Response to the Award of the Wigner Medal

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Abstract. An exciting journey through frontier science against obstacles now forgotten. Electrical engineering professors told us that there was no future in electronics. Niels Bohr told us that there was no future for quantum mechanics which was a theory for the atomic scale; the nuclear scale would need a new theory as different from quantum mechanics as quantum mechanics was from Newtonian mechanics. Fermi's theory of beta decay was wrong; a new theory was needed. The first experiment showing that relativistic positrons obeyed the Dirac equation. The nuclear shell model was nonsense. Parity was conserved, the Mossbauer effect was nonsense, group theory was useless and quarks were nonsense.

There were side trips like nuclear reactor dynamics, where this postdoc was allowed to test a theory of reactor stability by turning on a reactor and watching to see whether it would blow up like Chernobyl or stabilize according to his theory.

1. Introduction - What I learned from Wigner

I am very pleased to be honored at this meeting by the award of the Wigner Medal. I learned many things from my former teacher Eugene Wigner[1].

1.1. My first published paper in theoretical physics

When I was a graduate student at Princeton I suggested to Wigner one day at tea time that a particular radiative correction might explain a discrepancy between some new beta decay experiments (which were later shown to be wrong) and the Fermi theory. Wigner said "It sounds like a crazy idea, but why don't you calculate it?" I began the calculation, using the old quantum electrodynamics which I was just learning from Heitler's book before the new QED of Feynman, Schwinger and Tomonaga. If you don't know what you are doing and know the answer you want, you get it. But when I showed it to Wigner, he said "This is very interesting. But what is this? And how did you get that?" I learned a great deal by making all possible mistakes as he then guided me through the calculation. Although Wigner guessed correctly that this idea was useless he encouraged me to calculate the effect, to show that it would never explain anything, and then to publish it. I did not want to write the work up. It had nothing to do with my experimental thesis work, and it didn't solve the problem. But Wigner insisted. "You have done the work. You must publish it."

The publication[2] attracted the attention of a Harvard student, Eugene Merzbacher, whose thesis problem was a proper calculation of the same effect with the new QED. He confirmed that the effect was too small to ever be seen in any beta decay experiment.

1.2. An important lesson learned from Wigner

I always remember Wigner's remark: "I believe that this theory is wrong. But you know, the old quantum theory of Bohr and Sommerfeld was wrong, too. And it is hard to see how we could ever have reached the correct quantum theory without first going through this stage." I have been following this advice throughout my career and pursued approaches believed to be wrong by conventional wisdom; e.g. electronics, the future of quantum mechanics, the Mossbauer effect, group theory in physics, SU(3) symmetry and the quark model. In the remainder of this talk I shall try to describe how I became a theoretical particle physicist.

2. From Electrical Engineering to Physics

In high school in Rochester, New York in 1934 I liked building small radios and never thought of becoming a physicist. Before Hiroshima, most people had no idea what physicists did. I remember how a local draft board during World War II refused to defer physicists from army service. Their instructions were to defer only "chemists and scientists, but not physicists".

2.1. Cornell E. E. students told electronics impractical - no jobs, no future

When I entered the Electrical Engineering School at Cornell University in 1938 the curriculum included only one semester of electronics. The professors assured us that there were no jobs and no future in electronics. We had better study our machinery and power transmission courses and forget this useless electronics. Fortunately the university was flexible and allowed students to choose additional courses. Having heard that there were very interesting physics courses, some of us went over to the physics building and listened to two new refugee professors from Hitler's Europe, Hans Bethe and Bruno Rossi.

In the engineering school we learned that electrical energy traveled through wires. The engineers also knew that radio existed and that electrical energy also traveled through the air. But they didn't really understand it and it wasn't practical. In the physics department we learned about the basic properties of matter and energy without any pretense that this was practical. We also learned how electrical energy traveled through the air, as described by the famous equations of Maxwell, which engineering students did not study in those days.

2.2. Microwave radar at MIT

I graduated in 1942, after the U.S. had entered World War II, and joined the Radiation Laboratory at M.I.T. in the development of microwave radar. This was all based on electronics and electrical energy traveling through air, both considered impractical by engineers.

A radar receiver that I developed was produced by a well known industrial company in Chicago. I had to make a special trip from Boston to tell their engineers why the first model built according to my original design didn't work. The small changes introduced to facilitate mass production allowed electrical energy to travel through the air in peculiar ways that completely ruined the receiver performance. They looked at me as if I were crazy when I told them to move a wire soldered at one point on the chassis back to another point a centimeter away where I had originally put it. Every electrical engineer knew that when a chassis was grounded it made no difference where you soldered a wire to it. They had never worked with such high frequency, high gain amplifiers before. They moved the connection to humor this young fool and were amazed when all their troubles went away and the receiver worked. To them it seemed like black magic.

Electrical engineers with the standard training aimed at specializing in "practical" directions were unable to cope with the new phenomena of high frequencies and wave guides. They had learned how to solve the problems that were practical today, but were unprepared for the completely new problems that become practical and even urgent tomorrow. They knew all about how electrical energy traveled in wires but could not understand how it could travel through the air. They did not know how to design radar equipment and make it work.

The microwave radar program was an outstanding success. One of its major achievements, detecting German submarines from the air was possible because the German establishment had made a high level decision that radar at microwave frequencies was not practical. Their submarines were not equipped with microwave receivers which would warn them of an approaching airplane carrying microwave radar.

The key people at the Radiation Laboratory were all physicists, not engineers. The staff included Rabi, Alvarez, Bloch, Purcell, Schwinger, Dicke and many others close to the Nobel prize level.

2.3. Why I moved to physics

After working four years at M.I.T. as an electronic engineer, I decided to go to graduate school in physics and study the basic properties of matter and energy, rather than more "practical" subjects, in order to be better prepared for future developments.

3. From Experiment to Theory

3.1. Niels Bohr on Quantum Mechanics in 1946

Princeton in the late 1940's was very much influenced by the Copenhagen school and by the great giants who had carried through the remarkable revolution of the 1920's which completely transformed our ideas of space, time and continuity.

Niels Bohr and his associates told us that their revolution had succeeded in explaining all phenomena on the atomic scale, but was completely useless for the smaller nuclear scale. A new revolution was needed, leading to a theory as different from Copenhagen quantum theory as quantum mechanics was different from Newtonian mechanics. New revolutionary ideas like quantization of space-time might be needed. The road to the new physics would be paved by new exciting experiments whose results defied explanation by conventional quantum theory. I left Princeton as an experimentalist who had performed as a Ph.D. thesis the first experiment showing that relativistic positrons obeyed the Dirac Equation. But I never found any experiments showing that the old Bohr-Heisenberg-Schroedinger-Pauli-Dirac quantum theory was inadequate for the description of small distance phenomena.

Nobody has yet found such experiments. One of my quantum mechanics teachers at Princeton, David Bohm, tried very hard to find the keys to new physics in investigating the foundations of quantum mechanics. The Aharonov-Bohm effect, which caused great controversy when it was first proposed, is now one of the pillars of conventional Copenhagen quantum mechanics.

3.2. Experiment to test Dirac theory for relativistic positrons

My thesis work in experimental physics, Mott scattering of 1 MeV electrons and positrons, might be called in today's language "An experimental test of the standard model". It showed that the relativistic corrections to the Rutherford scattering formula which had opposite signs for electrons and positrons were correctly predicted by the Dirac equation. This was not easily achieved because positrons were only available from radioactive sources which had to be produced in cyclotrons and lived only a few hours.

The way I got my positrons would make anyone worried about nuclear safety and radioactivity shudder today. A copper target was bombarded all day in a cyclotron in Washington, D.C. Then a member of Princeton's administrative staff who knew no nuclear physics and nothing about nuclear safety flew down to Washington in a private plane, took the radioactive copper target from the cyclotron in a car to the airport, then flew it to Princeton in the private plane, and then took it by car to the Princeton physics building. A radiochemist then separated out the positron-emitting radioactive gallium and deposited it on my source holder. I then put it into my apparatus at about midnight and took data all by myself, day and night until the nine hour half-life radioactivity had decayed to the point where I could no longer get useful data. I repeated this several times to get enough data to confirm Dirac's theory.

4. The Beginnings of Nuclear Physics and Nuclear Energy in Israel

4.1. A Year in France

In 1950 when I came to Israel nobody in the country understood what a nuclear reactor was and how it worked, and it was impossible for students to get a Ph. D. in nuclear physics in Israel. The Israeli government sent three physicists including me to Paris in 1953 to learn about nuclear reactors. There I was asked to read an article by Alvin Weinberg about the nonlinear kinetics and stability of nuclear reactors. I generalized his treatment for a homogeneous reactor to the case of a heterogeneous reactor containing uranium and heavy water. I was then asked to test my theory on the French reactor ZOE at the Chatillon Nuclear Center. When a reactor is turned on, its power level increases. When it reaches the desired power level control rods are adjusted to keep the desired power. But suppose the reactor is left alone after it is turned on. Will the power stabilize itself at some level or will it blow up like Chernobyl? Lipkin's extension of Weinberg's theory said that the ZOE reactor would be stable. One shudders today to think that the French Atomic Energy Commission allowed this young postdoc who was only learning about nuclear reactors to test his theory one evening alone with two technicians at the reactor control room. Since we were working in the evening, our dinners were brought from the laboratory restaurant. Since this was France, the dinner included a carafe of wine. And since the technicians were working overtime, they received an extra carafe of wine with their dinner. We turned the reactor on and the technicians read the power level and uranium and heavy water temperatures at regular time intervals. It fit my theory, I wrote the paper and everyone was happy. The reactor was stable and did not blow up.

Some time later Weinberg invited me to visit Oak Ridge and introduced me to a reactor engineer and a mathematician who told me that I had found an application of Liapounov's second method for stability. We then wrote a paper about this mathematics.

4.2. Back in Israel

In 1954 the three of us came back to Israel along with three other Israeli nuclear physicists who had just returned from study abroad. The first stage toward developing any realistic nuclear energy program was to enable students to learn basic nuclear physics at home instead of abroad. This was achieved by founding a new Nuclear Physics Department at the Weizmann Institute and establishing a center for basic research with the installation of the country's first nuclear accelerator and a graduate training program. I became the country's leading expert in nuclear reactor physics and our group played key roles in the initial stages of the Atomic Energy program which led to the building and operation of Israel's two nuclear reactors at Nahal Sorek and Dimona.

The theoretical work in the department began with work on nuclear structure using the then very new nuclear shell model. The collective model and the applications of group theory and the new BCS theory of superconductivity to nuclear physics were also investigated. Rehovot rapidly became an international center for nuclear physics. The 1957 International Conference on Nuclear Structure was the first such physics conference held in Israel. I was on the organizing committee, edited the proceedings and prepared a list of humorous daily bulletins and jocular physics articles. These jokes are probably remembered much more than my physics and led to the founding of the "Journal of Irreproducible Results."

The applications of the mathematical techniques of group theory to collective motion and nuclear many-body problems led to the development of simple models which have by now become classics[3] and of the spectrum-generating algebra[4] for which I have now been awarded the Wigner Medal.

5. Parity and the Mössbauer Effect

In the late 1950's two opportunities arose for exciting research requiring only radioactive sources and simple detectors. Experimental breakthroughs in two areas of nuclear physics called "parity nonconservation" and the "Mössbauer effect" opened up possibilities for us to get in at the very beginning of these rapidly developing areas of frontier research.

In 1957, after the experimental discovery of parity nonconservation, I developed a "double-scattering method for measuring beta ray polarization". This simple extension of my Ph. D. scattering experiment became a classic described in textbooks. I spent the academic year 1958-59 at the University of Illinois in Urbana directing Hans Frauenfelder's group doing beta-ray polarization experiments, while Hans was on sabbatical at CERN.

During the summer of 1958 I lectured at the Ecole d'Ete at Les Houches and included the Spectrum-Generating Algebra[4]. I also visited Princeton and told Wigner about this work but did not do a very good job at explaining it. The name "spectrum generating group" had not yet been invented, and Wigner could not understand the point of a group that was not a symmetry of the Hamiltonian of the harmonic oscillator.

During that year in Urbana, we heard about Rudolf Mössbauer's discovery of the effect which now bears his name and won him a Nobel Prize. His original experiments were misunderstood and greeted with skepticism by the physics community because its understanding required the combination of languages of nuclear and solid state physicists who did not talk to one another. I learned enough of both at Urbana to become a Mössbauer expert, was the first to suggest that the effect was important enough to be called the Mössbauer Effect[5], and began to work on it with the Frauenfelder group..

6. From Groups and Many-body Physics to Particle Physics and Quarks

6.1. Lie Groups for Pedestrians

In his recollections Wigner[6] refers to Pauli's derisive popular label "Die Gruppenpest"and attributes this resistance to group theory to the absence of a first-rate textbook. Wigner's "little book" published in 1931 helped but did not solve this problem. In 1950 the top young particle theorists at Princeton including at least four future Nobel prize winners were sure that group theory was completely useless. None attended Giulio Racah's now famous Princeton lectures on "Group Theory and Spectroscopy", They thought that isospin was a rotation in some abstract three-dimensional space and did not realize that the combination of isospin and strangeness symmetries was $SU(2) \times U(1)$. Knowing nothing about unitary groups they spent eight years looking fruitlessly for higher symmetries by considering only rotations in higher and higher dimensions until Gell-Mann found SU(3) by accident..

Racah's remarkable insight into the relevancy of group theory for physics was evident in his lectures, but not in his papers and unavailable in the existing literature.. My ability to translate what I had learned from Racah into a language understandable to physicists produced a series of lecture notes that eventually appeared in books "for pedestrians". "Beta Decay for Pedestrians" gave physicists the tools to calculate angular distributions of the new parity experiments. "Lie Groups for Pedestrians"[7] enabled the nuclear and particle physicists to understand the group theory they could use. It also included my own original development and classification of the algebra of bilinear products of second-quantized creation and annihilation operators. Lie Groups for Pedestrians has now been reprinted by Dover and is available at a price students can afford.

In 1958 Victor Weisskopf told me about one such new algebra found by Arthur Kerman for nuclear physics with three operators satisfying angular momentum commutation rules. In Urbana I heard the same algebra described by Phil Anderson who had found them independently for the electron gas[8]. As an interpreter between these two groups who did not talk to one another, I told each about the other and noted that they had really discovered a two dimensional symplectic algebra that was isomorphic to the algebra of three dimensional rotations.

In September 1967 I attended an international conference in Warsaw celebrating the 100th anniversary of the birth of Marie Sklodowska Curie. My invitation to the conference had been sent in April, 1967. But in June 1967 after the six-day war Poland broke off diplomatic relations with Israel. I had no idea whether I would be welcome in communist Poland. The conference organizers did everything possible to make me feel welcome. They sent a young Polish student Richard Kerner to meet me at Warsaw airport and reassure me that everything was OK. I very much appreciated Richard's hospitality and gave him an autographed copy of my book "Lie Groups for Pedestrians." I lost track of Richard until July, 2002, when he re-introduced himself at this conference and asked me to add another note to the book that he had kept all these years.

6.2. Unitary Symmetry

The 1960's brought a sudden realization that group theory could be useful for particle physics and brought us to frontier particle physics. There was no particle theory at all in Israel when Yuval Ne'eman returned from London and gave a seminar in Rehovot about his "Eightfold Way" Unitary Symmetry.. But in the nuclear physics group at Weizmann Carl Levinson and Sydney Meshkov were using the group $SU(3)$ to study nuclear structure and realized that they had all the mathematical tools needed to calculate experimental predictions from the new theory. Since particle physicists knew no group theory at that time we were able to get into the lead in this activity.

In the spring of 1961 at a small meeting on unitary symmetry at Imperial College I told Abdus Salam about our $SU(3)$ calculations for proton - antiproton annihilation into two mesons. Salam said new experimental results from CERN were now available to be compared with our predictions. We went up to his office after the session and found that the data did not fit the predictions from the Eightfold Way, but favored the currently competing Sakata model. Salam was flying to Pakistan the next day and suggested that I write up the paper together with my colleagues in Israel.

Back at Weizmann Carl, Syd and I immediately saw that Salam and I had looked in the wrong column of a table of $SU(3)$ Clebsch-Gordan coefficients. The results from the right column were even more exciting but opposite. They killed the Sakata model and left the Eightfold Way in agreement with experiment. We immediately wrote up the paper. But we couldn't leave Salam out of it, because he had been in on the original idea. We couldn't put his name on the paper because the conclusions were now reversed. and there was no communication between Israel and Pakistan.

I sent the paper to my good friend Gerry Brown who was then starting a new journal "Physics Letters". I explained the situation and left further processing to his discretion. He found that a Pakistani student of Salam's, Munir Ahmed Rashid, had independently discovered the error. Gerry accepted the suggestion from Imperial College that Salam's and Rashid's names be added to the list of authors. So a paper appeared as a collaboration of three Israelis and two Pakistanis[9].

6.3. Beyond SU(3) symmetry to quarks

The work on symmetries continued as a group of very talented students (now all professors) joined in the effort, which culminated in the development of a symmetry called $SU(6)_W$ with the W for Weizmann. The group continued the tradition of plunging into new areas before they became fashionable by developing the quark model seriously while the particle physics establishment rejected quarks as nonsense. Combining the quark picture with dispersion relations led to a new approach called duality. the "Veneziano dual resonance model"[10] and "duality diagrams".

With this background I eagerly embraced the quark model as the key to new physics. Experiments told us that quarks were real objects. Establishment theorists insisted that quarks were nonsense. Their arguments recalled the arguments against Bohr's atom in which electrons moved in orbits without losing energy by radiation. Nobody found free quarks, but more and more experiments showed that hadrons were made out of quarks. Was this the key to the new revolution? Unfortunately no. Bohr, Heisenberg, Schroedinger, Pauli and Dirac can still explain everything with appropriate mathematical techniques.

When SLAC found scaling, Bjorken and Feynman explained it all with the quark-parton model, and the theory establishment insisted that this was all nonsense, high energy physics looked exciting again. Bjorken and Feynman were clearly describing the physics of the real world. Was this an opening to the new physics which theorists could not explain? Again disappointment. Someone found how to explain the experiments with the old theories.

7. Conclusions

The history of high-energy physics in the second half of the twentieth century has been the carrying of 1920 quantum mechanics into higher and higher energies and smaller and smaller distances finding very interesting physics and many new phenomena completely unexpected in 1950. But no new revolution. The quantum mechanics of Bohr, Heisenberg, Schroedinger, Pauli and Dirac not only stood the test of time. They were the pillars of the new knowledge accumulated that completely changed the quality of life of the ordinary citizen in ways that were completely unimaginable a half century ago when radio and television were in their infancy,. Transistors, lasers, personal computers, cellular telephones and the internet did not exist, and all required for their development the application of quantum mechanics in ways that the creators of quantum mechanics could never imagine.

All this makes one wonder how to direct promising young scientists toward fruitful applied work. What is most practical today will probably be out of date tomorrow. We cannot tell researchers to concentrate on directions which will be important in the future.

Who can predict the future? My professors at the university could not foresee the importance of electronics. Niels Bohr could not foresee the future consequences of his own quantum mechanics. When I hear some older people trying to tell younger people what they should be doing, I am reminded of the words my father used to say to me when I thought I had been very clever. ”If you knew what you don’t know, you would know more than you know.”

Graduate study in a good university in a pure science provides the training necessary for work in new areas which cannot possibly be anticipated at the time the student begins his studies. The student learns to solve new problems by developing new techniques and discovering new things. Exactly what he develops and what he discovers at this stage is not so important. It is learning the approach to search and discovery and gaining experience in attacking new problems, where one cannot find the techniques for solution in any text book or hand book, and one has to work it out all alone.

For this conference it is appropriate to note that the developments of the past half century have completely overturned our understanding of the fundamental building blocks of matter and the forces that bind them them together. Instead of neutrons and protons bound into nuclei we have quarks and gluons bound into new families of hadrons unknown in 1950. And a crucial ingredient of our new understanding is group theory. We have come a long way from the days when particle physicists had never heard of SU(n), thought it irrelevant to physics, and discovered the algebra of SU(3) by examining the commutators of weak currents without knowing group theory.

8. Epilogue - The Source of Wigner’s Encouragement

I return to Wigner and find in his book of recollections[1] some insight into his encouragement to me. In discussing the exciting colloquia where he heard great physicists as a student in Berlin, he writes: “One element missing from the colloquia was concrete encouragement. Einstein was very kind to young physicists, but even he did not push us along as he might have done. He never said, ‘Look here, this idea of yours is quite promising. Why don’t you work it out and publish it?’ I waited in vain to hear such words.”

Wigner evidently kept such words in mind for young students like me after he became a world famous professor.

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