions asked for about two hours; Oppenheimer's dry comment: "That was a good half hour." The more serious test came a month later when Pauli came to the N.Y. meeting of the American Physical Society and gave a talk in a closed session about this theory. I was not at this session but heard from various eye witnesses about the development of this catastrophe. On questionings by Feynman, Pauli suddenly realized that he held nothing in his hands and finished "I am beginning to suffer from the withering effects of my own criticism." During his subsequent stay in California he dissociated himself completely from this theory and began again to make jokes about it. He could not forgive himself for having yielded for a short while to the seduction by Heisenberg's ideas.

My contract with Princeton University was extended till fall 1959 and for the academic year 1959/60 Daniel Kastler had arranged for me a guest professorship at the University of Marseille. So I was under no pressure to produce any results fast. I could enjoy leisurely the neighbourhood of so many first rate physicists. Theoretical physics at the university was represented by Wigner, Wheeler, Bargmann and, in my age group, by Goldberger, Treiman and Wightman. At the Institute for Advanced Study there were Oppenheimer, Yang and Lee, who had just received the Nobel Prize, Dyson, Pais and half a dozen of visitors. I saw a lot of Murph Goldberger and Sam Treiman. For some time I shared the office with Goldberger and at noon we all went out together for our "brown bag lunch" at the cafeteria. Murph and Sam were very pleasant company. Both were open, lively, imaginative, equipped with a good sense of humour. These were the years in which the US Armed Forces spent the surplus money they had on contracts with scientists often concerning most esoteric projects without any relevance for military applications. After the visit to Princeton by some colonel in charge of such contracts Sam Treiman asked Arthur Wightman "What do you intend to do now for your country?". Wightman's reply: "Vacuum expectation values". Murph had some affinity to big money without possessing it. But in his later position as President of the California Institute of Technology he had indeed to deal with millionaires and billionaires soliciting donations for the institution.

A special place for me is taken by Valja Bargmann and his wife Sonja. He was a man of profound knowledge, good taste, and fine humour and he was interested in what I had to tell. He gave his advice in a most unselfish way to all who asked for it. Moreover he was an excellent pianist. In his student days he had sometimes earned his livelihood by playing in movie theatres supplying background music to the action on the screen in the era of silent movies. – I was a mediocre violin player but Bargmann was gracious enough to accompany me on the piano. So I spent many an evening at Bargmann's home where we made music and talked about many things.

In summer 1958, there was a great international workshop at Varenna on Lake Como in Italy, memorable for many reasons. Among the participants were Heisenberg and Pauli which made me worry as to how they would face each other after the short interlude of Pauli's joining Heisenberg's dream project and his subsequent desertion. I was surprised and glad that they met friendly as if nothing had happened. They jointly urged Källén, Lehmann and me to engage in a dispute about the merits of "adiabatic switching off of the interaction" defended by Källén, versus asymptotic weak or strong convergence. – Pauli was suffering from pains and complained "I feel that Heisenberg still lies in my stomach". We did not realize that these pains indicated a fatal illness. He died a few months later.

There were lectures by Wightman on axiomatic field theory, by Michel on group theory, by Lions on distributions. I learned from Lars Gårding that all topologies I might ever encounter as a physicist were defined by seminorms. There were several (then) young couples with small children whose lifelong friendship began at this workshop. Among them Daniel and Lisl Kastler, Steven and Hilde Gasiorovicz, Nicola and Elisabeth Khuri. There were young graduate students and post docs from various countries, among them Regge, Froissard, Ruelle who became well-known a few years later. My lecture at the workshop, entitled "The Framework of Quantum Field Theory", inspired David Ruelle for his basic paper [Ruelle 1962] in which he proved the fast decrease of the truncated Wightman functions and made the arguments of my paper [Haag 1958] watertight. Last but not least I should pay tribute to the beautiful setting in the villa Monastero, now the Enrico Fermi summer school, where you could step into Lake Como among an abundance of flowers.

One day in Princeton a young graduate student from Japan entered my office with the words: "I want to talk to you". His name was Huzihiro Araki and I had heard about his reputation as a brilliant student far ahead in comparison with others of his age. He enquired whether I could suggest a thesis problem and act as his advisor. I wondered why he should choose me, a young visiting professor, among so many famous scientists. Indeed there was a slight misjudgement involved on his side. He had judged my age by the scarcity of hair on my head and was later very surprised to realize that I was only ten years older than he. But there was a lot of common ground in our interests. Although I dutifully pointed out my lack of experience we ultimately decided to go ahead. I suggested the problem of proving from basic principles the fast decrease of truncated Wightman functions in space-like directions which I had stated as a conjecture in my paper [Haag 1958]. After a rather short time Araki had obtained the answer for the special case where the time coordinates of all points were equal in some Lorentz frame. The argument was too simple to qualify for a Ph.D. thesis and the method used could not be modified to treat the generic case. The answer in the generic case was obtained by Ruelle [Ruelle 1962] a few years later. His key idea was quite simple but it had not occurred to us. So I suggested to Araki another problem: to study the relation between the Hamiltonian and the representation class of canonical commutation relations. Here Araki obtained soon beautiful results and constructed interesting models. But my stay at Princeton was over before he could hand in his thesis. So ultimately Arthur Wightman had to step in and take the responsibility for the formal procedure.

It was probably in spring 1959 when I got a letter from Hans Frauenfelder inviting me to give a colloquium talk at Urbana/Illinois. It fitted well with other plans for a visit to the Middle West. I had previously promised to visit the University of Iowa for a week and I had begun a collaboration with Fritz Coester at the Argonne lab related to the thesis by Araki [Coester 1960]. We used a description of states by Schrödinger type functionals of the fields at a sharp time, discussed the linked cluster theorem and showed that the Hamiltonian is determined by the generating functional of equal time Wightman functions. The paper was not useful in relativistic field theory because there the equal time quantities are ill defined.

The colleagues in Iowa were Joseph Jauch and Fritz Rohrlich, authors of the best selling text book on quantum electrodynamics entitled "The Theory of Photons and Electrons". Meanwhile Jauch had experienced an "awakening" (his own words) rejecting the sloppy arguments of theoretical physicists with which he had been happy for many years and striving for high standards of mathematical rigour. I felt that the results of this transformation had not been beneficial. The obsession with rigour can have a stifling effect because we have a limited amount of time and energy to concentrate our efforts and if we apply all in one direction we are liable to loose the view of the landscape. Jauch had written a series of papers on collision theory in this style. My impression of these papers could be summed up in the words used by Pauli on other occasions: “They help where no help is needed and they do not help where help is needed.”

I have no memory of my colloquium talk in Urbana but I must have left a reasonably good impression because a few weeks later I got a letter from Fred Seitz, the head of the physics department, offering me a senior position in his department. Soon afterwards Wigner asked me what I thought about this offer. I said that I should return to Germany now but would be glad to come to the USA again on some later occasion. Wigner was visibly disappointed and just said: “Maybe there will be no other occasion.” I understood that I owed this offer to Wigner’s recommendation and, thinking seriously about it for one night, I realized that, of course, I should accept it. As a full professor in an excellent physics department where Fred Seitz had promised that I could always invite two research associates I would have ideal conditions to work. Kaethe agreed to undertake the venture and I felt confident that the family would soon feel at home in the friendly atmosphere at Urbana. Moreover, as Wigner pointed out, the salary would be adequate to allow us to visit Germany often.

Fred Seitz wanted me to start rather immediately. We finally resolved that by a compromise between Marseille and Urbana. I would go for one semester to Marseille and then come to Illinois.

Urbana

The seven years we spent in Urbana were happy ones for all members of our family. The landscape, though not spectacular, was pleasant and, as far as general living conditions and cultural liveliness were concerned, I appreciated the wisdom of the words by which Fred Seitz welcomed the foreign students and fellows in autumn. “A place of great physical beauty is liable to become a slum in a short time. The true Shangri-Las of our world are places like Urbana, Illinois”[5].

Indeed, in no other place did I go to hear so many concerts; faculty recitals, student recitals of the excellent music school and, each year in winter, some concerts by world famous artists like Isaac Stern or Rudolf Serkin. There were theatre activities and even a very good opera department, invited lectures by high ranking politicians or intellectual leaders. All this easily and quickly accessible without worries about entrance tickets. Last, and most important, there was a benevolent cosmopolitan spirit pervading faculty, graduate students and guests who came from many different countries.

Nishijima had received and accepted an offer of a professorship in Urbana in the same year. For some time we shared our office. It was a large room with a black board for each of us in a different corner where we gathered our collaborators or students for discussion. I asked Nishijima how we could arrange that we did not disturb each other by such discussions. His philosophical answer: “He, whom it does not concern, he does not listen”.

In the first years, I had invited Huzihiro Araki and Bert Schroer to come to Urbana as my research associates. Schroer had just finished his diploma in Hamburg and had not yet acquired a Ph.D. I relied on the strong recommendation by Harry Lehman and was not disappointed. Our first joint paper was finished after a few months [Araki 1961].

[5] Shangri-La, the utopian place in the novel “Lost Horizon” by James Hilton.

A landmark in the perception of mathematical physics was the gathering at Boulder, Colorado in summer 1960. The American Mathematical Society had organized a summer seminar on applied mathematics which really aimed at mathematical physics as was clear from the key note speech by Marc Kac: "A mathematician looks at physics. What divides us and what may bring us together." Parallel to this seminar the physics department of the University of Colorado at Boulder ran a theoretical physics summer school in adjoining premises. There was a lot of crossover between the two groups. I lectured at the physics summer school while Valja Bargmann and Res Jost were part of the mathematics seminar. Many activities like excursions, hikes in the mountains were jointly organized. There was good will on both sides to learn from each other. Mathematicians hoping to get familiar with problems bothering the physicists especially at the frontiers of development. Physicists hoping that results of modern mathematics could help them. I talked a lot with K.O. Friedrichs who was the chairman of the organizing committee of the mathematical seminar. From Princeton I had occasionally visited him at the Courant Institute of the New York University. He represented the absolutely reliable servant of truth, expecting from physicists that they formulate clearly the concepts used and the ensuing problems: after this "*the patient, estimating mathematician can get to work.*" – Quite different was the psychology of Irving Segal who, together with George Mackey lectured on "mathematical problems of relativistic quantum theory". He liked to build fundamental physical theories himself. I had visited him a few times in Chicago and he countered my objections to such attempts with the words: "If you want to build a car you must concentrate on the essentials. The car needs a motor and it needs wheels. The rest is a later worry." – Thinking of different styles of mathematicians I recall that K.O. Friedrichs once took me to the home of Richard Courant to introduce me to his former teacher. I remember vividly one remark by the old master because I was at first thoroughly shocked and thought much about it. So I can still remember it almost verbatim: "There is a fascist group of mathematicians in Paris who have not understood that mathematical reasoning is a natural activity of the human mind." Among my mathematical friends the work by the group which published under the pseudonym Bourbaki was very highly regarded and I was full of admiration for this unselfish collective effort at organizing all mathematical knowledge in logical order and elegant economical formulation. Why on earth did Courant classify this as fascist? Some possible explanation occurred to me gradually. The casting of mathematical insights into a sequence of propositions, theorems and lemmas has some similarity with a code of civil law. If it aims at encompassing all mathematics in a definitive ultimate form it is liable to regulate speech and channel thought. Perhaps Courant had acutely felt the complementarity between systematization and liveliness which in the case of physics had been felt by Arnold Sommerfeld and stressed by Niels Bohr.

The main objective of the research work of my group at Urbana was at this time the formulation of all general principles of quantum field theory (axioms, if you like) in terms of algebras associated to space-time regions. We focused on the von Neumann algebras generated by bounded functions of fields smeared out by test functions with support in the respective space-time region. Our first publication in this area was a paper by Schroer and myself finished in summer 61 at the summer school in Madison, Wisconsin [Haag 1962a]. It formulated well-known principles plus a few further postulates in terms of a net of von Neumann algebras. And it discussed their independence from each other by looking at various models. It was a programmatic paper without deep mathematical results. But it showed that such a net of algebras (in the sequel we shall call them von Neumann rings) allows a very simple, natural expression of causality in relativistic quantum theory. Going over from a von Neumann ring R to its commutant, denoted by R', mirrors the going over from a space-time region O to its causal complement, which we denote by O'. Then, if $R(O)$ denotes the von Neumann ring associated to the region O it is suggestive to demand

$$R(O') = R'(O). \tag{1}$$

This combines the usual causality principle

$$R(O') \subset R'(O) \tag{1a}$$

with some maximality requirement. The commutant of (1) reads $R'(O') = R''(O) = R(O)$ since for any von Neumann ring $R'' = R$.

On the other hand replacing in (1) O by O' yields $R(O'') = R'(O')$. So we get

$$R(O'') = R(O). \tag{2}$$

Now O'' is the causal completion of O. Thus (2) demands roughly that there should be an equation of motion of hyperbolic character. – The "duality relation" (1) played an important role in later work on superselection rules for charge quantum numbers.

Von Neumann had given a coarse classification of factors[6]. He showed that any two projectors in the ring can be compared according to size so that one can assign a relative dimension to each projector, unique up to an arbitrary normalization factor. This led to the distinction of several "types".

Type I: This is the familiar situation in which there are minimal projectors to which we naturally assign the dimension 1. The set of values which the dimension can take are natural numbers including infinity. This situation occurs when the ring is isomorphic to the set of all bounded operators in Hilbert space.

Type II: The dimension function runs through a continuum of values which may be the unit interval (type II_1) or the entire positive real axis (type II_∞).

Type III: All non-zero projectors have infinite dimension.

I believed at that time, for no good reason besides simplicity, that our local factors $R(O)$ were of type I. But we did not know and so we postponed the discussion of the type.

Araki was not among the authors because he left us for a year, following an invitation by Res Jost to the ETH Zurich. There he gave a course of lectures on axiomatic quantum field theory which became a standard reference for many years. Returning to Urbana in late 1962 he produced a series of papers discussing within the regime of rigorous mathematics the net of von Neumann rings in the case of free fields [Araki 1963a; 1964a].

It was probably in summer 1962 that I visited Irving Segal who had just moved from Chicago to the MIT near Boston. He had written a thick manuscript referring to our paper [Araki 1963a; 1964a] in the words: "Physicists have developed some ideas but it is necessary to put them on a rigorous mathematical basis". He was certainly right in that since some of our discussions had been quite heuristic. But he then proceeded to prove that the algebras $R(O)$ were of type I. Now I had just received a letter by Araki from Zurich containing a proof that the algebras of a half space could not be of type I. I told Araki's argument to Segal and he could not find a flaw in it. But his reaction was: "I shall write to you where the mistake of Araki lies."

[6] A von Neumann ring with trivial center is called a "factor". A general von Neumann ring can be decomposed into factors. We assume that all the local von Neumann rings $R(O)$ are factors.

Soon after I got a letter from Segal trying to do just this. Unfortunately his argument was incorrect. I pointed this out to him in my next letter and got the reply that he was busy with lectures but when he found the time he would let us know where our mistake was. Thus the controversy lingered on for several years till at a conference at the Endicot House of MIT where Araki presented his arguments again in a very modest manner, Segal finally conceded "maybe you have a point".

The physics department of the University of Illinois was particularly strong in the areas of many body problems, statistical mechanics, solid state physics. Its most distinguished scientist was John Bardeen. He and his collaborators Bob Schrieffer and Leon Cooper had just developed an atomistic theory of superconductivity for which they were later awarded the Nobel Prize. Incidentally, for John Bardeen it was the second Nobel Prize. The first, a decade earlier had honoured his part in the invention of the transistor. The BCS model of superconductivity was an idealization of very long range interaction between pairs of electrons in the superconductor (Cooper pairs). It had been shown by Bogolubov that in the limit of infinite volume the approximate solution given by BCS became exact. I was infected by the genius loci. Looking at the model I saw that in the infinite volume limit there was a very simple argument for getting the exact solution [Haag 1962b]. Since the argument is so simple and applies to many other cases of spontaneous symmetry breaking ("long range order") let me state it here. The spatial average of any local quantity commutes in the limit of infinite volume with every local quantity. It is an element of the centre of the von Neumann ring generated by all local quantities. Therefore in a primary representation it becomes just a multiple of the identity, an unknown c-number (sometimes called an ordering parameter). In the BCS-model the Hamiltonian density involves such a space average. The replacement of this by a number renders the model trivially solvable.

For the summer holiday 1962, I wanted to invite Hans Jürgen Borchers to Urbana. He had just spent a year at the Institute for Advanced Study in Princeton. Before starting the necessary procedure the department wanted two letters of recommendation. I asked Freeman Dyson whether he could send me such a letter. The reply was curious. He said he hoped that this was purely a bureaucratic routine. Borchers did not need a recommendation and he, Dyson, did not feel qualified writing one. When I asked him with whom Borchers had talked at Princeton he said "He sometimes talked with Wightman. But I guess he mostly talked with God." Anyway Borchers did come for a few weeks to Urbana and, together with Schroer, we addressed a problem which had bothered me for some time: If there are particles of zero mass it is hard to see how one could make a sharp distinction between the exact vacuum state and a state containing a cloud of infrared particles. So it seems doubtful whether the assumption of a sharp, normalizable vacuum is reasonable. In our paper [Borchers 1963] we claimed that this doubt was unfounded. As Borchers put it: "The vacuum has withstood all attempts at dissolving it." In later years my doubts returned and I am no longer sure that our arguments in this paper were relevant.

Two close friends of mine, Daniel Kastler and Theo Maris wanted to come to Urbana for a year and for both the academic year 1962/63 was the only possibility. I had already exhausted my quota of collaborators with Araki and Schroer. So I went to Fred Seitz and asked him what I could do. His unforgettable answer was: "Let me worry about it." He made it possible that both Kastler and Maris could come to Illinois as visiting professors for a year. They could bring their families along and, at least for the Kastler family, this first encounter with America, the overture for many shorter visits in the following decades, was of vital importance. – For one year we were now a group of five getting together almost every day for a couple of hours discussing in front of a blackboard all sorts of problems. But we did not all work on the same question. Theo Maris had ideas about a mechanism of spontaneous symmetry breaking and the possibility of explaining the μ-meson in Q.E.D. Bert Schroer had discovered "infraparticles" i.e. he showed that charged particles like the electron could not have a sharp mass in Q.E.D. due to a not rigidly attached cloud of soft photons. He wrote this up for publication [Schroer 1963] and submitted it as a Ph.D. thesis in Hamburg after he left Illinois. – On the front of local algebras Araki studied the situation in the example of free fields [Araki 1963a; 1964a] and the question about the type [Araki 1964b]. The result that the local von Neumann rings in quantum field theory provide examples of factors which are not of type I attracted the attention of mathematicians, in particular Dick Kadison and his school. – I wondered about an aspect of gauge invariance in Q.E.D. All observables commute with the electric charge. In the terminology of Wigner [Wick 1952] there is a superselection rule between states of different charge. In the usual description the Hilbert space decomposes into a direct sum of subspaces ("coherent sectors") labelled by a charge quantum number. In each sector we have a representation of all observables. This appears somewhat artificial. The natural explanation seemed to be that there is a unique abstract algebra of observables behind this and that the different sectors are just inequivalent representations of this abstract algebra. An abstract algebra, in contrast to an algebra of operators in Hilbert space, means a C*-algebra. So we were thrown back to the proposal by Segal at the Lille conference. But we were now in a better position to meet the objections. First, we were no longer talking about a single algebra but about a net associated to space-time regions to which the physical interpretation is hinged. Secondly, Daniel Kastler had discovered in the mathematical literature a theorem by Fell [Fell 1960] stating that the states occurring in any representation of a simple C*-algebra are weakly dense in the set of all states. Translated into physics this meant that all representations are physically equivalent. A weak neighbourhood is defined by the results of a finite number of measurements with some error bars. So, excluding the possibility of making infinitely many experiments or absolutely precise measurements we can never decide whether a state belongs to one representation or another one. The choice of a representation is a matter of convenience and our customary choice is to take representations which contain an exact vacuum state or which at large space-like distances from us look like the vacuum. – I had been somewhat tardy in writing down these considerations. In the night before the Kastlers went back to Europe we were together at the Argonne lab. Daniel commanded: "Now you sit down and start writing." I obeyed and wrote the introduction. Then we discussed what each of us should do in the next months. This was good because the ensuing paper [Haag 1964] became very influential and, without Daniel's insistence, it might have been finished a year later if at all.

In the mid sixties there appeared a leading article in the New York Times entitled "The Leisure of the Theory Class"[7]. The writer resented that the "Theory class", typically university professors, had an exorbitant amount of freedom. Indeed my contract with the university covered only nine months per year and in the three summer months I could either take a holiday or accept some other employment. In those golden days for physicists one could often combine work with pleasure by lecturing at some summer school, usually located at an attractive place like Madison/Wisconsin, Boulder/Colorado or Honolulu in Hawaii. Particularly fond memories are connected with the summer research institute at Aspen/Colorado. There in the middle of a fascinating mountain landscape some philanthropic businessman had created an "Institute for Humanistic Studies" where young executives could get an exposure to philosophy and general culture. It was accompanied by a music festival in a large tent and evening lectures on many topics by distinguished speakers. An astute colleague of mine had argued that theoretical physics constituted an important part of culture. Thus there

[7] A take-off from a well-known sociological treatise "Theory of the Leisure Class".

was a summer institute for theoretical physics added to the humanistic studies. We went there in three summers for a month with the whole family. – I acknowledge gratefully that we have been highly privileged. In recent times this has changed, partly because the number of professors and students has increased enormously but also because administrators believe in the wisdom of the slogan attributed to Lenin: "Trust is good but control is better". So there have sprung up many unproductive agencies controlling and evaluating, compiling statistics with often meaningless if not misleading numbers, forcing scientists to fill out questionnaires lest they have too much time to think or to play tennis.

Res Jost was a friend of Dr. Konrad Springer, one of the proprietors of the Julius Springer Verlag, the German science publishing house in Heidelberg. Together they discussed the project of creating an international journal devoted to mathematical physics. Res Jost told me that Laurent Schwartz was very interested in this and he asked me whether I was willing to take some responsibility in this project. I said yes, probably because I felt flattered. I did not anticipate that later I would be fully in charge as chief editor. Luckily I found among my friends a board of associate editors who were competent in the main areas to be covered: Nico Hugenholtz for many body problems and statistical mechanics, David Ruelle for quantum field theory, Laurent Schwartz for pure mathematics with relevance to physics and Abe Taub for general relativity. Then there was the problem of a name for the journal since there existed already the "Journal of Mathematical Physics" in America. I disliked intensely the proposal by the publishers to use a Latin name like Acta Matematica... Ultimately Abe Taub suggested the title "Communications in Mathematical Physics" which I considered excellent. For the first issues it was important to solicit good contributions. Later the problem turned around. We had to reject more than 50% of submitted papers even if they were correct and mildly interesting. This was a very painful task and involved of course subjective judgement. Anyway the "Communications" made their way. After 8 years I felt that I needed to be relieved from the job of chief editor and I am very grateful to Klaus Hepp who offered to step in for a few years. An act of true friendship. Then the job was taken over by Arthur Jaffe who served for many years. The CMP is still going strong now under the direction of Michael Aizenman.

In those years my market value had reached its maximum. I got numerous offers from institutions in America and Europe. Some quite tempting as far as prestige or salary were concerned. But I felt quite happy in Urbana and did not want to change. A different matter was offers from Germany. There were many strings pulling us back. Two places held a special attraction: The Technical University of Munich where Maier-Leibniz, who was a first rate physicist as well as a political wizard, had succeeded in creating a modern physics department in which I found my old friends Wilhem Brenig and Wolfgang Wild. Rudolf Mössbauer added to the aura. He had been a student of Maier-Leibniz and was just awarded the Noble Prize. The other place was Hamburg University where under the directorship of Willy Jentschke the electron synchrotron DESY had been built and a new professorship for theoretical physics had been established for which Harry Lehman was absolutely determined to get me. For Jentschke I could say similar words of praise as for Maier-Leibniz or for Fred Seitz as great science administrators.

There was also an offer from Göttingen where Friedrich Hund and Gerhard Lueders taught theoretical physics. I told them that it was very unlikely that I could accept the offer. Professor Hund, who was about to retire, asked me to come anyway for a few weeks to visit. Friedrich Hund was a truly admirable person; a scientist of great achievements without the slightest trace of vanity. With regard to his famous work on atomic spectra, where he was the first to classify spin- and orbital wave functions according to their behaviour under the permutation group, his comment was: "In those years a theorist of modest talent could harvest results of great interest.

Today persons of great talent produce results of modest interest." – After the war Hund found himself in the Russian occupation zone of Germany which later became the GDR. As one of the few famous scientists in the DDR he became rector of the University of Jena and was decorated with the National Prize of the DDR. He got soon into controversies with the political powers. At the last joint meeting of the German Physical Societies of East and West in 1952 at Bad Nauheim Hund came with his assistant Harry Lehmann. He organized a session in which Lehmann talked about his Ph.D. thesis. I remember that Erwin Fues was very impressed and commented: "He will make his way and achieve something." Hund procured a fellowship for Lehmann at Heisenberg's institute in Göttingen. Two years later both Hund and Lehmann had managed to move permanently to West Germany but in quite different manner. Lehmann who had refused to return to Jena was henceforth considered as "fugitive from the Republic". He could not visit his parents in the DDR for many years without the risk of long term imprisonment. When I asked Hund how he had managed to come across the iron curtain he said: "I told my government that I received an offer from Frankfurt University and that I decided to accept it". Maybe the powers in the DDR were glad to get rid of this troublesome person. But maybe it was just the disarming sincerity and simplicity against which scheming politicians had no defence. – Sincerity and simplicity indeed were salient traits of Friedrich Hund and they were paired with a widely ranging sweep of the mind and a love of nature. He was a great hiker. In summer 1954 in Copenhagen I made a Sunday excursion with him. We walked for many hours mostly without saying a word. In the end I was dead tired whilst Hund, though much older, did not show any sign of fatigue. At the age of 95 he still participated with lively interest in seminars and lectures, using public transportation to get there. When I visited him for the last time in a nursing home he was 101 years old, quite lucid and with remarkable self control. He asked me whether I still publish papers and urged me to keep on working. "That is very important. Keep it up."

In Winter 1964, I took leave for a few months from Urbana and went with the whole family to Munich.

From these months I remember my first meeting with Sergio Doplicher. He had come from the IHES in Bures near Paris and wanted to tell me about his recent work. When he started by referring to some fixed point theorem to prove the existence of the vacuum I waved him off: "No, no. We tried this and it does not work." He was not offended and asked politely: "Could I proceed anyway?" Then he gave a description of the energy-momentum-spectrum by a left ideal in the algebra of quasi local observables. I was startled and accepted his paper on the spot as the first contribution to the newly founded journal "Communications in Mathematical Physics".

The weeks in Göttingen were pleasant and harmonious. It was the only place where seminars could extend over more than two hours without a break and students did not show any signs of restlessness. This was due to the example of Friedrich Hund with his intense interest. Harry Lehmann told me later about some humorous criticism of my ways by Gerhard Lueders: "Of course it was nice to have Haag here. But he drank so much tea with the students and kept them from work."

In Hamburg I signed a document promising to come in two years to occupy the vacant professorship.

In Urbana my research associates were now André Swieca from Brazil and Derek Robinson from England. Actually André Swieca was born in Poland. His family succeeded in fleeing before the German troops which brought the rule of the SS. They reached Brazil in an adventurous journey and could settle there. André brought many important qualities; wide knowledge, intuition and motivation. He was an intensive worker, unfortunately an equally intensive smoker. I was a heavy smoker myself but André consumed twice as much. Maybe this was responsible for his serious health problems a decade later and indirectly for his much deplored early death. – In Urbana we wrote one paper together [Haag 1965]. I shall postpone the discussion since the most influential work of André originated later in Brazil. Another aspect I consider as very important is the formation of scientific families. André established for us the "Brazil connection". Bert Schroer and John Lowenstein (my last Ph.D. student in Urbana, now professor at NYU) went to Rio to work with André. For both this turned out to be a decisive period.

Derek Robinson had spent some time at the ETH Zurich where among other things he had discussed generalized free fields and exhibited simple examples of quantum field theories in which no particles occurred. In Urbana he continued to work with us on field theoretical questions but gradually his main interest shifted to many body problems. Daniel Kastler, always on the look out for talent, offered him a professorship at the University of Marseilles which he held for quite a number of years before moving to Australia. It was in France that he wrote together with Ola Bratteli his monumental two volumes on the application of C*- and W*-algebras in quantum statistical mechanics [Bratteli 1979].

Three of our publications originating in this period are worth mentioning. First the one with André Swieca quoted in reference [Haag 1965]. It did not answer the question posed in the title but discussed localization properties of states in terms of the response of detectors and coincidence arrangements of detectors. Most important, however, it pointed out one restrictive requirement on the theory beyond those previously considered in the listing of postulates. We called it the "compactness criterion". Roughly speaking it demanded that the set of states localized in some finite region and bounded in energy below some finite value E should be finite dimensional. The precise mathematical form used by us was: the set $P_E R_1(O)|0\rangle$ should be compact in the norm topology. Here R_1 is the unit ball of R (i.e. the set of operators with norm ≤ 1) and P_E denotes the projector to energies below E. This criterion was substantially sharpened and strengthened later by Buchholz and Wichmann [Buchholz 1986] as postulates of nuclearity and used by Buchholz to derive various structural properties of the theory. I believe that these restrictions could open the way to characterize a specific theory within the general frame. This is a task long overdue in the pursuit of the general theory of quantum fields. The specific information needed must be contained in the immediate neighbourhood of points since it is given by the Lagrangean density in the conventional approach. A beginning has been made by attempting to define the "germ" of a theory [Haag 1996]. If I was twenty years younger I would continue to work on it.

The second problem started with the question of how to treat collision processes in gauge theories without the use of gauge dependent operators. Since the algebra of observables and physical operations does not connect the vacuum with states of a charged particle one cannot use the LSZ-construction. But we can simulate detectors within the C*-algebra of observables as positive, almost localized elements which annihilate the vacuum. We do not know a priori what such an operator detects. It just responds to some local excitation. But the experimentalist building a detector without using the experience collected by generations of others is in the same situation. It is by many trials that he increases the sensitivity and selectivity of the instrument and one learns about various types of particles, their masses and collision cross sections by means of coincidence arrangements in many different geometric patterns. When Araki was visiting Urbana again in 1965 I told him about this project. His immediate answer was "I know precisely how to do it". So we slaughtered the problem in a short time [Araki 1967]. – The matrix elements of the operators used in the LSZ-formalism, connecting the vacuum with single particle states, decrease as $t^{-3/2}$ when the localization region is shifted to time-like infinity. The matrix elements of detectors decrease like t^{-3}. By focusing on them one obtains cross sections, though not S-matrix elements. This is helpful not only in gauge theories but also for instance in the case of zero mass particles. The essential message is however, that all physical consequences of the theory are implied by the assignment of sub-algebras to regions in space-time. No further labelling of individual elements (such as a relation to specific species of particles) is needed.

The third project concerned the treatment of statistical mechanics close to equilibrium in the algebraic setting where we consider from the beginning the thermodynamic limit i.e. an infinitely extended medium. A first step in this direction had been taken by Araki and Woods [Araki 1963a] who discussed the representations of the canonical commutation relations arising in the thermodynamic limit from grand canonical ensembles of a free Bose gas. With the algebraic approach the general situation came into sharp focus. Irrespective of whether we consider quantum field theory or statistical mechanics the theory is defined by the same net of abstract C*-algebras and its symmetries acting as automorphisms on the net. The representation of the abstract algebras and symmetries by operators in a Hilbert space depends, however, on the family of states one is interested in. In quantum field theory these are the states which result from the vacuum by local excitations. In statistical mechanics we replace the vacuum by a thermodynamic equilibrium state at given values of the inverse temperature β and of the chemical potentials μ_i.

In the standard approach one starts from a system enclosed in a container of finite volume V. Then an equilibrium state can be specified by a density matrix in the vacuum representation corresponding to a Gibbs ensemble, most conveniently a grand canonical ensemble characterized by the values of β and μ_i. In the thermodynamic limit as $V \to \infty$ keeping β, μ_i fixed the density matrix and the Hamiltonian get lost (the latter because in the infinite system not only the total energy but also its fluctuations become infinite). But the expectation values of local observables converge and define a state (expectation functional) for the net of algebras. It can be used to generate a representation of the algebras by operators in Hilbert space in which the equilibrium state is represented by a vector. This representation is, of course, inequivelant to the vacuum representation and to that for any other set of values β, μ_i.

The project received an essential push by the visit of Nico Hugenholtz in Urbana in Winter 1965/66. Together we first formulated the description of equilibrium for a finite system in a way such that most of the features remained valid in the limit $V \to \infty$. Though the equilibrium state is impure it can be described by a state vector corresponding to the square root of the density matrix. This $\kappa_0 = \rho^{1/2}$ is a Hilbert-Schmidt operator. The set of all Hilbert-Schmidt operators forms a Hilbert space K with the scalar product $\langle \kappa_1, \kappa_2 \rangle = \mathrm{Tr}\ \kappa_1^* \kappa_2$, and it is also a 2-sided ideal in the algebra of all bounded operators. So K can be used as a module on which the elements of the algebras act by multiplication from the left. This representation has a large commutant, namely the multiplication from the right which gives a conjugate representation related to the left multiplication by an antiunitary conjugation operator. This structure remains unchanged in the thermodynamic limit. – For the direct characterization of the equilibrium state in the thermodynamic limit we could resort to previous work by others. The fact that for the finite system the density matrix in a Gibbs ensemble corresponds to a time translation by the imaginary time $i\beta$ leads to relations for the temporal correlation functions. These relations persist in the thermodynamic limit. They were first given by Kubo [Kubo 1957] and used by Martin and Schwinger in their work on thermodynamic Green's functions [Martin 1959]. We called this the KMS-condition (for Kubo, Martin and Schwinger) and showed that it suffices to determine completely the equilibrium state of the infinitely extended medium.

When Nico left Urbana there were still some gaps in our arguments. He wanted to close them in Groningen and make everything watertight in collaboration with his assistant Marinus Winnink. The final result was the paper [Haag 1967].

It is amusing to note that many years later there arose a lengthy controversy between t' Hooft and Hawking about a factor 2 in the value of the Hawking temperature of a black hole. It resulted from the fact that t' Hooft had used the density matrix itself instead of its square root in the description of the state by a vector.

In early 1966 Nishijima received a very high prize in Japan honouring his work of 1952 when, parallel to and independent of Gell-Mann, he introduced the quantum number known as "strangeness" which was an essential step in the classification of "elementary particles". He had to travel to Japan to receive it from the Emperor's hands. But before he went we celebrated him appropriately in Urbana by a great party. The expenses were shared by several members of the department and the location was in our house because we had much space due to Kaethe's architectural efforts. It was a wonderful festival described as "a continuous glass of Champaign" and unintentionally it was a farewell party for both Nishijima and myself because both of us left Urbana for good in the following year.

Hamburg, Bandol and the Philadelphia quartet

For some years theoretical high energy physics had been dominated by ideas emanating from Berkeley. Ever since Geoffrey Chew's famous walk in the forest where he got the inspiration that quantum field theory was a dead end road and the future belonged to a maximally analytic S-matrix, a school of thought developed which attracted a rapidly growing number of followers. In analysing experimental data as well as in ambitious fundamental projects with slogans like "nuclear democracy", "bootstrap mechanism" the analytic S-matrix approach had become the dominant fashion till toward the end of the sixties other ideas captured the centre of interest: the quark model, the unification of weak and electromagnetic interactions, non Abelian local gauge theories. Fashions come and fashions go even in science, stimulated by the enormous increase of the number of scientists who all need jobs. But analytic S-matrix theory left us some harvests important in the phenomenology of elementary particles, among them the idea of Regge trajectories.

In Hamburg my closest friends and colleagues were Harry Lehmann and Hans Joos. With Lehmann I shared now the directorship of the so-called "Second Institute of Theoretical Physics" located in the premises of the DESY accelerator lab. Lehmann was gifted with a very sharp and quick grasp of the essentials in any dispute, whether in faculty meetings or in the scientific council of DESY or in our weekly seminars. He not only spotted immediately a fishy argument but could also formulate the issue precisely, often making the previous speaker look rather ridiculous. Here some typical examples of Lehmann sarcasms. In discussions about university reforms at the time of the "student revolution": "I guess the time has come to transform the university into a wildlife preserve for students". When a seminar speaker quoted the authority of an international collaboration of authors: "Fools of all nations unite!" He rarely missed an opportunity for a joke. His comment about my early work on collision theory: "Your almost local operators suggest that what you say may be almost true". Some quite senior colleagues were afraid of his sarcasm, some were hurt. But his charm and sincerity and the complete absence of vanity or presumptions earned him high esteem even admiration.

Hans Joos was at that time the chief theoretical advisor of DESY. He had spent some years in Brazil and, like Lehmann, he had disdained the standard requirements for an academic career. So he had agreed to acquire a Ph.D. only a few years ago under

Lehmann's patronage, after Willy Jentschke, the director of DESY, had insisted that it was absolutely necessary for the position offered to him. He was open, friendly, helpful. Only sometimes an undercurrent of choleric temperament erupted, e.g. when a seminar speaker refused to answer relevant questions.

In my decision for Hamburg rather than Munich the hope to engage actively in elementary particle physics rather than many body problems had played a role. Since the best way to learn about an area is to lecture about it, especially if one has a competent partner, I planned to give a series of lectures on elementary particle physics extending over several semesters together with my friend Hans Joos. After two semesters I realized that my part of the lectures had not been really helpful to the students and that I remained an alien in the world of high energy physics – at least as far as comparing experimental data with predictions of various theoretical models was concerned. Apart from my preoccupation with vested interests in other questions it was a laziness in starting to calculate from tentative assumptions. I remembered a remark of Wigner about a young instructor in Princeton: "He went for a year to Germany and when he came back he was transformed into a German physicist". I wanted to know "what is a German physicist?". His answer: "Well, you know, an American physicist if he has no ideas he calculates something and makes himself useful in some way. A German physicist if he has no ideas he just does nothing". With regard to my desire to get into high energy physics I realized that, after all, I was a German physicist as defined by Wigner and resigned to remain a spectator in this area.

In Spring 1967, there was a conference in Baton Rouge, Louisiana, important for a variety of reasons. It was a meeting between mathematicians who represented the heritage of von Neumann, with physicists who used C*-algebras and von Neumann-algebras in quantum field theory and quantum statistical mechanics. In Baton Rouge the first surprise was a thick manuscript by the Japanese mathematician Tomita. When Nico had looked over it he said: "If this is true then our paper [Haag 1967] is just a special case of a much more general situation". I asked Dick Kadison what he thought about it. He was not impressed: "I have seen such papers before. You start reading them and find the first mistake on page 15. It can be easily mended but then the next one comes on page 35; it also can be mended with some effort. Finally on page 57 you come across one which cannot be mended. No, I would not advise you to get into that". Luckily Masamichi Takesaki was not dissuaded but delved into Tomita's paper, simplifying the arguments, closing gaps until finally there resulted the Tomita-Takesaki theory [Takesaki 1970], one of the major advances in operator algebras, opening the door for many subsequent developments. For me this was a manifestation of the miracle called prestabilized harmony between mathematical structures which are pure products of the mind, with important areas of physics. Certainly neither Tomita nor Takesaki were thinking of quantum statistical mechanics. But their first central theorem stating that any faithful state of a von Neumann algebra defines an automorphism group called "modular automorphisms" and an anti-unitary conjugation, mapping the algebra onto its commutant, was a generalization of our discussion of thermodynamic equilibrium states in [Haag 1967]. In fact our paper could now be summarized by saying that the equilibrium state is a faithful state whose modular automorphisms are the time translations. The modular conjugation is simply related to time reversal.

At this conference the scientific exchange between us physicists and Dick Kadison intensified and led to the birth of what I like to call the Philadelphia Quartet. In the subsequent years the four of us, Kadison, Hugenholtz, Kastler and myself spent much of our free time together and, for some years all the four of us including our families used to turn up in the same places; in Florida, in Philadelphia, the home base of Dick Kadison, in Los Angeles, in Seattle, in Vancouver. Kadison liked to organize these meetings and he was able to do that due to the high prestige he enjoyed in the mathematical community. The scientific harvest of these meetings, though not negligible, was not as great as one might have hoped for. It was not always possible to focus on a common project of mutual interest and there were many other tasks and distractions limiting the time for systematic and concentrated discussions. But I keep precious memories of all the places visited and learned a lot which only gradually bore fruit. Dick Kadison liked to explain technical terms in connection with experienced situations in which he first had come across them. For instance, when I wanted to know what "complete positivity" meant Dick started: "About ten years ago I was sitting with my good friend Peter H. in a coffee shop in Chicago...". Meanwhile I forgot the rest of the explanation. One nice anecdote from our stay in Seattle comes to mind. We were in the Battelle Institute whereas Prof. Peierls, famous theoretical physicist from Birningham, was guest of the university. Of course we invited him to give a talk at the Battelle Institute. Reluctantly he came and started his talk with the remark that at first he had not wanted to come because he considered the spirit of our gathering as quite orthogonal to his style and illustrated this by saying that there are two kinds of owners of bicycles. Those who keep polishing and polishing their bike and those who ride on it. – A few years later I found myself giving a lecture at the summer school in Sitges near Barcelona, Spain and Peierls was the chairman. I started by recounting this story from Seattle and Peierls took up the ball elegantly by closing the session with the words: "We thank Prof. Haag for taking us on a ride through many lands in his beautifully polished car".

In France, Daniel Kastler was devoting much thought and effort to enrich the faculty of the University of Marseille and to establish in Bandol a tradition for meetings of mathematicians and physicists from many countries. He was the driving force in creating the new faculty in Luminy, a beautiful site on the eastern outskirts of Marseille. Together with his young friend the competent and enthusiastic dean Mohamed Mebkhout (in short "Momo" for his friends) he shaped its course in the first years. He succeeded in installing Sergio Doplicher from Italy and Derek Robinson from England, subsequently Joachim Cuntz from Germany and John Roberts from England as professors of physics or mathematics in the new faculty. Furthermore Daniel procured the means for inviting scientists from many countries for a few weeks each year to Bandol. There his wife Lisl took care of organizing accommodations for the visitors in houses owned by her friends and acquaintances. This was not always without problems. I remember that once, sitting with Daniel in his living-room working on some problem when Nico Hugenholtz marched in with the unforgettable words: "This is not an emergency but it may develop into one". Apparently the sewage in the residence was congested and started to overflow. So Daniel put on his rubber boots, picked up some tools and went over. Altogether Bandol became a great attractor for mathematically minded scientists.

The deepest question bothering me in these years concerned the wish for a natural understanding of the role of gauge invariance and charge quantum numbers. In the customary treatment in quantum field theory one dealt with two kinds of fields; "Bose fields" which commute at space-like distances and "Fermi fields" which anti-commute there. They generate different types of particles. "Bosons" for which the multiparticle wave functions have to be totally symmetric and "Fermions" obeying the Pauli principle, demanding total anti-symmetry. Furthermore, each field carries a charge quantum number characterizing the type of interaction in which it participates. This in turn defines a transformation group for the respective field, the "gauge group". The invariance of the theory under these gauge transformations implies "superselection rules" between states with different charge quantum numbers. This is a strong form of charge conservation implying that the charged fields cannot be observable. – In our paper on the algebraic approach we had argued that the theory was fully characterized by the net of algebras generated by the local observables i.e. that all physical information was contained in this net. The observables must commute at space-like distances; this is a consequence of relativistic causality and not introduced for the sake of Bose statistics. The question was then, how do we get from the abstract algebras of observables to the structure of charge quantum numbers, gauge transformations, Bose and Fermi type particles. This question was pressing immediately after the completion of our paper but I had no idea how to attack it until many years later. Meanwhile my attention focused on a small part of the problem. For a long time it had been asked again and again whether no other permutation symmetries besides totally symmetric or totally anti-symmetric wave functions could occur. There existed some pseudo proofs for this Bose-Fermi-alternative. But they were unsatisfactory because they relied on too narrow a view of the role of wave functions. For quantum field theory H.S. Green had proposed generalized commutation relations allowing other possibilities [Green 1953]. In the sixties there appeared quite a number of papers elaborating on this theme. I realized that these other possibilities were not just esoteric constructs. We would have met them in the early years of quantum mechanics if the spin was not accompanied by a magnetic moment so that no hyperfine structure in the spectra showing a direct manifestation of spin could be observed. Then the discussion of fine structure would have led to strange limitations of permutation symmetry, forbidding all orbital wave functions which could be symmetrized with respect to more than two electrons (compare the discussion of fine structure by F. Hund [Hund 1927; 1933]). In modern language this would have meant that the electron is a parafermion of order 2.

In Summer 1965, I met Hans Borchers in the Argonne lab. He told me that he had proved the Bose-Fermi-alternative as the only possibility allowed by the general principles. I objected that this could not be true and tried to tell him my simple counter example. But Borchers did not want to hear it and kept insisting: "No, the real problem is quite different…". We could not reach any agreement and therefore I resolved that I should attack these questions without Borchers. One embarrassing aspect was that I had solicited Borchers' paper and accepted it for publication in the *Commun. Math. Phys.* without having read it, on the strength of my high regard for Borchers. It appeared without corrections [Borchers 1965]. Actually I think that this was not bad because it stimulated other work. The paper contained a seminal idea whose relevance was appreciated 15 years later by Buchholz and Fredenhagen and served as the starting point of their analysis of the superselection structure [Buchholz 1982]. Indeed the whole episode is instructive in several respects. Trying to read Borchers' paper I was unable to find where a mistake had crept in. I could not see through the very sophisticated mathematical arguments and just saw that several conclusions were wrong. It took the mathematical prowess of John Roberts and Sergio Doplicher to clarify this [Doplicher 1969a]. This marked the beginning of a very fruitful collaboration.

The fertile seventies

We had finally found a key for analysing the possible superselection structures: a natural selection criterion for the states of interest and the duality relation $R(O') = R'(O)$. This led us to a complete classification of all possible charge structures and permutation symmetries [Doplicher 1969b; 1971; 1974]. This work took us into lofty areas of abstract mathematics where the air began to become too thin for me to breathe. So I quit this wonderful cooperation in 1974. Wightman referred to this series of papers as "the elephants' playground". After some interlude Doplicher and Roberts continued the work and exhibited structures which were of interest in pure mathematics as well as in basic physics. I quote only two examples [Doplicher 1989; 1990].

Another development in which pure mathematics owed much to the work of theoretical physicists like Araki was the full classification of the types of factors. It was essentially reached in the celebrated work by Alain Connes [Connes 1973].

For quantum field theory it ultimately led to the recognition that all algebras associated to local regions are isomorphic to the unique hyperfinite factor of type III_1 [Buchholz 1987].

The 70ies were years of great activity and achievements in theoretical physics. Most important for quantum field theory and elementary particle physics was the formulation of the standard model involving three generations of lepton fields unifying weak and electromagnetic interactions and three generations of quark fields describing strong interactions in a local gauge theory called quantum chromodynamics. The quark model and the discovery of various generations evolved from the empirical evidence. Gauge theory with non Abelian structure groups needed mathematical guidance going back to the work by Hermann Weyl. The classical picture involved the generalization of the concept of field to that of a fiber bundle in which no natural coordinatization of the fibre above a point exists and the values related by some "structure group" are equivalent. This leads to the distinction between sections and connections where the former relate to the quark field, the latter to "gluon fields", the generalization of the electromagnetic vector potential. In one of his self critical moods Harry Lehmann contended: "We have been fools believing for such a long time that meson theory is the key to the physics of strong interactions instead of looking for a theory which follows as closely as possible the example of electrodynamics." – Parallel to this success which specifies the degrees of freedom and the form of the Lagrangean there was the progress in the task of getting to numbers which could be compared with experiment. Ideas about the scaling behaviour, relation to critical points in thermodynamics, a "running coupling constant" with "asymptotic freedom" of strong interactions opened a regime of perturbation expansions for very high energy reactions.

The long search for the proper treatment of the local gauge principle in the renormalization of perturbation theory had ultimately led to the BRS-formalism [Becchi 1975]. This elegant scheme is generally accepted today as the adequate formulation of the local gauge principle in perturbation theory. But it bears no resemblance to the conceptually simple picture in classical theory with its continuous group acting on the fibers of a bundle. Instead the fact that the field operators are not by themselves physically meaningful objects finds its expression by letting them act in a Hilbert space of indefinite metric from which the subspace of "physical states" is selected by means of the BRS-charge Q formed by operators corresponding to infinitesimal generators of the gauge group and by Faddeev-Popov ghost operators. Q is nilpotent and thus may be interpreted as the coboundary operator of a cohomology.

Although QCD with asymptotic freedom and the BRS scheme is an enormous progress we cannot ignore that the total resulting scheme for high energy physics, the standard model, retains many unsatisfactory features. Its predictions become unreliable at very high energies since the self-interaction of the Higgs particle does not give rise to an ultraviolet fixed point under the action of the renormalization group. Problems involving moderate energies in QCD can only be attacked by the brute force method: to cut down the number of degrees of freedom to finitely many by introducing a cut-off in momentum-space as well as in position-space, replacing the infinitely extended continuum by a finite number of discrete cells. In this approximation problems in chromodynamics could be attacked numerically with the help of giant computers. This grand project called "Lattice gauge theory" absorbed the efforts of many highly gifted theoreticians for decades. It brought some insights but with the presently existing computer generations it remained still far from providing a reliable test of the predictions of the theory.

The strategy of starting with a finite number of degrees of freedom by means of cut-offs in position- and momentum-space had been employed in another project which developed into a major industry called "constructive field theory". The hope was here to construct simple models of quantum field theories satisfying all standard axioms by studying with rigorous mathematical methods the limit when the cut-offs are shifted to infinity. This could be achieved in some models living on two- or three-dimensional space-times. But in the realistic situation of four dimensions the difficulties exploded so that the project came to a halt. The program had been suggested by Wightman in the mid 60ies. It was pushed most strongly by Arthur Jaffe, one of Wightman's most gifted students together with Jim Glimm, a distinguished mathematician who had worked with Kadison. Ultimately Glimm resolved that sometime in his life he should do something else than showing that some quantity x is smaller than some quantity y. He quit quantum field theory and started to work on mathematical problems of fluid dynamics arising in the prospecting for oil.

In Summer 1970, I lectured at the Physics Summer School of Brandeis University at Boston. There I made the acquaintance of a young lady from Poland, Ewa Tulcijew. She had emigrated to Canada and was now a graduate student in physics at the University of Calgary. She liked my approach to the problems and asked whether I could supply a thesis problem for her. Two years later she came to Hamburg to work for a Ph.D. thesis under my guidance. The problem I suggested concerned the description of equilibrium by Gibbs ensembles, respectively KMS-condition. It should be possible to show that this is a consequence of simple natural requirements: the insensitivity against the presence of "grains of dust", more precisely some degree of stability against slight changes of the dynamical law. In her thesis Ewa showed this for the special case of a free Bose gas. In the discussion of the general case Daniel Kastler joined our efforts and together we succeeded in deriving the KMS-condition as a general consequence of the demand for stability of a stationary state of an infinitely extended medium against all small local modifications of the dynamics. Meanwhile Ewa had married Klaus Pohlmeyer and she signed her publications now as E.B. Trych-Pohlmeyer [Haag 1974]. – We continued to work along these lines for a few years studying different degrees of stability, allowing more extended perturbations [Haag 1977]. But to my knowledge nobody followed up this line in later years. – Another interesting property of KMS-states called "passivity" was pointed out by Pusz and Woronowicz [Pusz 1978]. It exhibited the relation to irreversibility as expressed in the second law of thermo dynamics.

Sometimes it happens that investigations within seemingly unrelated areas, using different concepts, different language produce results which later on are recognized to be closely related. Such was the case with two important papers appearing in 1975. One of them came from general relativity which had predicted long ago that a collapsing star, if its radius shrinks below the "Schwartzschild radius" $r_0 = 2GM/c^2$, a black hole of radius r_0 is formed from whose interior no message can reach the outside (G denotes Newton's gravitational constant, M the mass of the star). For an outside observer the black hole manifests its existence only by a few global quantities, like total mass, total angular momentum, electric charge. No details about the situation inside are visible. John Archibald Wheeler who liked picturesque language, used to paraphrase this by: "a black hole has no hair". J.D. Bekenstein [Bekenstein 1973] assigned an entropy to the black hole to describe the loss of information accompanying its formation. Steven Hawking finally showed that quantum theory implies that the black hole radiates like a black body of temperature (in natural units) $kT = (4\pi r_0)^{-1}$ [Hawking 1975]. Although this temperature and the ensuing radiation are so tiny that the effect is unobservable under realistic conditions (T being of the order of 10^{-8} K) the result captured enormous interest. For the first time one saw an interplay between gravity and quantum physics. Indeed it stimulated the formulation of quantum field theory in curved space-time i.e. quantum physics in a given classical gravitational background field. Still, quantum gravity, the synthesis of quantum physics and general relativity remained an enigma.

The other line started from an application of the Tomita-Takesaki theory to ordinary quantum field theory in Minkowski space. For any space-time region which has a non void causal complement the vacuum is a faithful state for the algebra associated to the region. Hence the vacuum defines a modular automorphism group for the algebra of each such region. For most regions the determination of this automorphism group is difficult and the result does not appear very useful. But there is one case in which the automorphism group has a direct geometric significance: the wedges, typically the set of points with coordinates

$$x \geq |t|; \;\; y \text{ and } z \text{ unrestricted.}$$

(In natural units where the velocity of light is put equal to 1 and distances are measured in units of time). The hyperbolic, accelerated orbits

$$x = \left(\rho^2 + t^2\right)^{1/2}$$

or in parametric form

$$x = \rho \cosh \tau$$
$$t = \rho \sinh \tau$$

(ρ fixed, τ varying) are generated by a 1-parametric subgroup of the homogeneous Lorentz transformations, "the boosts". One finds [Bisognano 1976] that these boosts are the modular automorphisms induced by the vacuum and that for an observer travelling along one of these orbits, who will use the proper time $\rho\tau$ as his time coordinate, the vacuum appears like a thermal state with temperature $T = (2\pi\rho)^{-1}$. In 1966, W. Rindler had pointed out that for observers moving on these accelerated orbits the boundary of the wedge is a horizon. A signal sent by one of these observers can never produce an echo from the other side of the horizon which can be received by him or his fellow travellers [Rindler 1966]. Thus the temperature appearing in the KMS-condition for the wedge algebra is the analogue of the Hawking temperature. The accompanying radiation can be observed in principle by a detector travelling on one of the mentioned accelerated orbits through the vacuum [Unruh 1976].

In 1975, I was invited by Yurko Glaser to spend a year at CERN in Geneva as a senior research scientist. This was a wonderful year for me. Apart from the beautiful surroundings of Geneva and the stimulating atmosphere at CERN I was free from all duties of lecturing and advising students and of most family duties. I planned to do something really great in fundamental physics and studied differential geometry and topology which I believed to be important for the unification of general relativity with quantum physics. Yurko Glaser shared his office with me and when we both were there we talked about many things. Our overlap in time was, however, not so great since Yurko's schedule was to work through most of the night and to come to the office in late hours of the afternoon. My ambition to find a synthesis between general relativity and quantum physics was not crowned by success. In special relativistic quantum physics there remained space-time as the one real, classical notion which could serve as the Archimedean point on which the theory can be hinged. In general relativity the space-time structure is dynamical and hence subject to quantum fluctuations. None of our conventional concepts remains as an ordering scheme. Moreover, since the interplay between general relativity and quantum physics becomes relevant only in situations of extreme density such as they might have existed in the early universe the basic notions of the Copenhagen interpretation do not fit. There can be no observable, no measuring results because there is no living being around who could plan an experiment. Perhaps the notion of "measuring result" can be replaced by that of an "event" to which one may attribute the status of representing an individual fact, roughly localized in space and time. According to the standard scheme of quantum theoretical predictions the facts of the past determine a quantum state which in turn defines a probability for the occurrence of the subsequent event but it leaves it entirely open which of the various possibilities will be realized.

The idea of basing the interpretation of quantum theory on the concept of "events" which may be considered as facts independent of the consciousness of an observer and not hinged to the performance of experiments has dominated my thoughts in subsequent decades. Since quantum theory is essentially indeterministic the transition from a possibility to a fact i.e. the occurrence of an event happening fortuitously introduces an element of irreversibility, distinct from the continuous, deterministic development of the quantum state described by a Schrödinger equation with time reversal (PCT) – invariance. This equation determines only the probability for the occurrence of the event. But there is no law of nature determining which of the possible alternatives is selected. I shall return to this circle of questions later.

An important development for mathematical physics taking shape in 1975 was the foundation of the International Association of Mathematical Physics. The creation of such an organization had been proposed for several years by Moshe Flato and pushed very rigorously by him against some opposing faction of scientists which included me.

The controversy was in part due to lack of clarity about the objectives of the proposed organization but in part also due to personal animosities. Some of us had begun our scientific life before the great inflation in numbers at a time when the theoretical physics community was a rather tightly knit group, inspired by great masters like Lorentz, Planck, Einstein, Bohr, Sommerfeld... We did not see any need for a new organization outside the existing mathematical and physical societies and feared the spectre of a public relations oriented lobby engaged in fund raising for some pet projects. In 1974, the attempt to create the organization by an overwhelming vote of the participants at an international congress on mathematical physics in Warsaw failed, mainly because the Russian delegation was uncertain whether this was politically correct. So in fall 1975, it was decided that a few representatives of the opposing groups should get together and settle the issue. We met in Geneva. On one side there was Flato and Piron, on the other side Hepp and myself and, if I remember correctly, Borchers as a neutral witness. In the course of the discussion Flato succeeded in convincing me that he was not a bad guy and we ultimately agreed that the organization should be created, that the first president should be Walter Thirring and that in the executive board there should be no person who had played any role in the previous controversy. Thirring accepted the task and appointed a committee of four persons, consisting of Araki, Piron, Ruelle and Streater, to work out the statutes of the organization. Araki in his usual careful, conscientious way wrote the final version of the statutes, which were approved by the vote of the inscribed members in July 1977. Thus the organization could start its life.

One year before I came to CERN an idea had emanated from there which caught the fancy of many theoreticians and kept its virulence to this day: "supersymmetry". It introduced a symmetry between fermionic and bosonic degrees of freedom. In this context I think of four colleagues whose paths I crossed at various times. There was the Russian mathematician Berezin whom I met during my first visit to Moscow probably in 1972. He tried to explain to me some parallelisms between bosonic and fermionic relations, in particular the analogue of integration over fermionic variables. Unfortunately I was unable to grasp the scope of this and I have forgotten all details. What I do remember is that Berezin took me on an unusual tour through Moscow. We met at some bus station and then walked for hours through areas of Moscow which one usually does not show to tourists. The slums we passed illustrated Berezin's bitterness against the regime under which he had been forced to spend his life. The terror had ended with Stalin's death but the feeling of imprisonment remained. In 1975, we succeeded in obtaining an invitation for Berezin to spend a year at CERN. But it was too late. We learned that Berezin had drowned in a river in Siberia.

The two persons whose work was most influential in propagating the idea of supersymmetry were Julius Wess and Bruno Zumino. They developed the formalism for a supersymmetric, Poincaré-covariant quantum field theory.

Supersymmetry had several good aspects to its credit. Some divergent graphs in perturbation theory cancelled. This was not very relevant since other bothersome divergent terms remained. More important was that the energy was represented by a positive operator. Thus no extra assumption was needed to ensure the positivity of energy. Furthermore it appeared that supersymmetry allowed a natural inclusion of exterior (non-geometric) symmetries. Though I was aware of these features and met Bruno Zumino at CERN almost daily I was first attracted to work on supersymmetry when my friend Jan Lopuszanski from Poland visited CERN and asked me why there couldn't be any fermionic charges. Indeed why not. He had just spent some time at the institute of Wess in Karlsruhe and together with Martin Sohnius, a graduate student of Wess, he had written a manuscript in which they introduced so-called "central charges". This gave some enlargement of the existing supersymmetric scheme. I got interested and proposed to Jan that we should find all supersymmetries which are compatible with a Poincaré-invariant S-matrix. It was the analogous question for supersymmetries which Coleman and Mandula had settled for ordinary symmetries [Coleman 1967]. This problem was not very hard and the result was mildly interesting because it gave a final answer. However the resulting scheme was not very beautiful. While the fermionic charges generated the space-time translations they did not generate the Lorentz transformations. These had to be added separately. A much more elegant scheme could be obtained when one allowed fermionic charges which did not commute with the linear momenta. Then the fermionic charges generated not only the full Poincaré group but also dilations and conformal transformations as well as a $U(n)$-group of external symmetries. This was somewhat contrary to our original aim since, unless the dilation symmetry is broken, it allowed only zero mass particles and no non trivial S-matrix. But as an algebraic scheme it looked very natural. During the time of computation, involving the checking of hundreds of generalized Jacobi identities, I had an extensive and very fruitful correspondence with Martin Sohnius. Then I went to see Jan Lopuszanski in Poland to talk it over with him. We had now enough material but I was somewhat tardy in writing it up. So ultimately Jan lost his patience and commanded: "Now you sit down at this desk and start writing." The ensuing paper [Haag 1975] attracted quite a lot of attention among the fans of supersymmetry.

Our friendship hat started 1968, when Jan Lopuszanski invited me to Wroclaw, where he was professor of physics.

My first visit to Poland in Winter 1968 had started in Wroclaw where Lopuszanski invited me to stay at his house. It proceeded to Karpacz, a mountain resort at the foot of Sniezka, the former German "Schneekoppe". There the University of Wroclaw owned a large house which could serve as a conference center. The physics department had organized a winter school there to which some colleagues from Western countries were invited as lecturers, among them David Ruelle, Nico Hugenholtz and myself.

There was a Russian delegation and a fair number of interesting colleagues from Poland.

On the way back after the workshop Ruelle and I had to go via Warsaw and spend a night there. The University of Wroclaw had booked a room for us at the hotel Bristol but when we got there the clerk at the reception desk told us that they could not accept the request and we would have to pay the rate for foreigners which was twice the rate of Polish citizens. It exceeded the amount of money we carried with us. David Ruelle blamed me for this because I had once said “Breslau” instead of “Wroclaw”. Anyway we decided that we had to activate the international brotherhood of theoretical physicists and we called Bialynicki-Birula, a colleague in Warsaw. He came, settled the affair with the receptionist and joined us for dinner at the hotel where an orchestra played evergreens by Emmerich Kálmán and many modestly dressed couples enjoyed dancing. I asked Birula “Who are the people who can afford to come for dinner and entertainment to such an expensive hotel?” His answer “Oh, just ordinary people who save money for a couple of months to be able to go out once in a while in luxury”.

After the year at CERN, I intended to continue working on supersymmetry and invited Martin Sohnius to come to Hamburg for some period. There was one obvious task: to arrive at a local version where the global fermionic charges were replaced by spinorial charge densities, possibly accompanied by some local fermionic gauge principle. We were not successful in this.

These years brought also two memorable extended stays abroad. Among them my first visit to Japan lasting six weeks. It was based on the Institute for Mathematical Sciences in Kyoto but the contract under which I was invited stipulated that I should visit a number of other institutions in the country, talk with the people there and give some lectures. Araki who was my host, took me to many places and was eager to introduce me to aspects of Japanese life such as the traditional Japanese Inn for staying overnight. He delegated the very competent and charming secretary Toshie, who had been to America and spoke fluently English, to see to it that I got something to eat in Japanese restaurants and to introduce me to the marvels of Japanese garden architecture around temples and imperial palaces. Araki also persuaded two young mathematicians to take me on a hiking tour in the Southern island Kyushu. So altogether I left Japan with vivid pictures and the wish to return soon. It took, however, more than 15 years till I could visit Japan again.

The other long visit was a three months stay at Berkeley, California following an invitation by I.M. Singer. He was a man of widely ranging interests who had contributed significantly to several branches of mathematics, co-author of the celebrated Atiyah-Singer index theorem. I had met Iz Singer on several occasions since he was a very old friend of Dick Kadison. We felt that it might be worth while to engage for a period in intensive discussions, hoping that once again the mutual inspiration between mathematics and physics might work and that our joint background could lead us to produce results in an area which could vaguely be called “quantum geometry”. This hope did not materialize. We produced nothing tangible together but at least for me the discussions were very fruitful.

Statistics claim that after the age of 35 or at most 40 the productivity of theoretical physicists quickly drops to zero. I do not believe the relevance of such statistics and therefore was not concerned about this message. But it is quite amusing to recall how various colleagues reacted to it. Harry Lehmann took it from the positive side and declared: “Now let the young people do something”. He saw his task now in keeping informed about new developments to be able to judge and give advice without the ambition to produce new ideas. Freeman Dyson also believed the verdict of the statistics and decided to change the field to astronomy or rather astrophysics where no age limit was proclaimed. Leon van Hove argued that it depended on the level from which one started and so he had a couple of years more time than others. George Uhlenbeck argued that the statistical verdict only concerned the revolutionary or romantic type, not the classical type of scientist to which he obviously belonged.

Fittingly the seventies ended with celebrations of the hundredth anniversary of the birth of Albert Einstein. I was invited to the celebration at the Institute for Advanced Study in Princeton. I remember the somewhat irreverent question by Rabi why Einstein produced nothing of lasting value in the last twenty years of his life in Princeton, comparable to his earlier achievements. Rabi suggested that Einstein had become too much drawn into mathematics. I do not agree with that. On the one hand it conforms with the general finding that no revolutionary idea in theory is produced beyond the age of 40 (Einstein was 54 when he came to Princeton) but also I see the parallel to Heisenberg. As a consequence of their brilliant achievements in the past their measuring rod had been placed so high that nothing short of the "world formula" could attract their aiming. They both produced one which earned great publicity but which ultimately disappeared again. – John Archibald Wheeler suggested in his talk that one needed two paradoxa to stimulate the creation of a new theory and he located one paradox in general relativity in connection with black holes and singularities, the other one in quantum physics as "no phenomenon is a phenomenon unless it is observed". I asked him later what he meant but still could not fully understand his position. Still I was glad to extract from him the clear statement "it has nothing to do with the mind". – Prof. Sciama from England urged me to look into the paper by Hawking on black hole radiation. I was surprised and thought first that he had mistaken me for somebody else, but no. He referred to our paper on stability [Haag 1974] and said that the Hawking effect has much to do with stability. A few years later I looked indeed into this problem and found the suggestion by Prof. Sciama essentially corroborated.

Open end

Since the mid-seventies Walter Thirring and Heide Narnhofer in Vienna had become interested in the algebraic approach to local quantum physics and especially to its use in statistical mechanics. So there developed some exchange of visits between Hamburg and Vienna. Heide Narnhofer came regularly for a couple of weeks per year to Hamburg. Gradually our collaboration focused on one topic: the Hawking effect of radiation by a black hole. It had become clear to us that for a satisfactory treatment one had to understand first the general principles of quantum physics in a given gravitational background, more specifically quantum field theory in curved space-time. Since the relativistic causal structure, with its distinction between space-like and time-like distances, survives, the algebraic part of the theory i.e. the assignment of C*-algebras to space-time regions with commutativity for space-like separation, carries over directly. But Poincaré-invariance is lost and with it the notion of a global energy as a positive operator defining a vacuum as its ground state. These properties played an important role in standard quantum field theory and one has to find a replacement for them in the case of curved space-time. Once one recognizes this and starts thinking the answer is evident. The infinitesimal neighbourhood of a point i.e. its tangent space is isomorphic to Minkowski space. We have "local Poincaré-invariance" and can define an energy operator in tangent space and demand its positivity. We called this principle "local stability". It gives a restriction for the allowed states and one shows that in the case of a spherically symmetric black hole this restriction demands that a stationary state in the outside region is a KMS-state with the Hawking temperature [Haag 1984]. In two subsequent papers [Fredenhagen 1987; 1990] I profited from a collaboration with Klaus Fredenhagen. The first was an effort to find a synthesis of the principles of general relativity with quantum physics. We started from a 4-dimensional differentiable manifold with a priori no metric structure and a net of algebras generated freely from test functions so that diffeomorphisms of the manifold act naturally as automorphisms on the net. A metric on the manifold and relations in the algebra are introduced by primary states. The set of admissible states is determined by their scaling limit (reduction to tangent space at a point) and their germs (their restriction to arbitrarily small neighbourhoods of a point). This is, of course, a minimalistic approach to the problem of quantum gravity but I think it should be more thoroughly investigated. – In the second paper we treated the time dependent process of the development of the radiation during the gravitational collapse leading to the formation of a black hole.

Some of the progress in the algebraic approach to local quantum physics during the 80ies I had already mentioned earlier [Buchholz 1986; 1982; 1987].

Another line of development starting from reference [Buchholz 1982] showed that for the description of exchange symmetry in lower dimensional space time the permutation group has to be replaced by the braid group [Fredenhagen 1989] leading to a much richer superselection structure and making contact with very recent developments in mathematics, the celebrated work by Vaughn Jones on towers of subalgebras defining an index for subfactors [Jones 1983]. From the side of physics there were the contributions by Longo and Rehren [Longo 1989; 1990; Rehren 1992; 1995].

One of the prime achievements of the algebraic approach to local quantum physics had been the analysis of possible superselection structures characterized by charge quantum numbers with their relation to exchange symmetries and global gauge transformations. It is deplorable that till now this approach has made no contact with the specific features of local gauge invariance which, after all, constitutes the essence of QED and QCD. The elegant formalism proposed by Becchi, Rouet and Stora [Becchi 1975] is far removed from algebras of local observables. It appears to me to be a task worth the sweat of the noble to fill this gap.

The mid-80ies marked the advent of “string theory” which gradually began to dominate the scene in fundamentally oriented theoretic physics. There was the suggestion that within this scheme only one unique possible physical theory remained. Hans Joos who was eager but sceptical said “This may bring a revolution of our ideas in the next few years or it will quickly disappear again.” Unfortunately neither one of these alternatives was realized. The hope to be led to a unique theory, a theory of everything, had disappeared soon. Instead one was led to lofty areas of mathematics with the challenge of developing novel branches. This was in parts an asset. It attracted mathematicians of the stature of Michael Atiyah who saw in string theory at last one range of ideas in physics which could stimulate developments of new mathematics. But for physics this was negative because it drew the approach more and more into speculative areas in which contact with questions subject to experimental test got lost. Still the prestige of string theory was steadily rising. I visited Princeton in the early 90ies. At that time Sam Treiman was head of the physics department at the university. I had known him since 1958 and highly appreciated his sober judgement. So I asked him about his assessment of the future of string theory. He said that he had not occupied himself with it but that he was supporting it without reservation because the people who worked on it were very very good. He meant primarily Ed Witten who was now the spearhead of this approach. I had been asked to give a physics colloquium talk about my views on quantum gravity and hoped to have some discussion with Ed Witten. Next morning he greeted me by saying: “Your talk was very interesting but I would really advise you to work on string theory”.

When he saw the somewhat incredulous look on my face he added "I really mean it. I shall send you the manuscript of the first chapters of our book". This ended our discussion. Back in Hamburg I received the manuscript but it did not convert me to string theory. I remained a heathen to this day and regret that meanwhile most physics departments believe that they must have a string theory group and have filled their vacant positions with string theorists. To be precise: It is good that people with vision like Ed Witten spend time trying to develop a revolutionary theory. But it is not healthy if a whole generation of young theorists is engaged in speculative work with only superficial grounding in traditional knowledge. In many popularised presentations the starting point of string theory is explained as the replacement of the fundamental notion of "particles" with its classical picture of a point in space or a world line in space-time by a string in space respectively a two-dimensional world sheet in space-time. This, I think, is a misunderstanding of existing wisdom. First of all, paraphrasing Heisenberg, one may say "Particles are the roof of the theory, not its foundation"[8].

Secondly points in space can not be defined as possible positioned of a particle in a relativistic theory.

String theory is hailed as the most promising among present endeavours. But it is an overstatement to call it a theory. It has not settled down to a well defined formalism nor has it explained any existing puzzle nor can I see that it can make contact with any observable phenomenon in the foreseeable future.

My colleague Walter Thirring in Vienna and Wolf Beiglböck from the Julius Springer publishing house urged me to write a book on the algebraic approach to quantum field theory and quantum statistical mechanics. I accepted but enlarged the topic to a presentation of my understanding of fundamental physical theory. It became a very personal book, mirroring my strengths and my weaknesses. I worked on it for five years. It was published by Springer in 1993 with the title "Local Quantum Physics".

The fall 1987 was the time of my official retirement from Hamburg University. I kept an office there for some more years and had some Ph.D. students working on aspects of quantum gravity. But in 1994 we left our residence in Pinneberg near Hamburg for good and moved to Schliersee, a small village at the edge of the Bavarian Alps. Apart from preparing a revised, slightly enlarged second edition of my book which came out in 1997 my scientific interest now centred on the project of developing an "event theory". I made some remarks about this earlier in these notes, but I think it is worthwhile to place it in context and make it more concrete since it became apparent that it involves modifications and enlargements of the Copenhagen interpretation which are not merely verbal.

The advent of quantum theory had shaken the firm ground on which 19th century physicists had stood: the belief that they were facing an external world governed by laws which were understood to a large extent. The essential turning point was Niels Bohr's conclusion from matrix mechanics that "it is impossible to assign any conventional attributes to atomic objects", and his criticism of the naive interpretation of Max Born's statistical interpretation of the Schrödinger wave function. It does not describe the probability for individual particles within an ensemble to have a definite position in space at a given time, but only the probability for the result of a position measurement.

Not all physicists were willing to accept this and there were various attempts to save the idea of particle positions changing in the course of time. To reconcile

[8] Actually Heisenberg said this about the S-matrix. But he considered particles as discrete points in the spectrum of the mass operator in analogy to the discrete energy levels of atoms, not as basic elements of the theory.

this with observed interference effects various proposals for changing the classical equations of motion were offered. I remember an attempt by Fritz Bopp introducing stochastic elements [Bopp 1955]. Best known is the proposal by David Bohm to obtain a deterministic theory by introducing a "quantum force". It still has a sizeable number of followers today [Dürr 1992]. In my opinion, however, this has no future, because it is too mechanistic and touches only the surface, being far from coping with phenomena of quantum optics, entanglement, creation, annihilation, and transmutation of particles in high energy physics.

The most influencial criticism of the Copenhagen school is contained in the writings of John Bell [Bell 1987]. On the other extreme, doubts about the existence of the external world gained weight. In his analysis of the measuring process, John v. Neumann pointed out that we might include the measuring instrument in the physical system considered, and then needed somebody to measure the results of this instrument. Eminent physicists like Eugene Wigner, at least temporarily, suggested that "If one formulates the laws of quantum mechanics in terms of probabilities of impressions, these are ipso facto the primary concepts with which one deals" and "The principal argument that thought processes and consciousness are the primary concepts is, that our knowledge of the external world is the content of our consciousness..." [Wigner 1967].

One fallacy of this is that we are not concerned with impressions of any individual consciousness, but with features we can communicate with others and find agreement. We may consider such features as if they were attributes of an external world. Niels Bohr did not put in question the reality of hardware used by an experimenter: "We must be able to tell our friends what we have done...".

To reconcile this with the "inability to assign conventional attributes to atomic objects", he introduced a cut between this world of every day experience and the microscopic world of atomic objects about which we can get knowledge only by amplified interaction processes with the macroscopic observation instruments. The totality of such observation results still cannot lead to an assignment of realistic attributes due to the "principle of complementarity".

This "Copenhagen interpretation" describes perfectly what is being done and learnt in a laboratory experiment. It is, however, not adequate for other areas of endeavour e.g. astrophysics and cosmology, where one would like to tell a history of the universe.

As already mentioned Archibald Wheeler spoke about the paradox in Quantum Mechanics "No phenomenon is a phenomenon unless it is observed" and clarified: "It has nothing to do with the mind." Thinking of it again I believe that he just wanted to emphasize that in orthodox quantum theory nothing happens without an observer and that he considered this as a paradoxon worth thinking about. I guess the answer lies as usual in a relaxation of orthodoxy.

Let us illustrate it by looking at a typical accelerator experiment, say $e^+ - e^-$ collisions in a storage ring. Scanning the bubble chamber pictures we find many typical fire works with a number of almost straight lines emerging from one single point, the vertex. Various types of such patterns occur and to each of these the experts can tell a story, for instance: "The vertex is the point where an electron and a positron annihilated each other. This resulted in the formation of a quark – antiquark – pair which fractured into many hadrones forming the two narrow jets leaving the vertex in opposite directions." Each pattern is undeniably a macroscopic fact and must be regarded as a record of a microscopic event described by such a story.

Now suppose that the detector fails for a day while the accelerator is fully working. During that time there will be no records while we know that previously in such a period there appeared between twenty and thirty pictures of interesting events. By inductive reasoning we believe that such a number of microscopic events did indeed happen and form part of reality though nobody could observe them.

What does the quantum theory of $e^+ - e^-$ collisions say? Starting with a two particle state describing an electron and a positron moving with high, almost sharp, momenta towards each other the dynamical laws lead to a final state whose state vector is a sum of many terms. Each of them corresponds to some configuration of outgoing particles. The standard interpretation is that the state describes a statistical ensemble of $e^+ - e^-$ pairs. The different terms in the final state describe different possible events and also give the relative frequency for their occurrence. This can be compared with experiment.

I omit here a discussion of the decoherence of phase relations between different patterns. It is obvious here and would detract from the main point to which I would like to draw attention: The quantum state cannot address individual events. It does not tell us which pattern will appear next nor where or when this will happen. But individual pattern are the facts from which reality is formed. The quantum state describes only possibilities and probabilities not reality. This entails several significant changes in our outlook. First we note that the emergence of an individual fact is irreversible [Haag 1990]. The fact did not exist in the past and cannot be undone in the future. This irreversibility is directly tied to the indeterminacy of phenomena in quantum physics. The deterministic dynamical laws in classical mechanics allow us to predict the future from the past and vice versa. Their formal quantization leads to a Schrödinger equation which shares with its classical counterpart the property of time reversal invariance. The difference is, however, that the Schrödinger equation does not deal with facts but with probabilities for them. Therefore, to the extent to which we can speak of individual processes lasting not from $t = -\infty$ to $t = +\infty$ (as one conveniently idealizes in the discussion of collision processes), but, say lasting for one microsecond, to be followed by a subsequent process separated by a clear time interval, the initial state of each process has a menue of possible outcomes out of which precisely one choice is realized. If we do not believe in the existence of hidden variables restoring a deterministic development then this random "decision by nature" remains an irreducible no more explicable feature. It is the central contribution of quantum physics to our "Weltbild" relieving us from the nightmare of a universe functioning as a clockwork. Instead we are led to an evolutionary picture as envisaged by A.N. Whitehead [Whitehead 1929]. Compare also [Weizsäcker 1973; Stapp 1977; 1979; Haag 1996].

I was careful to point out that all this reasoning depends on our ability to single out individual facts as parts of reality. I am aware of the holistic objection that this cannot be precisely satisfied because everything hangs together [Haag 1999; 2004]. Nobody can contradict that. But we must recognize that the focusing on individual elements whatever these may be is absolutely indispensable for all our thinking. It is the basis of mathematics, beginning with natural numbers, sets, relations... It has been the starting point for all our scientific efforts and, of course, it is a folly to believe that this can ever become a precise picture of the world. What is important is to make the right choice for the individual elements so that the subdivision catches the essential features and allows small corrections to be added later. In our context that means that we must first answer the question: "What may be regarded as an individual event?" The simplest regime is that of very low density, close to the vacuum. There it is clear that an individual event is a collision process. Its attributes are an approximate position in space-time and the types of incoming particles with their approximate momenta.

Here it is important to note that the connection to space-time (i.e. the marking of the approximate space-time-point) is given by events, not by the particles. Bohr's statement that we cannot assign any conventional attributes to atomic objects is sometimes coarsened to the weirdly sounding phrase: "An electron has no position unless it is measured." This formulation simply means: position is not an attribute of the electron but of an event i.e. an interaction process involving the electron. Just think of the bubble chamber picture, where the bubbles mark ionization processes serving as condensation kernels and the vertex marks the position of the central high energy event. We also expect that the sharpness of the position depends on the energy-momentum transfer of the process, i.e. that it is sharp for high energy processes and fuzzy for low energy transfer. In this context I remember a discussion with Julius Wess at the MPI in Munich. I asked him what was known about the sharpness of the vertex and he consulted an experimentalist. She answered: "I would like to know that, too. The uncertainties in the extrapolation of the tracks do not allow to get below 10^{-5} cm." Last but not least, what is the status of particles in this picture? Obviously the characterization of different types of particles by charge quantum numbers and internal structure was one of the great triumphs of quantum theory. These may be regarded as unconventional attributes, as constituent properties. But beyond this the notion of particles is essential as causal links between events and enters in the probability of their occurrence. It belongs to the quantum state, the realm of possibilities acquiring reality only after the target event is realized [Haag 1998]. This plays a role e.g. in the discussion whether in entanglement locality or reality is violated [Bertlmann 2002; Bell 1987]. In our picture the events have both properties, whereas for particles both locality and reality hold only in a restricted sense.

In particular the indistinguishability of particles in one species implies the loss of individuality of a particle as demonstrated by the Pauli principle or the phenomena of Bose-Einstein condensation. In the effect of Hanbury-Brown and Twiss two photons originating from two separated emission processes lead to correlations in twofold coincidences. This shows that a causal link does not necessarily connect a single source event with a single target event but can correspond to a more complicated graph. In 1956 this caused quite controversial reactions, but ultimately led to a great number of subsequent developments.

With increasing density more complex structures must be considered and a thorough discussion of decoherence is needed in which we will not enter here.

The move from Hamburg to our new residence in Schliersee implied of course the loss of daily contact with my old friends Joos and Lehmann, with my young collaborators Buchholz and Fredenhagen and of the atmosphere and the facilities of DESY. Instead I could cultivate contacts with institutions in Munich where I had quite a number of friends from old times. Julius Wess was now in Munich and was eager to get me a parking license at the institute of theoretical physics of the university. For some years I profited much from this.

Georg Süssmann organized a small, very high brow discussion circle with Karl Friedrich von Weizsäcker. Among the few participants there was usually Arnulf Schlüter, expert in astrophysics and many other fields, Jürgen Ehlers, the most prominent exponent of general relativity in Germany and, especially important for me, Berthold Georg Englert who was familiar with all recent experiments in quantum optics and atomic physics which had produced amazing tests of quantum mechanics. Our discussions roamed around many topics with one of us reporting about recent progress in his area of expertise. But it came always back to puzzles presented by quantum physics and our understanding of them.

After a few years my visits to Munich became rare. The discussion group with Süssmann and Weizsäcker had ended. A few times I met Julius Wess but though we both had the wish to learn from each other this never caught fire. There remained the fascination with recent experiments in quantum optics and atomic physics which Englert explained to me. Prof. Bonifacio from the University of Milano organized yearly conferences with the title "Paradoxes, Puzzles and Mysteries in Quantum Mechanics" in Gargnano at Lake Garda in Italy. Its logo was a T-shirt which the participants could buy showing Schrödinger's cat in two versions, alive and as a ghost. Englert had introduced me to these meetings and for some years I enjoyed participating in these wonderful conferences where you met leading experimentalists and thinkers about the foundations of quantum theory in a relaxed atmosphere overlooking Lake Garda from the Palazzo Feltrinelli [Haag 2001].

When Englert went to Singapore that marked the end of my close scientific contacts. Perhaps this is also a good place to end these memories.

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