The relativistic electron wave equation*

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I would like to talk to you about events that happened 50 years ago. It was a period of great excitement in physics, and I would like to try to convey to you some of this feeling of excitement. Also I want particularly to tell you why it was that I thought of things rather differently from other physicists at that time and was led to follow a line of my own.

Before this period that I want to talk about, that is to say in the early 1920's, we had a period of frustration. We had the theory of the Bohr orbits. These orbits worked very well for some simple problems, essentially for those problems where only one electron was playing an important role. People were trying to extend the theory to deal with several electrons, for example to the spectrum of helium, where two electrons are concerned, but they did not know how to do it. There were basic ambituities in applying the rules of quantization and people did not known what to do. They could only proceed by making various artificial assumptions and these assumptions were not very successful.

Now this frustration is something that one can understand again very well at the present time, because we have a similar situation with regard to the relativistic quantum theory for dealing with high energy particles. Again we have this feeling we don't know the basic rules. We know some rules which work only with a limited degree of success and essentially we are in a similar situation where we don't know what are the correct basic assumptions that we can hold fast to.

Well, I spent two years in this period of frustration, and two years is long enough to appreciate it. I felt the basic helplessness of the situation and I was wondering if one would ever make any progress in getting a real understanding of atomic mechanics.

Then, the whole situation was suddenly changed by Heisenberg in 1925. He had a really brilliant idea. He was led to introduce the idea of noncommutating algebra into physics. This idea was most startling and most unexpected. And of course, Heisenberg was led to it only in an indirect way.

The outline of Heisenberg's method was to set up a theory dealing with only observable quantities. These observable quantities fitted into matrices, so he was led to consider matrices, and he had the idea of considering the matrix as a whole instead of just dealing with particular matrix elements. Dealing with matrices one is then directed to noncommutative algebra.

Now it was really very difficult for physicists to accept noncommutative algebra in those days. Heisenberg himself had very grave doubts when he first noticed that his algebra was actually noncommutative, and he wondered very much whether he wouldn't have to abandon the whole idea because of the noncommutation. But still he found that it was unavoidable and he had to accept it.

I learned about this theory of Heisenberg in early September of 1925 and again it was very difficult for me to appreciate it at first. It took about two weeks; then I suddenly realized that the noncommutation was actually the most important idea that was introduced by Heisenberg. It was the one drastic new idea which would provide the whole basis of any new theory which one was going to construct. Working with his matrices, Heisenberg was led to a new equation of motion for them, namely

$$i\hbar\frac{du}{dt}=uH-Hu, \tag{1}$$

where u is some dynamical variable and H is a diagonal matrix which represents the energy.

I was thinking over Heisenberg's ideas, concentrating on the non-commutation, and it occurred to me rather by accident that there was really a great similarity between the commutator of two quantites that don't commute and the Poisson bracket which we have in classical mechanics. As a result of this similarity, the equations in the new mechanics with noncommutation appeared as analogous to the equations in the old mechanics of Newton, when these old equations were expressed in the Hamiltonian form. On the strength of this analogy one immediately had a general connection between the old mechanics and the new mechanics of Heisenberg.

That was the start of my work. It gave me a rather different handle from Heisenberg because I had the noncommutation right as the essential new feature of my work.

The idea of bringing in noncommutation proved to be the key to developing a new mechanics, which enables one to escape from the frustration that had been holding us up during the previous years. The result was a period of great activity among theoretical physicists at

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that time. Great excitement together with great activity. There was so much work to do developing the new ideas and seeing how the equations of the old mechanics could be translated into the new theory. One could get the new results very easily and one had great confidence that one was really getting somewhere. One had the possibility of developing the new theory in a general way and also of applying it to examples and working out equations.

These equations involved noncommuting quantities. There was the problem of getting some physical interpetation for the results that were obtained with the new equations. This problem of getting the interpretation proved to be rather more difficult than just working out the equations. It was not completely solved until two or three years after the original idea of noncommutation was introduced.

I don't think it has ever happened in physics before that one had equations before one has known the general was to interpret them. But that is what happened in this case.

In the early examples one just had special rules for interpretation. For example, one had a matrix to represent the energy, a diagonal matrix, and one said its diagonal elements were the energy levels. That was just a special assumption giving us the energy levels, and it worked.

To get a general interpretation one was helped by some other work that was done independently by Schrödinger. Schrödinger was working quite independently of Heisenberg, and to begin with he knew nothing about Heisenberg's work. Schrödinger was working from an equation of de Broglie. This was the wave equation

$$\left(\frac{\partial^2}{c^2\,\partial t^2}-\frac{\partial^2}{\partial x_1^2}-\frac{\partial^2}{\partial x_2^2}-\frac{\partial^2}{\partial x_3^2}+\frac{m^2c^2}{\hbar^2}\right)\psi=0. \quad (2)$$

De Broglie had proposed this wave equation simply because he noticed that there was an interesting connection between its solutions and the relativistic motion of a particle. If you assume that p_r stands for the three components of momentum with $p_0=W/c$, then $p_\mu\psi$ corresponds to $i\hbar\partial\psi/\partial x^\mu$.

With this connection between the waves and the momentum variables of a particle one had a relativistic theory. De Broglie postulated these waves associated with the motion of a particle. He did that before Heisenberg had introduced his quantum mechanics. He did it in 1924.

I had read this paper of de Broglie, but did not take the waves seriously. I thought these waves were just a mathematical curiosity without any physical importance. There I was wrong. Schrödinger did take these waves seriously. He thought that they really would be associated with the motion of an electron in an atom, but one would have to modify the wave equation somewhat to take into account the electromagnetic field in which the electron was moving.

He tried to guess a good way to modify this equation (2) of de Broglie keeping to the requirements of relativity. Well, he was able to guess this equation:

$$\Big\{\left(i\hbar\frac{\partial}{c\,\partial t}+\frac{e}{c}A_0\right)^2-\left(i\hbar\frac{\partial}{\partial x_1}-\frac{e}{c}A_1\right)^2-\left(i\hbar\frac{\partial}{\partial x_2}-\frac{e}{c}A_2\right)^2 -\left(i\hbar\frac{\partial}{\partial x_3}-\frac{e}{c}A_3\right)^2-m^2c^2\Big\}\psi=0. \quad (3)$$

This equation reduces to the previous equation (2) when you put the electromagnetic potentials A_μ equal to zero. So far as I know it was just guesswork of Schrödinger to obtain this equation from de Broglie's equation.

Now when Schrödinger had that equation, the first thing he did, of course, was to apply it to the electron in the hydrogen atom. He worked out the energy levels of hydrogen, and he got a wrong result. The reason why he got a wrong result was that his equation did not take into account the spin of the electron.

Now, at that time the spin of the electron was unknown. Some physicists had thought about it, in particular Kronig had thought about it and had suggested it to Pauli. Kronig was then working in Pauli's school. Pauli said: oh no, the spin of the electron is quite impossible. Pauli often first had a wrong impression about a new idea. Well, poor Kronig was completely crushed by the authoritative opinion of Pauli.

The idea of the spin of an electron occured independently to Goudsmit and Uehlenbeck, who were working in Leyden. They wrote up a little paper about it and presented it to their professor, Ehrenfest. Ehrenfest liked the idea very much. He was quite excited about it. He told Goudsmit and Uehlenbeck to go and talk it over with Lorentz in Haarlem. Well, they went to Haarlem and spoke about it to Lorentz, and Lorentz said no, it isn't possible. I have myself worked on the idea of the electron having a spin, and I found that the surface of the electron would have to move faster than light, and so the whole idea is quite impossible. Lorentz was making the mistake of taking the classical model of the electron too seriously.

Goudsmit and Uehlenbeck were completely crushed by Lorentz. They went back to Ehrenfest and asked to withdraw their paper. Ehrenfest said it is too late, I have already sent it in for publication. In that way the idea of the spin of the electron got published. We really owe it to Ehrenfest's enthusiasm and impetuosity that it got published.

Schrödinger knew nothing about this. He found his wave equation gave results in disagreement with observation and he was very depresed about it. He abandoned the work for the time being.

He went back to it a few months later and then noticed that if he was less ambitious and just wrote his equation in a non-relativistic way and then applied it, he got results in agreement with observation apart from the fine structure of the hydrogen spectrum, which depends on the relativistic corrections. In the non-relativistic approximation Schrödinger's equation reads like this, in the absence of a magnetic field:

$$i\hbar\frac{\partial}{c\,\partial t}\psi=\left\{-\frac{\hbar^2}{2m}\left(\frac{\partial^2}{\partial x_1^2}+\frac{\partial^2}{\partial x_2^2}+\frac{\partial^2}{\partial x_3^2}\right)-\frac{e}{c}A_0\right\}\psi. \quad (4)$$

With this non-relativistic approximation, one had results in agreement with observation for the energy spectrum of hydrogen. One had both a discrete spectrum giving the spectrum lines of hydrogen, and a continuous spectrum corresponding to the electron being scattered by the hydrogen nucleus.

After this success, this limited success, of Schrödinger, one had two quantum theories. The one based on the wave equation of Schrödinger and the Heisenberg one.

I know when I first heard about these two quantum theories, I felt a bit annoyed. If we have one good theory, that is all we really want. This was rather too much, an excess of richness. But it was very soon shown by Schrödinger that the two theories are really equivalent to one another. You may write the Schrödinger equation

$$i\hbar\frac{\partial}{\partial t}\psi = H\psi, \tag{5}$$

and then this H corresponds to the matrix H in the Heisenberg theory. It was then just a question of a mathematical transformation to pass from the Heisenberg theory to the Schrödinger theory. They were two mathematically equivalent theories for the same underlying physics. That underlying physics is what we now call quantum mechanics.

We then had a satisfactory situation of one good theory. The result of Schrödinger's work was to introduce a new concept, the wave function ψ, which was a great help for the physical interpretation of the theory. It was found, that if you take ψ, and suitably normalize it, then $|\psi|^2$ gives the probability of finding the particle in any place.

One had to get used to the idea that the new mechanics only gave one probabilities and did not give one the determinism of the previous classical mechanics. That was a feature which a lot of physicists found very hard to accept, but which turned out to be unavoidable when one had more power for understanding the results of calculations with the noncommutative algebra.

I was working on this and considering the problem of getting the probability for other dynamical variables to have specified values. I worked out a general theory for these probabilities. This general theory enabled one to transform the Schrödinger wave function to other forms. One then had the possibility of calculating the probability of any dynamical variable having a specified value, or of several variables simultaneously having specified values, provided they commute with each other. The method was to transform the Schrödinger function to refer to these variables that one is interested in, and again, to form the square of its modulus.

I was able to work out this general transformation theory and I felt very pleased with it, I think that is the piece of work which has most pleased me of all the works that I've done in my life. It pleased me because it did not come from some lucky accident; it came from logical thinking step by step, seeing each step giving rather more detailed knowledge and leading on to the next question to examine and resolve. And in this step by step way I was able to pass to a general theory.

As a result, one had a pretty powerful method of interpreting the new mechanics. One then had a really satisfactory theory in many respects. One was able to deal with all dynamical variables and one saw that the most one could calculate about them was probabilities for variables that commute with each other.

There was just one bad feature of this new theory. That is the feature that it was not relativistic. It would not apply to particles moving with speeds comparable with the velocity of light, because it was based on the Newtonian pre-relativity mechanics. The operator on the right hand side of (4) corresponds to the energy in Newtonian mechanics and not according to Einstein. This expression has to be modified for particles moving with high speeds.

According to Einstein a theory should be basically symmetrical between the time and the three space coordinates. Now, you see that we do not have that symmetry. In (1) we have $\partial/\partial t$, but no corresponding $\partial/\partial x_1$, $\partial/\partial x_2$, $\partial/\partial x_3$. In the Schrödinger equation (4) or (5) we again have $\partial/\partial t$ and no corresponding operators of differentiation with respect to the space coordinates. So we had the problem to modify the theory to make it relativistic.

The way most physicists tackled that was to go back to equation (3), the extended de Broglie equation. This is a relativistic equation. It was first discovered by Schrödinger and was not published by him because it gave results not in agreement with observations for the hydrogen spectrum. It was rediscovered independently by Klein and Gordon and they did publish it. They were not deterred by its disagreement with observation. So this equation is now known as the Klein-Gordon equation. It should be, of course, the Schrödinger equation, but Schrödinger was not bold enough to publish it.

Now, this is a relativistic equation and one can develop it relativistically. One can set up the expression

$$\left[\left(i\hbar\frac{\partial}{\partial t}+eA_0\right)\psi\right]\bar{\psi} + \text{conjugate complex}$$

and can interpret it as the charge density associated with any solution of the wave equation. And one can put down corresponding expressions for the current density to satisfy the requirements of relativity, and one finds that charge is conserved. Further, one can put down expressions for the energy density and momentum density and for the stress. These expressions are all relativistic and in agreement with the conservation laws.

Now most physicists were very happy with this development of the Klein-Golden equation. They said, here you have a good relativistic quantum theory. But I was most unhappy with it, because you cannot apply the transformation theory to it. For the transformation theory you need this equation (5) of Schrödinger, involving just the operator $\partial/\partial t$, and not the square of this operator, such as occurs in (3).

The transformation theory had become my darling, I wasn't interested in considering any theory which would not fit in with my darling. I remember a discussion about it with Bohr at the Solvay Conference in 1927 in the autumn. Bohr seemed to be pretty satisfied with the Klein-Gordon theory. I didn't have time to explain my objections fully to Bohr on that occasion, but I could see where his opinion lay, and that was the opinion of most physicists of that time, perhaps of all of them.

I just had to worry over the problem of getting a relativistic theory which should be linear in the operator $\partial/\partial t$. The linearity in $\partial/\partial t$ was absolutely essential for me, I just couldn't face giving up the transformation theory. You see with the transformation theory you could work out also the probability of the particle having given momentum values. You couldn't do that at all with the Klein-Gordon equation. You could only work out the charge density, you could not even work out the probability of the electron being anywhere. You could not use the expression for the charge density, because that would sometimes give you negative values for this probability. If you wanted to find the probability of the momentum having specified values you cannot answer the question at all. Similarly for other dynamical variables, you cannot get any information at all about their probabilities.

So I continued to worry about this question till the end of 1927, and eventually the solution came rather by accident, just by playing with the mathematics. I noticed that if you take the matrices σ_1, σ_2, σ_3 describing the three components of spin for a spin of half a quantum as described by the general transformation theory, then if you form

$$(\sigma_1 p_1 + \sigma_2 p_2 + \sigma_3 p_3)^2,$$

you get a very interesting result, just

$$p_1^2 + p_2^2 + p_3^2.$$

You had thus a sort of square root for $p_1^2 + p_2^2 + p_3^2$.

Now I needed a corresponding expression for the square root of the sum of four squares. One had to have the sum of these three squares plus a mass term. One could not get an expression for the square root of the sum of four squares just by working with these three σ matrices, (which are called the Pauli matrices because he had built up the theory of electron spin in terms of them). That was a serious difficulty for me for some weeks, until I noticed that there is really no need to keep to two-by-two matrices like the σ's. One can go to four-by-four matrices, and then one can easily get an expression for the square root of the sum of four squares.

That led me to a new wave equation

$$\left\{ i\hbar\left(\frac{\partial}{c\,\partial t} - \alpha_1\frac{\partial}{\partial x_1} - \alpha_2\frac{\partial}{\partial x_2} - \alpha_3\frac{\partial}{\partial x_3}\right) + \alpha_4 mc \right\}\psi = 0, \qquad (6)$$

involving these α's, which are four-by-four matrices. They are required to satisfy certain algebraic conditions, as a result of which the square of this operator is just $p_1^2 + p_2^2 + p_3^2 + m^2c^2$.

Here we have a wave equation which satisfies the requirement of being linear in the operator $\partial/\partial t$, and therefore one can apply the general transformation theory to it, a feature which I consider essential. Also, one can show that it is really a relativistic equation. It's not obvious that it is so. You see it is linear in $\partial/\partial t$ and similarly linear in $\partial/\partial x_1$, $\partial/\partial x_2$, $\partial/\partial x_3$. But even so one has to make a certain calculation to check that one can apply Lorentz transformations to it and bring it back to its original form. One sees in that way that it is really a relativistic equation.

One can modify this equation (6) to bring in the electromagnetic field in the same way that Schrödinger brought in the electromagnetic field to the de Broglie equation (2). The result is an equation for the electron moving in the electromagnetic field, in agreement with the basic requirements of relativity and quantum mechanics.

It was found that this equation gave the particle a spin of a half a quantum. And also gave it a magnetic moment. It gave just the properties that one needed for an electron. That was really an unexpected bonus for me, completely unexpected.

At that time I only wanted a quantum theory which would satisfy the general requirements that one could apply the transformation theory to it, and the requirements of relativity. It turned out that the simplest particle satisfying those requriements is a particle with a spin of a half. That was a great surprise to me, I thought that the simplest particle would naturally have a zero spin, and that a spin of a half would have to be brought in later as a complication, after one had solved the problem of a particle with no spin. But it turned out otherwise.

I applied this equation to the electron in a hydrogen atom in the first approximation and got results in agreement with observation. This equation automatically gives the correct magnetic moment, and that's why it did not have the error which the Klein-Gordon equation had in giving the wrong results for the spectrum of hydrogen.

There was one further difficulty left with this equation, namely, it was quite possible for the particle to have states of negative energy. I was well aware of this negative-energy difficulty right at the beginning, but I thought it was a less serious difficulty than the other, less serious than our not being able to apply the transformations of the general transformation theory.

This negative-energy difficulty was solved a little while later by my idea of bringing in the exclusion principle of Pauli for electrons, the principle that one cannot have more than one electron in any state, and making the rather bold assumption that all the negative energy states in the vacuum are filled up, and when there is a hole in the negative energy states it appears as a physical particle. It would be a particle with a spin similar to that of the electron and it would have a positive charge instead of the negative charge of the electron, and it would have a positive energy.

When I first thought of the idea I thought that this particle would have to have the same mass as the electron, because of the symmetry between positive and negative masses and energies which occurs all the way through this theory. But at that time the only elementary particles that were known were the electron and the proton. I didn't dare to postulate a new particle. The whole climate of opinion in those days was against postulating new particles, quite different from what it is now. So I published my work as a theory of electrons and protons, hoping that in some unexplained way the Coulomb interaction between the particles would lead to the big difference in mass between the electron and the proton.

Of course I was quite wrong there and the mathematicians soon pointed out that it was impossible to have such a dissymmetry between the positive and negative energy states. It was Weyl who first published a categorical statement that the new particle would have to have the same mass as the electron. The theory with equal masses was confirmed a little later by observation when the positron was discovered by Anderson.

At that stage we had a satisfactory theory, not for a single particle, but really for several particles, because with this theory one could have electrons jumping between positive and negative energy states and such jumps would correspond either to the simulteneous annihilation of an electron and a positron or the simultaneous creation of an electron and a positron. The number of particles was no longer conserved. This was a physical development which was quite acceptable at that time and the final result was a theory in agreement with the transformation laws and with relativity.

It was pointed out by Pauli and Weisskopf that one could get a similar theory of several particles by working from the Klein-Gordon equation and taking the expression for the energy density, which is

$$\left(i\hbar\frac{\partial\psi}{c\,\partial t}+\frac{e}{c}A_0\psi\right)\left(-i\hbar\frac{\partial\bar{\psi}}{c\,\partial t}+\frac{e}{c}A_0\bar{\psi}\right) + \sum_r\left(-i\hbar\frac{\partial\psi}{\partial x_r}+\frac{e}{c}A_r\psi\right)\left(i\hbar\frac{\partial\bar{\psi}}{\partial x_r}+\frac{e}{c}A_r\bar{\psi}\right)+m^2c^2\psi\bar{\psi}.$$

Pauli and Weisskopf had the idea of changing the ψ and $\bar{\psi}$ here into dynamical variables referring to emission and absorption of particles, and using the total energy, which is the integral of this expression over three dimensional space, as the Hamiltonian, and then putting down the standard Schrödinger equation in terms of a big Ψ referring to the whole assembly of particles. With this development of the Klein-Gordon equation one has a theory referring to several particles which all have positive energy, and which have to be bosons now, not fermions as one had previously. This theory is also relativistic and in agreement with the transformation theory.

Thus there were two possible theories for particles, both relativistic, both in agreement with the requirements of the transformation theory, one of them for particles of zero spin satisfying the Bose statistics, the other for particles of spin 1/2, satisfying the Fermi statistics. These theories were in a sense equally good. The Fermi theory applies to electrons and to other particles of spin 1/2, like protons. The Klein-Gordon theory may apply to certain kinds of mesons with zero spin.

For both of these theories we have the electromagnetic potentials coming in. These electromagnetic potentials have to refer to an external field. Now, the next step which we would like to do would be to make these potentials into quantum variables satisfying suitable commutation relations, so as to refer to a quantized field of radiation interacting with the assembly of particles.

Now, when you do that, you get into trouble. You can put down a Schrödinger equation for the whole assembly, particles and electromagnetic field. When you try to solve that Schrödinger equation, you find you cannot do it. You can apply standard perturbation methods and you then run into infinities. You cannot find any solution. You cannot even get a simple solution referring to the vacuum state.

The only sensible conclusion to be drawn is that it's a bad theory. That I have insisted on all along, but most physicists are inclined to be rather satisfied with it and to work with it. There is some justification for doing so, because at the present day one doesn't have a better theory.

People have done an enormous amount of work with this quantum electrodynamics, as it is called. They have noticed that, although attempts to solve the wave equation always lead to infinites, those infinites can be managed in a certain way. In particular, it was shown by Lamb that the infinites could be removed by a process of renormalization. Renormalization means that you assume that your parameters e and m occurring in the original equations are not the same as the physically observed quantities. The general idea of renormalization is quite sensible physically, but the way it is applied here is not sensible, because the factor connecting the original parameters with the new ones is infinitely great. It is then not a mathematically sensible process at all!

But still, people have worked with it, in particular Lamb. The surprising thing is that with the infinities discarded by these artificial renormalization rules, you get results in agreement with observation. The agreement holds to a very high degree of accuracy.

Most physicists are very satisfied with that result. They say that all that a physicist needs is to have some theory giving results in agreement with observation. I say, that is not all that a physicist needs. A physicist needs that his equations should be mathematically sound, that in working with his equations he should not neglect quantities unless they are small. You certainly should not neglect quantities which are infinitely great just on the ground that you don't like to have them present.

Well, here again I find myself in disagreement with the great body of theoretical physicists. They are complacent about the difficulties of quantum electrodynamics, and I feel that kind of complacency is similar to the complacency which people at one time had with the original Klein-Gordon equation. It is a complacency which blocks further progress.

Any substantial further progress, I feel, must come from some drastic changes in the basic equations. Just where they should be I don't know, but I feel that this change will be rather similar to the change that Heisenberg introduced in 1925. It is a change which people will probably come to eventually only by an indirect route. The only feature of the new theory which one can be sure of is that it must be based on sound and beautiful mathematics.

Most of my later work has been on such lines, trying to look for mathematical ideas which may help one in getting a better quantum electrodynamics. I have had several ideas on these lines, but none of them has been very successful. One of the early ones, as you known, led to the idea of magnetic monopoles. People have searched for monopoles but haven't found them with certainty to date, but still the monopole theory is, I would say, an alive theory. Monopoles might be discovered sometime in the future.

I have found other equations rather similar to my original electron wave equation with some more complicated kind of internal freedom for the electron. These equations are beautiful mathematically, but so far they have not led to anything of physical importance. I believe that one must continue on these lines trying to guess at some suitable mathematics which will lead to a good theory of the future.

You might ask, should one not be pretty well satisfied with the present quantum electrodynamics on account of its great successes in accounting for observations? Well, I feel that these successes are essentially coincidences. There may be some reason underlying them, a reason of the nature of a good deal of similarity between various features of the new theory which is not yet discovered and the present quantum electrodynamics. Presumably there are such features of similarity which lie at the basis for the success of the explanation of the Lamb shift.

One can compare the situation with the successes of the Bohr theory. The Bohr theory did very well for cerain single electron problems, in spite of the concepts of the Bohr theory being basically wrong. It seems that one does get coincidences of this sort in the search for understanding Nature. My own belief is that the successes of the existing quantum electrodynamics in explaining the Lamb shift are coincidences of that nature. It is nothing that one should really be complacent about.

I'll conclude at this point. I really spent my life mainly trying to find better equations for quantum electrodynamics, and so far without success, but I continue to work on it. Any work that one does on these lines must be based on sound mathematics. Presumably it will involve representations of the Lorentz group. So one must study the representations of the Lorentz group, find out more about them, and hope that one will eventually think of those representations which are physically important Of course the mathematicians have worked out all the irreducible representatons of the Lorentz group, but the irreducible representations don't take one very far. Physicists are not concerned very much with irreducible representations, but with representations which are very far from being irreducible, and there is an enormous field for further investigation in searching for these general representations. Thank you.