THE IMPORTANCE OF RADIATIVE CORRECTIONS

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I did not invent the title of this talk. It was invented by the Organising Committee. I thought it was a bit funny because everyone who is here must know why they are working on radiative corrections. It seems the oddest place to have to explain why radiative corrections are important. Also most of you have probably spent more time than I have recently on the subject. So what do I do? I am sure you do not want me to talk on very technical matters.

Thinking about what I should say, I felt a tendency to be nostalgic: to dwell on the past. This of course happens when you are getting old and have spent a long time in a subject. But I think it may not be such a bad idea to try to remember what the motives of people working in the subject were in the old days. Twenty years ago my motives were certainly different than they are now; our objectives today are somewhat different. Whenever you do work of this type which is often tedious you need to have some sort of goal in mind which motivates that work. So I will try to clarify why we used to do radiative corrections and then give my opinion of why we do them now.

Well when we go back 20 years to 1969 the known particles were:

$$\begin{array}{cc} u\ u\ u & s\ s\ s \\ d\ d\ d & \\ \nu_e & \nu_\mu \\ e & \mu \end{array}$$

We did not know then that quarks came in three colours but that is the way I have written them anyway. We also had a theory then called SU(3) which united the up, down and strange quarks into one triplet; you see how much things have changed as we don't think in that way any more. · Anyway, we were on the verge of having the renormalisabilty of the standard model being proven. Then came one of the spectacular successes of the renormalisability idea: the GIM mechanism. Then charm was discovered in 1974. You have to understand that that was a spectacular success of the idea of renormalisability.

Radiative Corrections, Edited by N. Dombey and
F. Boudjema, Plenum Press, New York, 1990

When we talk about radiative corrections we have to keep two things in mind: first we can compute to a high order of accuracy and thus verify the theory that we are working with. To a large extent doing radiative corrections means verifying the renormalisability of the theory. Second, in the case of a gauge theory renormalisability means that coupling constants of different particles are the same; that you have complete multiplets, and all kinds of other relations which follow from the gauge invariance of the theory. So, for example, in the standard model not only should the W's be where you expect them to be but there should also be the one thing that we have not yet discovered which is the four-point vector boson interaction.

So when you speak about radiative corrections being a verification of the renormalisability of a theory you are losing information, for renormalisbility of a gauge theory implies much more than just the finiteness of the perturbative calculations. Now we can see that history is repeating itself for in 1969 renormalisability predicted the charmed quark, while in 1989 renormalisability predicts the top quark. Except that now no one talks of SU(5) as unifying the quarks: that is a very different point of view. Few people now actually doubt that the top quark exists. This shows the difference between now and twenty years ago because although the argument is precisely the same as it was in 1969 for the prediction of the charmed quark, most people then did not believe it.

Round about 1974 or so, the main prediction arising from gauge theories apart from the existence of charm was the existence of neutral currents in weak interactions. At that time I well remember that the theory we worked with then called the Weinberg Model--now called the Standard Model-- predicted not only neutral currents, but a neutral current with a very well-defined strength when compared to the charged current. There were many neutral current experiments of different types then under way but the experiments were difficult and sometimes there was too much neutral current and sometimes too little. So I started to think about it and thought that this was crazy: there must be sufficient flexibility in the theory to encompass different strengths of neutral current. You needed to use the Higgs mechanism in any event to generate the vector boson masses, and a sufficiently complicated symmetry breaking would generate any neutral vector boson mass you like. So it must be possible to work with the neutral vector boson mass as a free parameter. Douglas Ross and I worked on that[1] and we tried to understand the general structure of the Higgs sector which would allow this. That was the aim of the enterprise at that time: we wanted to establish that if there was a neutral current with a strength not in agreement with the standard model, then we could still have a renormalisable gauge theory.

That was much against the conventional wisdom of the time. I well remember an episode which illustrates this. There was a neutrino conference in Paris around Christmas 1975 and I had twenty minutes to talk. I decided to use one transparency, and on that transparency I had just three lines in a box. These said (I can't recall the exact words):

IN A GAUGE THEORY OF ELECTROWEAK INTERACTIONS
THE Z MASS IS A FREE PARAMETER
THEREFORE NEUTRAL CURRENTS CAN BE MADE IN ANY STRENGTH

For twenty minutes I just repeated that over and over again. There is not a single person in the world today who remembers that talk! Everyone must have been thinking about food, or sex, or just sleeping.

I am also telling this story to illustrate our motivation. Why did Douglas Ross and I do this work? Well, we were thinking about the cosmological constant and how to keep it zero. That led to the study of a general type of spontaneous symmetry breaking through the Higgs mechanism, and that led in turn to the freeing of the Z mass from its Weinberg model value and therefore to the introduction of a new parameter β which we now call the rho-parameter (following Sakurai and Hung[2] who later came to a similar conclusion from a non-gauge theory point of view). So it was then that I began to recognise that the Higgs sector is a very special part of the standard model.

Well in the 1970s charm was discovered, then the tau, and then the bottom quark. So we tried at Utrecht to establish a limit on the number of generations that we can have. Now if you look at the picture that was building up you can see that while the generations go on and the quarks get correspondingly heavier, to each generation there is a massless neutrino. Thus rather than looking for all the heavy quarks, you can look for all the light neutrinos. There was then a paper by a Russian[3] that you can count neutrinos using cosmological arguments, so at least it was realised that the number of generations was finite. Then at some point in trying to do radiative corrections in this theory which was so different to QED I discovered this extraordinary radiative correction which is sensitive to high mass particles, even if this mass is large compared to the energies you are working with. This is, of course, the radiative correction to the rho-parameter which depends on the top mass squared.

This is where we are now and it is this result in the minimal standard model which gives us the upper bound on the top mass. So let us go back to the original question of why we need radiative corrections. Of course we need to check the theory, and of course we need to test that the theory is renormalisable, but as I have already said renormalisability predicts a lot of other things and you check it in many more ways than by just calculating radiative corrections. Now we also have to keep in mind that not all radiative corrections are equally important and that usually they are not sensitive to a high energy scale. For example, the radiative corrections to g − 2 for an electron or muon, even if calculated to many places of decimals, are not very sensitive to strong interactions. In practice, nothing with a mass of over 200 MeV or so will affect g − 2. So g − 2 is a poor indicator of high energy processes and we will not find out anything about the mass of vector bosons or the top quark from it.

Much better, and much better than many people think, is the radiative correction to β-decay which has been calculated by Professor Blin-Stoyle[4] and Professor Sirlin[5], both of whom are here. That calculation and its verification are much more of a test of the standard model than is commonly understood. It extends the energy scale in which we have confidence in the standard model to about 60 GeV. These calculations also show that the vector bosons are not composite: I do not have time to elaborate on this but if the W were composite, there would be an additional diagram to calculate in the radiative correction to beta-decay which would spoil the present excellent agreement between theory and experiment.

Now more information is available to us from the calculations of the radiative corrections δM to the vector boson mass, but as has been emphasised by many authors those results follow predominantly from the running of the fine structure constant from QED between 0 and 80 GeV. It does not result from any special details of the standard model. On the other hand the radiative corrections to the rho-parameter are sensitive to such details. The sensitivity is there because if you start with a gauge model there will always be cancellations between different terms of the theory. For example if the top does not exist the correction diverges: moreover it is a quadratic divergence and therefore the radiative correction depends on the square of the top mass. Similarly, if there is no Higgs in the theory it blows up. But now this is only a logarithmic divergence and hence the Higgs mass enters only as a logarithm. That is why it is so difficult to obtain information on the Higgs: we have a limit on the top mass but no limit on the Higgs mass.

The rho-parameter is important but can only give you one number. You require further information from, for example, e^+e^- to muon pairs. That will of course be studied at LEP as time goes on. At LEP2 one of the first results which will come out in full clarity will be the mass of the W. This is the region where you will find the strongest influence of the Higgs. But unless we are able to go up to an energy of 250 or 300 GeV as I have always advocated very strongly, we will only be able to examine the small range shown. Maybe LEP2 sometime will be considered to be an enterprise as mistaken as NINA or Frascati. Only the future will say.

What are the things that we do not know at the present time? They are the existence of the top; the existence of other neutrino families, and the possibility of other heavy multiplets, not just the ones we are used to. And then there is the Higgs. Now in my view there are only two things to learn about the top quark and that is the date when it will be discovered and its mass. The number of neutrino families will be determined very soon from the total width of the Z. I have no idea how you find out about other multiplets.

So if you take this view, the future seems meagre and bleak. But I don't think that is right. You have to have some vision of what you are after, otherwise you die of boredom. My vision of the situation is this: we are looking at the standard model which has many characteristics, but we do not understand any of it. No one has explained to us why there are only three generations or why mixing is important or why we have $SU(2)_L$ X $U(1)$. We have absolutely no clue. Then there is another fact which has not been advertised very much: you can introduce any mass simply by hand, or you can get a mass by spontaneous symmetry breakdown. Now in the standard model every particle gets a mass through the Higgs mechanism. Why is this? For some reason or other, Nature has constructed the world in a way where all masses come via the Higgs mechanism. So the Higgs mechanism is a shorthand for how particles get their mass. To me it is a door in the wall: when we learn how to open it we will discover how particles acquire mass. I do not think that the way we describe the Higgs mechanism today is right. We are missing something. There is a threshold energy where the theory changes and we will go through the door and enter another world.

Where will it be? At the moment we are seeing a mass spectrum of particles which stops somewhere around 100 GeV. So it seems that there is a dividing line somewhere like 200 or 300 GeV with the property that all particle masses are below that line. It will be the limiting mass scale in our world.

We must take this into account when we consider radiative corrections. This is a necessary constraint on their size. The top quark seems to have a mass no smaller than 100 GeV, but from radiative corrections we have an upper limit which is something like 190 GeV. So maybe the mass is 150 GeV.

There is an amusing question which I ask myself from time to time: what if the top quark was not found below 200 GeV. This is called thinking the unthinkable. There may, of course, be other contributions to the rho-parameter. Just almost all contributions that you can think of: scalars, fermions, a large class of multiplets, all give a positive contribution[6] to the rho-parameter. So the limit of 190 GeV is not something that can be easily disposed of. Hence if the top is not found at the limit given by radiative corrections you have a serious conflict. There seem to me to be just two ways out of this. The trivial way out is that the Higgs sector is not what you think it is and there is an admixture of another Higgs sector. Then the rho-parameter simply becomes an arbitrary number as the ratio of vector boson masses becomes a free parameter and there is nothing to explain. That is the end of the story. You will just have to explain why the ratio of vector boson masses is so close to the value given by the standard model.

There is another way out: perhaps the way we do our calculations is not correct. Politzer and Wolfram and Hung some time ago[7] demonstrated that if the Higgs mass is heavier than 100 GeV and the top mass heavier than 100 GeV then the theory possesses certain non-perturbative effects. You look at the Higgs sector at the minimum of the potential where you obtain the vacuum but you get radiative corrections to Higgs-Higgs scattering via a fermion loop. That is something you can treat according to the ideas of Coleman and Weinberg[8]. You can ask what contribution this scattering makes to the Higgs potential. If the Higgs is in some mass range and if the top is sufficiently heavy then the minimum of the Higgs potential simply disappears. So you no longer have spontaneous symmetry breaking.

This paper has not been taken sufficiently seriously in the past because nobody believed that the top mass could be so heavy. Everyone expected the top to be just around the corner. But these ideas become important now. I would expect that if something like this is true, then it must be associated with some new and interesting physics. So now we have something to look forward to in the development of LEP.

I want to discuss another matter now and in doing so to take issue with Alberto Sirlin. That is this subject of renormalisation schemes which is advertised to be a hot topic at this conference: all kinds of people are set to talk about these schemes. But this is a subject which always makes me mad. So I will try to explain why I feel about it as I do so that maybe you will get mad too. This is the situation: the standard model Lagrangian has a number of parameters in it; you fit the data including radiative corrections to this Lagrangian and you get the value of all the parameters which appear in the Lagrangian: g, sin θ_W, M_Z, M_W, etc. As far as I am concerned that is the whole affair.

The only thing more that I can think of in this connection is to relate this to how renormalisation was carried out in the old days. Then there really was some physics connected with the idea of the subtraction of infinities. The idea was that every infinity was literally related to some physical quantity. There was a one-to-one correspondence. There was a bare electron mass and then came the infinity and this resulted in the dressed mass that we could observe. That defined the physical electron mass. There was real physics behind this interpretation: there was the bare particle surrounded by a cloud of pairs from radiative corrections. So that attitude led to a correspondence between infinities and measurable physical parameters like the electron mass or the coupling constant in QED which was unrenormalised at zero momentum transfer so here nature was even kinder.

But now we know this is too narrow a view. In a non-Abelian gauge theory many relations hold in the Lagrangian but to which radiative corrections must be computed before the theory is checked against experimental data. The most notorious example is sin θ_W. This occurs all over the place in the Lagrangian: then you can do experiments, for example a neutral current experiment, and you find that it occurs in the ratio of the axial to vector current strengths g_A/g_V; but of course it also comes in through the ratio of vector boson masses: $\cos\theta_W = M_W/M_Z$ in the minimal standard model. So you can define sin θ_W in either way: from the ratio of neutrino-electron to antineutrino-electron scattering where the vector boson mass cancels out, or from measurements of the vector boson masses. This is clearly a domain of confusion. At least the ratio of scattering cross sections is not sensitive to the Higgs sector. So which is the real sin θ_W?

I can play the same game with the coupling constant which also appears in many places in the Lagrangian. It appears in the coupling of a W to an electron-neutrino pair (for example in muon-decay). It also appears in the trilinear interaction of vector bosons and the interaction of the Higgs with the vector bosons. Now all these terms give rise to different radiative corrections. So you may say that there are three separate coupling constants, but the requirements of the gauge theory are that they should be equal when you remove the radiative corrections. So you can define the ratios of these coupling constants as different types of rho-parameter and study those radiative corrections. So I can confuse you as much as you like. But you see that we have lost any semblance of being able to say that a given infinity is related directly to a given physical quantity. This is now seen to be too narrow minded a view.

Also we do not have to say "this is the coupling constant" of the theory, or "this is the sin θ_W." That is the quarrel I have with Sirlin. He makes a big point of having a renormalisation scheme in which sin θ_W is defined as the ratio of vector boson masses. If he takes that to be the definition it is fine by me. I don't need a definition in the first place as I hope I have shown. The second reason I have against this particular definition is that he has precisely obscured the one place where I hope to get information about the theory, because the ratio of vector boson masses is the parameter which tells us about the Higgs sector.

Of course, if the rho-parameter is defined to be one as it is in the Sirlin definition, the problem arises somewhere else. Furthermore, the definition of sin θ_W in terms of the vector boson masses assumes that these masses are precisely defined. Yet these particles are unstable and so do not have a uniquely-defined mass. I am not saying that you cannot work with unstable particles but it is a matter of principle. The width of the Z is about 3% of its mass. We are now entering a new era; the era of precision measurements, 0.1% measurements. I therefore ask you how you measure the weak mixing angle to 0.1% from a measurement of the W and Z masses, neither of which are well-defined to the level of 1%. You can of course locate a peak to a precision of 0.1% but there are many ways of parametrising it and the mass will depend on the parametrisation.

We don't need to do any of this. When we start computing our radiative corrections we compute to the greatest precision in the region of the whole peak: then you can fit to the parameters of the Lagrangian. But maybe I have misunderstood the problem. That is always possible. So to conclude this section I would advocate the simple procedure of expressing everything in terms of the "bare" parameters in a specific gauge (the 't Hooft-Feynman gauge). Then throw away 1/(n - 4) (MS-scheme) or 1/(n - 4) + ln c ($\overline{\text{MS}}$- scheme).

The important issue is how to confront theory and experiment. The precision measurements that will be available in the near future are the position of the Z peak and the asymmetry measurement using polarised beams. As long as the top mass is unknown it may be used as a free parameter which may be predicted as a result of these measurements.

Finally, I would like to ask a different question: how sure can we be that the Higgs system of the real world is the simplest one assumed in the standard model. This I want to discuss from a somewhat different point of view. Traditionally, for example see the account by our host Professor Dombey[8], fermion-W coupling is described by a left-handed doublet L and a right-handed singlet R with

$$L = i \bar{L} \gamma^{\mu} \left[\frac{1}{2} g \underline{\tau} \cdot \underline{W}_{\mu} - \frac{1}{2} g' B_{\mu}\right] L - i r \bar{R} \gamma^{\mu} R B$$

showing the SU(2) × U(1) structure of the model where g' and r are free parameters. Now introduce mixing

$$Z = W^3 \cos \theta_W - B \sin \theta_W$$

$$A = W^3 \sin \theta_W + B \cos \theta_W$$

(for the e, ν_e case)

(i) the neutrino has no charge, so

$$g^1 = g \tan \theta_W$$

(ii) electromagnetism preserves parity, so

$$r = g^1$$

Thus conservation of parity is an accident.

Let us now assume that

(a) all masses derive through the Higgs mechanism,

and

(b) the Higgs mechanism comes about through the linear σ-model:

$$L_H = -\frac{1}{2}(\partial_\mu \varphi_\alpha)^2 - \frac{1}{2}\mu^2 \varphi_\alpha^{\ 2} - \frac{1}{8}\lambda \varphi_\alpha^{\ 2}$$

$$\alpha = 0,1,2,3, \text{ where } \sigma = \varphi_0$$

This is obviously invariant under 0(4) which is a six parameter group.

Rewrite L_H :

$$L_H = -\frac{1}{4} T_r \left[\partial_\mu \Phi^+ \partial_\mu \Phi\right] - \frac{1}{2}\mu \, \mathrm{Tr}\left[\frac{1}{2}\Phi^+\Phi\right] - \frac{1}{8}\lambda \, \mathrm{Tr}\left[\frac{1}{2}\Phi^+\Phi\right]^2$$

$$\Phi = \sigma \tau^0 + i \varphi_\alpha \tau^\alpha$$

$$= \begin{bmatrix} \sigma + i\varphi_3 & i\varphi_1 \ \varphi_2 \\ i\varphi_1 - \varphi_2 & \sigma - i\varphi_3 \end{bmatrix}$$

Tr $\left[\Phi^+\Phi\right]$ is easily shown to be invariant under

$$\Phi \rightarrow G \, \Phi \, H^+$$

where G and H are $SU(2)_L$ and $SU(2)_R$. This is also six-parameter, and is of course equivalent to the O(4) description.

We can gauge $SU(2)_L$ with three parameters, while rotations about the 3-axis of $SU(2)_R$ gives one additional parameter.

If $SU(2)_R$ is not gauged we would have a strict global $SU(2)_L \otimes SU(2)_R$ symmetry. This is ordinary isospin. The vector bosons are an isospin triplet. It follows that

$$M(W^+) = M(W^-) = M(W^0)$$

Mixing modifies this to

(A) $$\rho = M_W^{\ 2} \,/\, (M_Z^{\ 2} \cos^2\theta_W) = 1$$

Now fermion masses. They arise from a term of the form

$$\bar{\psi}_L \psi_R \Phi + \text{h.c.}$$

Since Φ transforms as a doublet under $SU(2)_L$ it follows that ψ_L cannot be in the same representation of $SU(2)_L$.

(B) Parity is broken in weak interactions:

The relevant two coupling constants are g from $SU(2)_L$, and g' from $SU(2)_R$ restricted to rotations about the 3-axis. Define θ_W by

$$g' = g \tan \theta_W$$

Take

$$\langle\Phi\rangle_0 = \begin{bmatrix} f_0 & 0 \\ 0 & f_0 \end{bmatrix}$$

$$L_H = -\frac{1}{4} f_0^2 g^2 W_\mu^+ W_\mu^- - \frac{1}{8} f_0^2 g^2 \left[W_\mu^3 \cos \theta_W - B_\mu^0 \sin \theta_W\right]^2 / \cos^2 \theta_W$$

The quantity in the brackets above is just

$$Z_\mu = W_\mu^3 \cos \theta_W - B_\mu \sin \theta_W$$

and clearly parity is not conserved in charged weak interactions, nor in neutral weak interactions for general θ_W. Furthermore the Z has a mass.

(C) There is one zero mass vector boson:

The Higgs system has only 4 degrees of freedom of which at least one Higgs particle must remain physical. So at most three vector bosons can acquire a mass.
So

$$A_\mu = W_\mu^3 \sin \theta_W + B_\mu \cos \theta_W$$

which is orthogonal to Z must remain massless.

It is now easy to check that the photon A_μ couples identically to left-handed and right-handed fermions, so that

(D) Parity is conserved in electromagnetic interactions.

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