Correcting LEP: where we stand

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1 Introduction

In this talk I shall try to review the activities of the study group on Monte Carlo software which has been organized at CERN in the context of the current workshop on Physics at LEP and of which I am the convenor. The workshop has been devided into three sections: one to deal with standard-model physcis issues (convened by G. Altarelli), one to study possible signals for new physics (convened by C. Verzegnassi), and one which concentrates on the implementation of all these results in terms of software and Monte Carlo event generators. At this moment I shall only discuss what the study group has done so far in the field of Monte Carlo programs for electroweak physics – no programs for hadronization and jet fragmentation.

This talk is divided in several parts. First I shall discuss the accuracy that we are aiming for, and for which processes this accuracy is relevant. Then I review briefly the status of our knowledge on the various ingredients that go into a Monte Carlo program. I will then describe a number of programs for μ pair production and their agreement. Finally I shall formulate my own personal view on where we stand and what has to be done. As a word of warning, it should be remarked that this is ongoing research – we learn better which questions to ask, and the software is continuously updated. Consequently the results we have so far will probably already be a bit obsolete by the time these proceedings are published, and the general remarks I am making are to be considered more important than any numerical results I could show at this moment. In the following, a name like ZBATCH always refers to the name of an available program. Finally, I have decided not to quote any references at this place in order to avoid putting unreasonable emphasis on some results at the expense of other equally relevant ones – this is just a brief remark on research that has just begun, and we are still in the process of collecting the necessary references which will appear in our contribution to the CERN Yellow Report on the workshop.

2 Accuracy and processes

LEP is supposed to be (amongst other things) a machine to do precision physics. This precision must obvioulsy be reflected in the software used to make predictions. The most accurately known number will probably be the Z^0 mass M_Z. Taking that value to be 92 GeV, and the obtainable error δ M_Z about 50 MeV, the relative accuracy will be about 0.05%. On the other hand, from the forward-bakward asymmetry A_{fb} we shall probably find $\sin^2\theta_W$ about 0.23 with an error $\delta\sin^2\theta_W$ of about 0.0012, implying a relative accuracy of about 0.5%. We conclude that the error on the theoretical prediction should typically be smaller than 0.5% but not necessarily as small as 0.05% (since the value of M_Z is a fundamental input parameter anyway). The accepted value of the error one should aim for is generally thought to be 0.1%: in that case the theoretical predictions will have about one more digit of accuracy than the measurement, assuming the errors add in quadrature.

Obviously, an accuracy of 0.1% is not required for all possible predictions for all possible processes. We can divide the physics processes to be expected at LEP in three categories, depending on the accurcay with which they are measured:

- **High accuracy**: processes which will be used to measure the fundamental parameters of the Standard Model to the desired one-loop precision. These are the production of μ pairs, light or b quark pairs, τ pairs, Bhabha scattering. Also the production of a Higgs boson with a virtual Z^0, $Hf\bar{f}$, and radiative ν pair production, $\nu\bar{\nu}\gamma$, are in this category, for which the desired accuracy will be 0.1-0.3%.

- **Medium accuracy**: processes that will not be used for precision test themselves, but may figure as backgrounds. This group comprises the production of two or more real photons, and the whole host of two-photon processes. An accuracy of 1-3% is considered adequate here.

- **Low accuracy**: in this category fall all new-physics phenomena. In this area the predictions are so speculative, and so many different scenarios may be realized (Z' or other new gauge bosons, new fermions or Higgses, SUSY or compositeness...) that it is pretty useless to aim for an accuracy of more than 10-30%.

For the moment the study group is concentrating on the first category, and in particular the process $e^+e^- \rightarrow \mu^+\mu^-$. The idea is that this is the archetypical Z^0-mediated process. Adding Coulomb scattering to it gives Bhabha scattering, and changing the final-state couplings would give quark pair production, and so on. In other words, this process is a benchmark: if we cannot understand it to the desired 0.1%, we have to give up on all the other processes as well.

3 Ingredients of a Monte Carlo

Before we start looking at general aspects of radiative correction Monte Carlos, it should be remarked that of course Monte Carlo prorgams are not the only valid description of the physics going on at LEP. Historically, analytical results (for instance, expressions for the total cross section) have played a very important rôle, and they will continue to do so. However, nowadays even the analytical results are given in the form of a computer algorithm (maybe including one or two numerical intergations) since the expressions themselves are *very* complicated. Moreover, these (semi)analytical results are only feasible for very inclusive quantities like total cross sections and total asymmetries: imposing any cuts beyond the most trivial ones is practically impossible. The general idea nowadays is that analytical results are very valuable *to check the Monte Carlo predictions*: after this 'calibration' the experimentally relevant results are then obtained from the Monte Carlo itself.

I shall now discuss some aspects of radiative corrections to μ pair production at LEP. A good Monte Carlo or (semi)analytical calculation encompasses two precision effects: on the one hand, the one-loop predictions of the Standard Model, including all WW boxes and what not; on the other hand, QED effects and the emission of (possibly many) bremsstrahlung photons. As to the weak corrections, a lot of activity is going on and at this moment more or less complete agreement has been achieved between the various groups working in this field, as far as the one-loop level is concerned. Some uncertainty remains in the effects of a heavy t quark in the production of $b\bar{b}$ pairs, but it is generally agreed that this will disappear with more work being done. The above statement does not mean that all groups get identical results for, say, the total cross section σ including weak corrections, but rather that where there are discrepancies they are well understood and people *can* get identical numbers out by for instance agreeing on the same value for the hadronic vacuum polarization. Also, the numerically most important higher-order weak effects are under similar control: there is broad agreement on the effect of the Dyson-summable higher order terms, again with the possible exception of the case of a heavy (more than,say, 200 GeV) t quark.

It appears that in fact the total cross section including all numerically relevant weak effects (but without QED) can be written by adopting a simple modifcation of the Born formula: if one neglects the photon-exchange diagrams, a formula like

$$\sigma(s) \propto \frac{G_F^2 M_Z^4 s}{(s - M_Z^2)^2 + s^2\Gamma_Z^2/M_Z^2} \tag{1}$$

turns out to describe the total cross section to about the desired accuracy of a few thenths of a percent. For more information on this I refer to the beautiful and useful work that is being done by the study group on the Z^0 line shape in our workshop, convened by F. Berends. In fact, whether a formula like the above is able also to give the differential cross section $d\sigma/d\Omega$ to this precision has not yet been determined, but it seems to be more or less possible.

This bring us to the question of input parameters. From the point of view of our study group this is very much down-to-earth: which are the numbers that one has to specify before a given Monte Carlo can be run? One can distinguish two broad choices for this:

- **Theorist's ideal**: the input can be the parameters of the SM Lagrangian, that is, M_Z, M_W, M_H, M_t, $\alpha(0)$, $\alpha_s(M_Z^2)$, and so on. Given these, the Monte Carlo computes everything (including Γ_Z) from first principles. The formula for $\sigma(s)$ will then of course be much more complicated than the modified Born formula given above, but also more accurate. However, one is *always* confined to precisely the Standard Model predictions – nothing more, nothing less. Monte Carlo programs using this approach may be described as **stand-alone programs**. Examples are ZBATCH (G.Burgers and W.Hollik) and EXPOSTAR (B.Lynn, D.Kennedy, R.Stuart and K. Im) which compute only the total cross section and hance are of the (semi)analytic type, and KORALZ (S.Jadach, B.Ward, R. Stuart, Z.Wąs) and MUONMC and BABAMC (F.Berends, W.Hollik, R.Kleiss) which are full-fledged Monte Carlo event generators.

- **Experimentalist's ideal**: one can resign onself to using the modified Born formula, in which case for instance M_H and M_t disappear as parameters, but Γ_Z and, say, all kinds of branching ratios come in. It is clear that this approach is much closer to experiment and more suited for fitting the line shape etctera, at a cost in precision which may or may not be bearable. At any rate the idea of having to fit the line shape to a given mass of the top quark does not seem very attractive anyway. More importantly, this approach allows one to examine deviations from the Standard Model (for instance, an anomalously *small* width of the Z^0, which simply cannot be accomodated in the Standard Model). Monte Carlo programs working along these lines concentrate themselves on taking the modified Born result and applying to this the QED corrections, and can hence be called **QED dressers**. Obviously, they tend to be faster than stand-alone programs, at the cost of a built-in limit on accuracy. The are not stand-alone in the sense that to simulate the Standard Model prediction, they have to get accurate numbers for Γ_Z and so on from somewhere else – the initialization of a stand-alone program, for instance. Examples of this type are DYMU2 (J.-E. Campagne and R.Zitoun), MOE (G.Bonvicini and L.Trentadue) and MMGE92 by G.Alexander.

As we see, both stand-alone Monte Carlos and QED dressers exist at this moment. This we consider very useful since it allows us to gauge the respective merits of each approach.

4 Aspects of QED corrections

I now turn to the problem of including the QED effects in the prediction. Needless to say, this is the point where the importance of having full-fledged event generators (as opposed to more or less analytical results) comes in: so far we have only dealt with the 'elastic', two-to-two process in which the only dynamical variable is the polar angle of the two back-to-back muons. Once we include bremsstrahlung the full problems of multiparticle phase space and detector acceptances arise.

Let us consider some orders of magnitude for the moment. The famous ***initial-state radiation*** is known to give first-order corrections of about -40%, to which the higher orders add about 10% (all at the Z resonance). Final state radiation amounts to the well-known number $3\alpha/4\pi$ (α_s/π in QCD), but only if no cuts are applied: under even reasonable cuts on, say, the $\mu\mu$ acollinearity the correction from final-state radiation can be also about -10%. The interference between initial- and final-state radiation can be shown to be a truly small effect, but only under quite loose cuts, and at the resonance: under other circumstances, its effect may amount to a few % and we conclude that ***to first order*** we cannot neglect any of these effects if we want to have a general-purpose Monte Carlo program which allows in principle all kinds of cuts. Higher order corrections from at least the initial-state radiation are also necessary.

A second observation is typical for Monte Carlo event generators as opposed to analytical calculations: it is the so-called **positivity problem**, in the jargon also called the k_0-problem. I shall try to indicate what it is, and then how people try to get around it. Let us first restrict ourselves to a fixed $\mathcal{O}(\alpha)$ computation of initial-state radiation. We denote the Born cross section (either total or differential) by σ_0. The QED corrections can be divided into three types: those due to virtual photons; those due to soft photons, for which the bremsstrahlung amplitude factorizes into the Born amplitude and a so-called *infrared factor*; and hard bremsstrahlung for which the factiorization no longer holds. The soft and hard bremsstrahlung regions are separated by a threshold in the photon energy $\Delta E = k_0 E$, where E is the beam energy. At PETRA/PEP energies, k_0 can typically be taken to be 0.01. The three cross sections corresponding to these types are:

$$\begin{aligned}
\sigma^{\text{virtual}} &\sim \sigma_0(1+\beta\log(m_\gamma/E)+\cdots) \ , \\
\sigma^{\text{soft}} &\sim \sigma_0\beta\log(\Delta E/m_\gamma)+\cdots \ , \\
\sigma^{\text{hard}} &\sim \sigma_0\beta\log(E/\Delta E)+\cdots \ ,
\end{aligned} \tag{2}$$

where m_γ is a small regulatory photon mass, and $\beta \sim (2\alpha/\pi)(\log(s/m_e^2)-1)$ which is about 12% at the Z. The above is only symbolic – I have left out a lot of nonleading terms and so on. Now in a Monte Carlo event generator we have to specify for each event whether it will or will not contain a visible bremsstrahlung photon. This means we have to add the virtual and soft cross sections, but keep these separate from the hard photon contribution:

$$\begin{aligned}
\sigma^{0\gamma} &\sim \sigma_0(1+\beta\log(k_0)+\cdots) \ , \\
\sigma^{1\gamma} &\sim \sigma_0\beta\log(1/k_0)+\cdots \ .
\end{aligned} \tag{3}$$

As long as ΔE is smaller than the experimentally available resolution this is OK and the total cross section does not depend on k_0, which after all is a purely artificial parameter. However, we have a lower limit on k_0 from the requirement that the probabilities for having either zero or one observeable photon should both be positive – else no statistical interpretation of the cross sections, and hence no Monte Carlo treatment, is possible. Performing the calculation a bit more carefully and including final-state radiation and the interference, we find that for scattering angles of respectively 5, 90 and 175 degrees the lower limit on k_0 is about 0.0001, 0.002, and 0.01. This implies that to have a muon-pair Monte Carlo program applicable in the whole detected region, k_0 has to be taken not much smaller than 1% of the beam energy, i.e. photons with energy smaller than 500 MeV are neglected. This is clearly unacceptable for precision physics: such photons can themselves be seen explicitly by several LEP detectors, and even if they are not seen themselves they can generate a muon acollinearity angle of up to 0.5 degrees – easily measured! I would like to stress that this problem is independent of the question of whether the first-order correction itself is so large that we have to worry about higher orders. Indeed, whereas an analytical prediction (where we do not worry about adding positive or negative numbers since they are not interpreted as probabilities) can be improved by going to some higher order, the k_0-problem remains also in higher order: in second order we have

$$\begin{aligned}
\sigma^{0\gamma} &\sim \sigma_0(1+\beta\log(k_0)+\beta^2\log(k_0)^2/2+\cdots), \\
\sigma^{1\gamma} &\sim \sigma_0(1+\beta\log(k_0)\beta\log(1/k_0)+\cdots\ , \\
\sigma^{2\gamma} &\sim \sigma_0\beta^2\log(1/k_0)^2/2+\cdots\ , \qquad (4)
\end{aligned}$$

where we see that the cross section for 1 observable photon will again become negative, and for ***the same*** k_0 value as before! The conclusion (reached independently by a number of people in the last few years) is that ***in a Monte Carlo event geneator*** the only way to get the event topologies simulated correctly to less than 1% is to include bremsstrahlung (at least from the initial state) **to all orders**. The way this is currently done is by introducing the so-called ***exponentiation*** procedure. In the formula for $\sigma^{0\gamma}$ evaluated to first and second order one can recognize the expansion of an exponential. This can in fact be proven to hold to all orders and consequently we may write

$$\begin{aligned}
\sigma^{0\gamma} &\sim \sigma_0 e^{\beta\log(k_0)}(1+\cdots)+\cdots \\
&\sim \sigma_0 k_0^{\beta}(1+\cdots)+\cdots \qquad (5)
\end{aligned}$$

and, for instance, by differentiating this with respect to ΔE one can obtain the exponentiated bremsstrahlung spectrum

$$\frac{d\sigma}{dE_\gamma} \sim \sigma_0\beta E_\gamma^{\beta-1} \qquad (6)$$

It has to be remembered, however, that this procedure is only rigorous for really infinitesimally soft photons (no one so far really knows how to exponentiate correctly in the presence of hard bremsstrahlung), and that (as the $\cdots$ indicate) the procedure in not completely unambiguous even then: about ten different possibilities can be encountered in the literature, with results for e.g. the Z line shape that fortunately are in very good agreement (less than about 0.2%). Another complication is that the exponentiation is really understood only for inclusive quantities like the integrated bremsstrahlung spectrum: to get back to the spectrum itself by differentiating is not completely rigorous [1]. Also, the kinematics of an unlimited number of soft photons is somewhat complicated and it has become practice to neglect the ***kinematical*** effects of all photons softer than, say, 10^{-5} or so of E: this is again a remnant of the k_0 but now much more harmless. So far, exponentiation for initial- and final state radiation is quite well understood: their interference is still somewhat uncertain, but the general expectation is that this is just a matter of time.

Among the various Monte Carlo programs, one can discern four strategies to implement exponentiation:

1. The rigorous approach, based on the classic paper of Yennie, Frautschi, and Suura. This is indeed rigorous in the limit $E_\gamma \to 0$, and has been implemented in the beautiful algorithm **YFS2** by S.Jadach and B.Ward, implemented in Monte Carlo prgrams for both $\mu\mu$ (**KORALZ**) and Bhabha scattering (**BHLUMI**). Programs like these can in principle generate events with any number of soft photons, just by lowering the limit on the energy below which a photon's kinematics will still be taken into account.

2. More simple-mindedly, one may decide to take an old-fashioned, first order Monte Carlo program and modify its photon spectrum, so as to take the main effects of exponentiation on the total cross section and such into account, without bothering about the tiny details of the event topologies. This is the ad-hoc approach, leading to fast programs which however may have a more limited accuracy, since they will always generate at most one photon. This procedure is followed in **MMGE92** and **HOWLEEG** (for Bhabha scattering, by W.de Boer, based on **OLDBAB** by R.Kleiss and F.Berends)

3. Another more or less rigorous approach is based on the use of QED structure functions. This has been pioneered by a great number of (mainly Russian) authors: Gribov, Lipatov, Kuraev, Fadin, Khoze ..., and implemented into the Monte Carlo program **MOE** by Treantaduc,

[1] As an illustration of this difficulty, note that if we first integrate over the μ scattering angle and then exponentiate, we get a different result from the one following from first exponentiating and then integrating over the μ angle: which one is correct?

Nicrosini and Bonvicini. Here, a kernel similar to that in the Altarelli-Parisi splitting functions is iterated a number of times, corresponding to repeated emission of bremsstrahlung photons, at each step lowering the remaining energy of the beams to take momentum conservation into account. Programs like these can in principle also generate any number of photons. Unfortunately, at this moment we have not had opportunity to study MOE into detail. Also the program EXPOSTAR uses these structure functions, however neglecting the bremsstrahlung transverse momentum. Consequently I consider it more or less useless as an event generator, although its result for the total cross section is fine and as a (semi)analytical program it seems to be OK.

4. A variant of the structure-function method has been developed by Campagne and Zitoun in their DYMU2. It consists of modifying the QED structure function with higher-order terms, constructed in such a way that precisely *two* iterations (one corresponding to 'radiation from the electron', and one to 'radiation from the positron') will lead to a total cross section precisely equal to the full exponentiated expression. In a matter of speaking, this is something like a second-order Monte Carlo with an ad-hoc exponentiation of the two photon spectra. This program, which also emits one photon from the final state (also with exponentiation), therefore always gives you three photons in the final state. Here we expect to have a much better simulation of the event topology than in the ad-hoc approach, but maybe not as good as in the more rigorous approaches.

How do all these programs compare in practice? To study this question is not at all trivial: one can in principle imagine any kind of experimental cut, and would have to make an enormous number of runs of all the Monte Carlo programs to test this in detail. We have been using a somewhat simpler approach: first we haver tried to give *the same physics input*, like the value of M_Z, to all the programs. This itself is not straightforward, since for instance the value of α_s (occurring even in purely leptonic processes, in the value for the total Z width Γ_Z which contains also hadronic decay modes) has to be located and fixed in all the programs. A check is provided by the fact that then all programs should yield the same value for Γ_Z and so on. Then, we have looked at the value for the total cross section for

$e^+e^- \to \mu^+\mu^-$: this most basic quantity should also come out the same, and moreover programs that do not agree on this can be expected to have all other quantities and distributions different as well. Here the usefulness of semianalytic results comes in: the program ZBATCH, which is generally considered to be very reliable, can be used as a calibration to test the resluts of the other programs, and is itself free of the various bugs and difficulties that may be present in the algorithms to generate particle momenta – it is also free of the positivity problem. A cut on the $\mu\mu$ invariant mass can be put in: we have taken $M_{\mu\mu}$ larger than $0.2\sqrt{s}$, in order to avoid possible problems with extremely low-mass pairs. This cut is not supposed to be realistic in an experimental sense (in fact, it isn't) but is adequate if one is just interested in the agreement between various programs. Tentative results so far are:

- The two semianalytic results from **ZBATCH** and **EXPOSTAR** are in good agreement, after some trivial debugging etcetera. Their result stands as 'the best knowledge on the line shape' to date.

- Event generators that have no higher-order QED corrections, like **MUONMC** and **BREM5** (which I have ot discussed so far, by R.Stuart, B.Lynn and R.Kleiss) are in wild disagreement with the correct answer. We knew that already, of course, since the higher-order initial-state radiation accouns for a full 10% or so of the cross section: programs like these should *not* be used at the Z resonance!

- Ad-hoc exponentiation of the cross section as in **MMGE92** is in better but not satisfactory agreement (a few percent difference). This may partly be due to problems with overall normalization which has to be taken differenty in the Z exchange and photon exchange channels: except for a rough estimate, one should try not to use this.

- The stand-alone Monte Carlo **KORALZ** seems to be doing nicely. However it is a bit slow and consequently the statistical error on the result is quite large – we intend to put it on the CERN CRAY to improve this.

- a QED dresser like **DYMU2** seems to have about as good agreement as **KORALZ**. This is no surprise since it was constructed to have the total cross section come out correctly -- other variables may display deviations.

As I said, so far we have no result from **MOE**: that result should be very interesting since it does its exponentiation in a completely different way: agreement between, say, **MOE** and **KORALZ** would indicate that we really know what we are doing. A word of caution: we have have so far only looked in the region from 88 GeV up to 96 GeV. At higher energies things become again more tricky due to the unusual photon spectrum (no longer going like $1/E_\gamma$ but containing another peak due to the tail effect of the resonance). Also, under even a simple cut like that on the $\mu\mu$ mass all agreements become slightly worse.

A similar study for the total forward-backward asymmetry A_{FB} reveals even larger discrepancies. Here we are hampered by the fact that so far no reliable (and checked!) analytic result is available: work is in progress on this.

5 Concluding remarks

I shall finish with some observations that are purely mmy personal view of where we are and where I think we are going. On the whole, the situation for μ pairs is not too bad: the programs for which agreement can in principle be expected (e.g. *not* **MUONMC**), can in fact be made to agree to some extent. On the other hand, the situation is not too good: the disagreements are still of the order of percents, that is a full order of magnitude more than our goal of 0.1%. For less inclusive quantities, like the forward-backward asymmetry, the agreement is appalling: if we do not improve it, the experimental data from LEP will be much more accurate than out best guess for the standard model prediction. Fortunately, much can still be done and I expect improvement on the situation as we come to understand the programs better, and more semianalytic results become available. With cautious optimism I think that also for light quarks we shall reach a more or less satisfactory situation. For τ pairs the situation is a bit different: as far as their production is concerned, the same remarks as for μ pairs hold: but, at this moment only the program **KORALZ** handles τ decay adequately. Personally I feel that the authors have done a very good job in that respect, but additional confirmation would be very welcome. For Bhabha scattering, the situation is much worse! Only four Monte Carlo programs seem to exist: **BABAMC** contains the full electroweak corrections, but only to one-loop order. **BHLUMI** and **HOWLEEG** both contain an exponentiation procedure, but again they are only good at very small scattering angle since they do not have an adequate treatment of the Z resonance, only of the QED Coulolmb part. Finally, the program **TEEGG**, written by D.Karlen, is good to second order in QED, but since the scattering angle of the fermions is not restricted in this program, practically all generated events will come out with essentially zero scattering angle: a fine thing for background studies in ν counting, for which it was written, but not for comparison with other programs! Needless to say that a good deal of work is *urgently* needed here.

Summarizing I would like to say that by now 1) we know what we want, and can expect from Monte Carlo software; 2) we are slowly getting there, as far as muon pairs etcetera are concerned; 3) for Bhabha scattering we see a clear lack of good software apparatus. Let us hope that the LEP machine does not beat us by giving the data more accurately than we can predict them!