All that theories can tell us is how the world could be

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Report from Zeuthen meeting:

$\cdots$ **little to report** and transparencies not available

- $\alpha^{(5)}_{\mathrm{had}}$ updated by **F. Jegerlehner** but not yet a new *official* value;
- *Official* consensus on

$$\Delta_{\mathrm{th}}\, M_W \;=\; 2 \div 4\;\;\mathrm{MeV}, \quad \Delta_{\mathrm{th}} \sin^2\theta^l_{\mathrm{eff}} = 8\;\times\;10^{-5}$$

error on M_W decreased after $\Delta\rho$ extension to three-loops by **Kühn et al**. These will not be upgraded till a new, full two-loop calculation for $\sin^2\theta^l_{\mathrm{eff}}$: expected time

few months (optimistic) $\div$ *1.5 year*

- **QCD** at **Z - peak**, i.e. A^b_{FB}: nothing to report
- **Prob(MSM)** $\approx$ **Prob(MSSM)** (thanks Aleph!) $\Longrightarrow$ **ifNP** $\equiv$ hard breakdown;
- A_{LR} theoretical analysis: a dead horse (**M. Swartz**);
- NuTev theoretical analysis: there are controversies;
- $\Delta\chi^2$? When they have nothing better to do theorists angrily dispute on probability.

Comments, nothing more than comments:

- $\alpha^{(5)}_{\rm had}$ from theory? Yes when different calculations will agree better;
- Does the new **TU** on M_W depend on the definition of m_t (pole, $\overline{MS}$, etc)? Yes, agreement must be reached;
- Do we agree on the width of the Blueband? Yes, but only on *numerics*, not on the **rationale**. Furthermore we must reach better precision also on the extraction of $\sin^2\theta^l_{\rm eff}$, not only on the pseudo-obervable itself!
- What do you use for **QCD** corrections to A^q_{FB}? It seems that for $q = b$ the agreement between **Ravindran - van Neerven** and **Catani - Seymour** is a numerical accident;
- TU on luminosity OK but some recent reductions are based on one calculation only! Furthermore, a change in **VP** - contributions will also be a change in the central value, not only in the error.

Prolegomena, Comments & Conclusions:

- We have made plenty of mistakes;
- we could have done it better,
- but this hardly explains the present would-be-crisis and tell us that the **RO** - branches of our codes have been working properly.

Therefore, $\cdots$

for many years all the fits have been telling us that

the new theory is smootly joined to the **MSM**,
no edge no wedge.

You do something else for a while and, all of the sudden, it is 1% probability (make it 4%).

What's going on?

As usual many will take care of *speculations* while few will be left behind the enemy's line to make sure that the dirty stuff is OK. *Here I will talk only about that.*

Mistakes & Incompleteness: Babel's tower

- Definition of mass for unstable particles; we *insisted* on using OMS masses in a situation where we had to use,

  complex poles

  This is, perhaps, not a big deal with M_Z (input parameter) but it is a mess – now that we start having two-loop results – for M_W (a derived quantity);
- Most likely, SA - codes could now be replaced by MC (with the same speed);
- **QED** would be even better: better treatment for SF, also for processes non-annihilation-dominated , for IS/FS interference.
  etc;
- the whole procedure of de-convolution could be changed or made more efficient like the (in)famous and well-known t-channel subtraction for Bhabha.
- **signal** defined by cuts and not by diagrams

$\boxed{\text{Quintessence of Fits}}$

Fits are (usually) performed from **PO** $\rightarrow$ lagrangian parameters and **RO** are basically used to operate the procedure of de-convolution

so, before discussing New Physics – at the tree level ? –

we must make sure that all the ingredients are correct,
are the whole answer or, at least, the bulk of it
and do represent the status of art.

Technically, we are forced to go from σ's through the **PO** to a LEP-combined set of **PO** , and then we interpret these within the frame-work of the **SM**

Question:

- to what extend are the **PO** *model-independent* measurements, valid even in the case of the **SM** being not the correct theory?
- Many effects are indeed absorbed in the **PO** , e.g. the Γ's are largely independent of **QED** or **QCD** ; these only enter when interpreting the widths within the **SM** to extract its parameters.

- the quantities that have been measured, M_Z, Γ_Z etc have a dependence on the **SM** and cannot be understood without it

  In a sence, the *measured* value of M_Z does depend on our present-day understanding of the theory.

Underground work

A painful but necessary work was, towards the end of the Lep era, devoted in fixing all the conventions/definitions regarding

- □ **PO** ,
- □ **RO** $\to$ **PO** , **hep-ph/9902452**
- □ **SM** remnants,
- □ occurence of imaginary parts, Z/γ etc.

Quest for error analysis of phenomenological tools

The dirty work

- **Exp. strategy**

Technically, each LEP experiment extracts a set of **POs** , from their measured σs and $A_{_{\rm FB}}$s The 4 sets are combined, taking correlated errors into account $\Rightarrow$

LEP-average set of **POs** , $< PO >_{\rm Lep}$ $\Rightarrow$

then interpreted, e.g. within the **MSM**.

Practical attitude: to stay with a **MI** fit, from **ROs** $\to$ **POs** ($\oplus$ a **SM** remnant) for each experiment, and these sets of **POs** are averaged.
The extraction of $M_{_Z}, m_t, M_{_H}, \alpha_{_S}(M^2_{_Z})$ and $\alpha(M^2_{_Z})$, is based on $< PO >_{\rm Lep}$.

- **Th. strategy**

Within the context of the **SM** the **ROs** are described in terms of some set of amplitudes

$$A_{_{\rm SM}} = A_\gamma + A_{_Z} + \text{non-factorizable},$$

$$\sigma(s) = \int dz\; H_{\rm in}(z,s)\, H_{\rm fin}(z,s)\, \hat{\sigma}(zs),$$

One needs to specify M_Z, the (remaining) relevant **SM** parameters for the **SM** -complement,

$$\mathrm{RO} = \mathrm{PO} \oplus \overline{\mathrm{SM}}.$$

This part of the procedure is particularly cumbersome. However, one has to live with the fact that – for practical reasons – this is the procedure and one is left with the task of making sure that it is acceptable.

PO - ology: anti-realistic approach

The explicit formulae for the $Zf\overline{f}$ vertex are

$$\rho^f_Z \gamma_\mu \left[\left(I^{(3)}_f + i\, a_L\right)\gamma_+ - 2\, Q_f \kappa^f_Z s^2 + i\, a_Q\right] \;=\; \gamma_\mu \left(\mathcal{G}^f_V + \mathcal{G}^f_A\, \gamma_5\right),$$

where $\gamma_+ = 1 + \gamma_5$ and $a_{Q,L}$ are the **SM** imaginary parts.

By definition, the total and partial widths of the Z boson include also **QED** and **QCD** corrections. Therefore

$$\Gamma_f \equiv \Gamma\left(Z \to f\overline{f}\right) \;=\; 4\, c_f\, \Gamma_0 \left(|\mathcal{G}^f_V|^2\, R^f_V + |\mathcal{G}^f_A|^2\, R^f_A\right) + \Delta_{\mathrm{EW/QCD}}\;,$$

where $c_f = 1$ or 3 for leptons or quarks $(f = l, q)$, and R_V^f and R_A^f describe the final state **QED** and **QCD** corrections and take into account the fermion mass m_f. The last term,

$$\Delta_{\mathrm{EW/QCD}} = \Gamma^{(2)}_{\mathrm{EW/QCD}} - \frac{\alpha_S}{\pi}\,\Gamma^{(1)}_{\mathrm{EW}}\,,$$

accounts for the non-factorizable corrections.

The standard partial width, Γ_0, is

$$\Gamma_0 = \frac{G_F M_Z^3}{24\sqrt{2}\,\pi} = 82.945(7)\ \mathrm{MeV}.$$

The peak hadronic and leptonic cross-sections are defined by

$$\sigma_h^0 = 12\pi\,\frac{\Gamma_e\Gamma_h}{M_Z^2\Gamma_Z^2} \qquad \sigma_\ell^0 = 12\pi\,\frac{\Gamma_e\Gamma_l}{M_Z^2\Gamma_Z^2}\,,$$

where Γ_Z is the total decay width of the Z boson, i.e, the sum of all partial decay widths.

The effective electroweak mixing angles (*effective sinuses*) are always defined by

$$4\,|Q_f|\,\sin^2\theta^f_{\mathrm{eff}} = 1 - \frac{\mathrm{Re}\,\mathcal{G}_V^f}{\mathrm{Re}\,\mathcal{G}_A^f} = 1 - \frac{g_V^f}{g_A^f}\,,$$

where we define

$$g_V^f = \mathrm{Re}\,\mathcal{G}_V^f, \qquad g_A^f = \mathrm{Re}\,\mathcal{G}_A^f.$$

The forward-backward asymmetry $A_{\rm FB}$ is defined via

$$A_{\rm FB} = \frac{\sigma_{\rm F} - \sigma_{\rm B}}{\sigma_{\rm F} + \sigma_{\rm B}}, \qquad \sigma_{\rm T} = \sigma_{\rm F} + \sigma_{\rm B},$$

where $\sigma_{\rm F}$ and $\sigma_{\rm B}$ are the cross sections for forward and backward scattering, respectively.

Before analysing the forward-backward asymmetries we have to describe the inclusion of imaginary parts. $A_{\rm FB}$ is calculated as

$$A_{\rm FB} = \frac{3}{4}\frac{\sigma_{\rm VA}}{\sigma_{\rm T}},$$

where

$$\sigma_{\rm VA} = \frac{G_F M_Z^2}{\sqrt{2}} \sqrt{\rho_e \rho_f}\, Q_e Q_f {\rm Re}[\alpha^*(M_Z^2) \mathcal{G}_V^e \mathcal{G}_A^f \chi(s)] + \frac{G_F^2 M_Z^4}{8\pi} \rho_e \rho_f {\rm Re}[\mathcal{G}_V^e \left(\mathcal{G}_A^e\right)^*] {\rm Re}[\mathcal{G}_V^f \left(\mathcal{G}_A^f\right)^*] s\, |\chi(s)|^2.$$

To Summarise the MI ANSATZ:

One starts with the **SM** , which introduces complex-valued couplings, calculated to some order in perturbation theory.

Next we define g_V^f, g_A^f as the real parts of the effective couplings and Γ_f as the physical partial width absorbing all radiative corrections including the imaginary parts of the couplings and fermion mass effects.

Furthermore,

$$R_q = \frac{\Gamma_q}{\Gamma_h}, \qquad R_l = \frac{\Gamma_h}{\Gamma_l},$$

for quarks and leptons, respectively.

The experimental collaborations report **POs** for the following sets:

$$(R_f, A_{\mathrm{FB}}^{0,f}), \qquad (g_V^f, g_A^f), \qquad (\sin^2\theta_{\mathrm{eff}}^f, \rho_f).$$

In order to extract g_V^f, g_A^f from Γ_f one has to get the **SM**

-**remnant**, all else is trivial. However, the parameter transformation cannot be completely MI, due to the residual **SM** dependence.

In conclusion, the flow of the calculation requested by the experimental Collaborations is:

1. pick the Lagrangian parameters $m_t, M_{_H}$ etc. for the explicit calculation of the residual **SM** -dependent part;
2. perform the **SM** initialisation of everything, such as imaginary parts etc. giving, among other things, the complement $\overline{\mathrm{SM}}$;
3. select $g^f_{_V}, g^f_{_A}$;
4. perform a **SM** -like calculation of Γ_f, but using arbitrary values for $g^f_{_V}, g^f_{_A}$, and only the rest, namely

$$R^f_{_V}, \quad R^f_{_A}\,, \qquad \Delta_{_{\rm EW/QCD}}\,, \qquad \mathrm{Im}\,\mathcal{G}^f_{_V}, \quad \mathrm{Im}\,\mathcal{G}^f_{_A}\,,$$

from the **SM** .

An example of the parameter transformations is the following: starting from $M_Z, \Gamma_Z, R_{e,\mu,\tau}$ and $A^{0,e,\mu,\tau}_{\rm FB}$ we first obtain

$$\begin{aligned}
\Gamma_e &= M_Z\Gamma_Z\,[\frac{\sigma^0_h}{12\,\pi R_e}]^{1/2}, \\
\Gamma_h &= M_Z\Gamma_Z\,[\frac{\sigma^0_h R_e}{12\,\pi}]^{1/2}.
\end{aligned}$$

With

$$\mathcal{A}_e = \frac{2}{\sqrt{3}}\sqrt{A^{0,e}_{\rm FB}}\,, \qquad \text{and} \quad \gamma = \frac{G_F M^3_Z}{6\sqrt{2}\,\pi}\,,$$

we subtract **QED** radiation,

$$\Gamma^0_e = \frac{\Gamma_e}{1 + \frac{3}{4}\frac{\alpha(M^2_Z)}{\pi}}\,,$$

and get

$$\begin{aligned}
\sin^2\theta^e_{\rm eff} &= \frac{1}{4}\left(1 + \frac{\sqrt{1-\mathcal{A}^2_e}-1}{\mathcal{A}_e}\right), \\
\rho_e &= \frac{\Gamma^0_e}{\gamma}\,[\left(\frac{1}{2} - 2\,\sin^2\theta^e_{\rm eff}\right)^2 + \frac{1}{4} + \left(\mathrm{Im}\,\mathcal{G}^e_V\right)^2 + \left(\mathrm{Im}\,\mathcal{G}^e_A\right)^2]^{-1}.
\end{aligned}$$

With

$$\mathcal{A}_f = \frac{4}{3}\frac{A^{0,f}_{\rm FB}}{\mathcal{A}_e}\,,$$

We further obtain

$$\sin^2\theta^f_{\rm eff} \;=\; \frac{1}{4\,|Q_f|}\,\left(1+\frac{\sqrt{1-\mathcal{A}^2_f}-1}{\mathcal{A}_f}\right),$$

$$\rho_f \;=\; \frac{\Gamma^0_f}{\gamma}\,[\Big(\frac{1}{2}-2|Q_f|\sin^2\theta^f_{\rm eff}\Big)^2+\frac{1}{4}+\Big({\rm Im}\;\mathcal{G}^f_{V}\Big)^2+\Big({\rm Im}\;\mathcal{G}^f_{A}\Big)^2]^{-1},$$

where $f=\mu,\tau$. For quarks one should remember to subtract first non-factorizable terms and then to distinguish between R^f_V and R^f_A.

RO - ology: structuralist approach

$RO_{\rm ext}\;\equiv$ **extrapolated setup**

$RO_{\rm real}\;\equiv$ **realistic kinematical cuts**

The **ROs** are computed in the context of the **SM** . Thus the comparison between `TOPAZ0` and `ZFITTER` is mainly a **SM** comparison, however one of the goals was to pin down

- the definition of **POs** ;
- the calculation of **ROs** in terms of the defined **POs** for the purpose of MI fits, showing that for **POs** with values as calculated in the **SM** , the **ROs** are *by construction* identical to the full **SM RO** calculation.

The last point requires expressing ρ's and effective mixing angles in terms of **POs** , assuming the validity of the **SM** .

After this transformation the **ROs** will be given as a function of the **POs** at their **SM** values.

This is not at all a trivial affair because of gauge invariance and one should remember that gauge invariance at the Z pole (on-shell gauge invariance) is entirely another story from gauge invariance at any arbitrary scale (off-shell gauge invariance).

Some of the re-summations that are allowed at the pole and that heavily influence the definition of effective Z couplings are not trivially extendible to the off-shell case.

Therefore, the expression for **RO** =**RO** (**PO**), at arbitrary s, requires a careful examination and should be better understood as **RO** =**RO** (**PO** ,$\overline{\mathrm{SM}}$), that is, for example:

$$\sigma_{\rm MI} = \sigma_{\rm SM}\Big(R_l, A^{0,l}_{\rm FB}, \dots \to g^f_{_V}, g^f_{_A} \to \rho_f, \sin^2\theta^f_{\rm eff}; \text{residual SM}\Big).$$

As long as the procedure does not violate gauge invariance and the **POs** are given **SM** values, there is nothing wrong with the calculations. It is clear that in this case the **SM** **ROs** coincide with the MI **ROs** .

Ingredients for **ROs**

Next-to-Leading and Mixed Corrections

The inclusion of mixed two-loop correction for **RO** , at $s \neq M^2_{_Z}$, can only represent an approximation to the real answer.

One should realize that s' is not equivalent to the invariant mass of the final-state $f\overline{f}$ system, $M^2(f\overline{f})$, due to final state **QED** and **QCD** radiation.

For an s'-cut both **QED** and **QCD** final-state radiation are included through an inclusive correction factor. For M^2-cut the result remains perfectly defined for leptons, however for hadronic final states there is a problem. This has to do with **QCD** final-state corrections.

Indeed we face the following situation:

- for $e^+e^- \to f\overline{f}\gamma$ the exact correction factor is known at $\mathcal{O}(\alpha)$ even in the presence of a $M^2(f\overline{f})$ cut;
- the complete set of final-state **QCD** corrections are known up to $\mathcal{O}\left(\alpha_S^3\right)$ only for the fully inclusive setup, i.e., no cut on the $f\overline{f}$ invariant mass;
- the mixed two-loop **QED** /**QCD** final-state corrections are also known only for a fully inclusive setup;
- at $\mathcal{O}(\alpha_S)$ **QCD** final-state corrections in presence of a M^2-cut follow from the analogous **QED** calculation.

The ideal thing would be to have **QED** $\oplus$ **QCD** final-state *radiator factors* R, with a kinematical cut imposed on $M^2(f\overline{f})$. Missing this calculation, we have three options

$$
\begin{aligned}
R^{\rm FS} &= 1 + R^{\rm FS}_{\rm QED,cut} + R^{\rm FS}_{\rm QCD,ext}\,, \\
R^{\rm FS} &= \left(1 + R^{\rm FS}_{\rm QCD,ext}\right)\left(1 + R^{\rm FS}_{\rm QED,cut}\right), \\
R^{\rm FS} &= 1 + R^{\rm FS}_{\rm QED,cut} + R^{\rm FS}_{\rm QCD,cut}\,.
\end{aligned}
$$

The (**QCD** ,ext) corrections are understood up to $\mathcal{O}\left(\alpha_S^3\right)$, while those corresponding to the (**QCD** ,cut) setup are only computed at $\mathcal{O}(\alpha_S)$.

DECONVOLUTION: a matter of public anxiety

- Single-de-convolution (SD), giving the kernel cross-sections without initial state **QED** radiation, but including all final-state correction factors.
- Double-de-convolution (DD), giving the kernel cross-sections without initial- and final-state **QED** radiation and without any final-state **QCD** radiation.

By comparing SD with DD quantities we are able to disentangle the effect of initial-state **QED** radiation from final-state **QED** $\oplus$ **QCD** radiation.

Note the following relation between the **PO** R_b and the ratio of DD cross-sections ($\mathrm{exp} = 0.21644 \pm 0.00065$)

$$R_b = \frac{\sigma_b}{\sigma_{\mathrm{had}}}\Big|_{\sqrt{s}=M_Z} - \begin{cases} 0.00146 & \texttt{TOPAZ0} \\ 0.00146 & \texttt{ZFITTER}\,. \end{cases}$$

The difference reflects the $\overline{SM}$-remnant effect, since the ratio of **RO** cross-sections has γ-exchange, imaginary parts, $\dots$, and (substantially negligible) weak boxes.

$\sqrt{s}$ [GeV]	central	minus error	plus error	T-Z
σ_μ				
$M_z - 3$	0.22849 nb	0.04 pb	$\leq$ 0.01 pb	0.07 pb
$M_z - 1.8$	0.47657 nb	0.08 pb	0.01 pb	0.04 pb
M_z	1.48010 nb	0.09 pb	0.20 pb	0.16 pb
$M_z + 1.8$	0.69512 nb	0.08 pb	0.06 pb	0.03 pb
$M_z + 3$	0.40642 nb	0.06 pb	0.03 pb	0.04 pb
$A^\mu_{\rm FB}$				
$M_z - 3$	−0.28312	0.00009	0.00001	0.00018
$M_z - 1.8$	−0.16977	0.00008	0.00004	0.00008
M_z	−0.00062	0.00006	0.00009	0.00004
$M_z + 1.8$	0.11186	0.00004	0.00012	0.00004
$M_z + 3$	0.15466	0.00004	0.00012	0.00005
$\sigma^\mu_{\rm F}$				
$M_z - 3$	0.08190 nb	0.03 pb	$\leq$ 0.01 pb	
$M_z - 1.8$	0.19783 nb	0.05 pb	0.01 pb	
M_z	0.73959 nb	0.04 pb	0.17 pb	
$M_z + 1.8$	0.38644 nb	0.06 pb	0.08 pb	
$M_z + 3$	0.23464 nb	0.04 pb	0.04 pb	
$\sigma^\mu_{\rm B}$				
$M_z - 3$	0.14659 nb	0.02 pb	$\leq$ 0.01 pb	
$M_z - 1.8$	0.27874 nb	0.03 pb	$\leq$ 0.01 pb	
M_z	0.74051 nb	0.04 pb	0.04 pb	
$M_z + 1.8$	0.30868 nb	0.02 pb	$\leq$ 0.01 pb	
$M_z + 3$	0.17178 nb	0.02 pb	$\leq$ 0.01 pb	
$\sigma_{\rm had}$				
$M_z - 3$	4.45012 nb	0.99 pb	1.40 pb	−1.29 pb
$M_z - 1.8$	9.59909 nb	1.81 pb	3.41 pb	−2.49 pb
M_z	30.43639 nb	1.85 pb	14.27 pb	−11.83 pb
$M_z + 1.8$	14.18269 nb	2.14 pb	6.01 pb	−1.27 pb
$M_z + 3$	8.19892 nb	1.46 pb	3.38 pb	−0.36 pb

Table 1: Theoretical uncertainties for $\sigma_\mu, A^\mu_{\rm FB}, \sigma^\mu_{\rm F,B}$ and for $\sigma_{\rm had}$ from `TOPAZO`.

RO Rating:

$\star\star\star$ FS **QED**

$\star\star\star$ M_H-dependence of D - observables

$\star\star$ C - observables, extrapolated setup

$\star\star$ M_H-dependence of C - observables

$\star$ C - observables, realistic setup

$\star$ C - observables, MI approach

Rating to be understood as compared to exp. error

Ontology: the Blue Band

The most celebrate figure of the LEP era: the blue-band.

I remember a meeting at Cern where I proposed to produce theoretical results with a $\Box$, reflecting our lack of knowledge of missing higher order corrections, instead of dimensionless $\circ$. There was an immediate consensus in the community. This is the progenitor of the blue-band.

This band was intended to

honestly show our degree of ignorance and,

several times, it was repeated that it should be used and interpreted with great care.

Actually there is no definition of *theoretical error* (only of theoretical stupidity) and one should not attach to it any meaning more deep than

modelling & selecting a set of options and see how large is the band,

If it is too large then we better do a new calculation in that direction. If it is small yet it does not mean that we should take it as a rigorous bound.

Memento:

$$TU(PO) \;=\; PO_{\rm max} - PO_{\rm min},$$

$$PO_{\rm max} = \max_{\{i_1\in I_1,\cdots,i_n\in I_n\}} PO(O_{i_1},\cdots,O_{i_n})$$

$$PO_{\rm min} = \min_{\{i_1\in I_1,\cdots,i_n\in I_n\}} PO(O_{i_1},\cdots,O_{i_n})$$

$$TU(PO) < \max_{i_1,\cdots,i_n} PO(O_{i_1},\cdots,O_{i_n}) - \min_{i_1,\cdots,i_n} PO(O_{i_1},\cdots,O_{i_n})$$

From this point of view I disagree *ab initio* on having any discussion of this kind,

one should even remember that some of the options in the codes have been buried and forgotten simply because they were not following the orthodoxy giving a too large band.

Yes, we defintely need a complete two-loop calculation for some or all the **PO** and the fact that these calculations are not coming yet is a sign that they are not easy at all.

I have no objection in having right now a larger blue-band (why not a safety factor?) but I refuse to accept incomplete arguments as the rationale for justifying rigorously the enlargement.

logic of illogicality: chop the band?

should a theoretical calculation which is running over the Higgs mass and assumes the validity of the **SM** include everything that is allowed despite experimental evidence?

NO

absence of a very light Higgs should be used and the $\Delta\chi^2$ curve should be replaced with another one where a penalty function is included and we never move into the forbidden region.

Almost forgotten, pair production

- ⊘ There are situations where pair corrections are a very small effect due to the near-cancellation of real and virtual pairs.
- ⊘ Whenever the effect of pairs is of order 0.1%, it is below the LEP combined precision of any **2f** cross-section.
- ⊘ Whenever pairs are an issue of order 1% or more this can become important for LEP wide combinations.

Let's continue with a simple case,

$$e^+e^- \quad \text{PP-corrections to} \quad e^+e^- \to \overline{b}b \tag{1}$$

The relevant diagrams are:

- **Multi-Peripheral** or **MP**;
- **Initial State Singlet**, or **ISS**;
- **Initial State Non-Singlet**, or **ISNS**;
- **Final State**, or **FS**.

Note that we include both γ and Z exchange, so that one could still distinguish between $\mathbf{ISNS}_\gamma$, $\mathbf{ISNS}_Z$ and interference. On top of real **PP** one has to include virtual e^+e^- pairs.

Tentative conclusions:

⌢ The whole **4f** must be included to compute the **2f** cross-sections;

⌣ The whole **4f** is to be divided into two components, **signal** and **background**.

We go from one extreme solution to the other:

- **background** $= \emptyset$, if everything is included in the **SA**. **MP** is an example of something difficult to implement into the **SA** if low-**IM** regions are required.

- **signal** $=$ **ISNS**, i.e. everything else (large effects) is subtracted by **MC**. However, using different **MC** programs would bring to subtractions that differ by some per-cent, which then would have to be regarded as a *theoretical* systematic uncertainty.

$\boxed{\text{Autopsy:}}$

Should we retreat from a metaphysics of fits entirely?

Another approach exists, extraction of lagrangian parameters directly from the **RO** , which are not (of course) raw data but rather educated manipulations of raw data, e.g. distributions defined for some simplified setup.

I have nothing to say about this first part, theorists should do theory and experimentalists should do experiments.

I have been asking so many times to produce this kind of fits that at the end I managed to get some answer. This goes back to 1999:

Changes in DELPHI fits ('93-95 data)

SM parameters directly from **RO** or through **PO** .

- The largest change was observed in M_Z: it amounts to 1.2 MeV or roughly 1/3 of their error (25% of common exp. error for Aleph);
- For m_t there was no observed change;
- $\log_{10}(M_H)$ changed by roughly 10% of its error, and α_S by roughly 15%.

Aleph '99

It has been tested how the results on **SM** parameters differ between

a **SM** fit to the measured **ROs** (to say 'direct fit') and

a **SM** fit to the **POs** which themselves are derived in an MI fit to the measured **ROs** .

The observed differences in fitted central values in the ALEPH case are

- 20% of the fitted error for M_Z,
- 5% on α_S and
- < 1% on m_t and $\log_{10}(M_H/\text{ GeV})$.

L3 '99

SM fit to **PO** (**POs** having been fitted to L3 **ROs**)

$$
\begin{aligned}
1/\alpha &= 128.9000 \pm .0874 \\
\alpha_{_S} &= .12614 \pm .00579 \\
M_{_Z} &= 91.18917 \pm .00308 \\
m_t &= 175.70 \pm 4.83 \\
M_{_H} &= 30.0 \pm 38.1
\end{aligned}
$$

SM fit to **RO** (from L3)

$$
\begin{aligned}
1/\alpha &= 128.9006 \pm .0886 \\
\alpha_{_S} &= .12581 \pm .00583 \\
M_{_Z} &= 91.18927 \pm .00310 \\
m_t &= 175.64 \pm 4.83 \\
M_{_H} &= 29.8 \pm 39.0
\end{aligned}
$$

This is some shift but hardly explains the present paroxysmal attack of distress and tell us that for all what we know the **RO** - branches of our codes have been working properly.

How about Theoretical agreement/disagreement†?

σs generated with TOPAZ0 at seven energy points, with errors assigned such as to represent the experimental statistics, plus correlated luminosity errors, were run through an MI fit with ZFITTER. The agreement of the parameters is remarkable:

TOPAZ0 **PO** values

$$M_Z = 91.1867, \quad \Gamma_Z = 2.4955, \quad \sigma^0_{\rm had} = 41.476$$
$$m_t = 173.8, \quad M_H = 100.0, \quad \alpha_S = 0.119, \quad 1/\alpha = 128.878$$

Parameters fitted with ZFITTER 5.20 from TOPAZ0 cross sections:

$$M_Z = 91.1866, \quad \Gamma_Z = 2.4956, \quad \sigma^0_{\rm had} = 41.476$$

† It is converted into a covariance matrix of $T_{\rm error}$

From 2001 (hep-ex/0101027):

	A	D	L	O
$\chi^2/N_{\rm df}$	174/180	184/172	168/170	161/198

Δ LP	% of error
ΔM_Z [MeV]	1
$\Delta \log_{10}(M_H/ \mathrm{GeV})$	4
$\Delta\alpha_S$	4
$\Delta(\Delta^{(5)}_{\rm had})$	2

- Reasonable $\chi^2/N_{\rm df}$ for each **Lep** experiment; shifts on lagrangian parameters $\equiv$ few % of the experimental error (although M_H). 2002 analysis?
- What about now as compered to **All Data** – 27.7/13 ?
- This we have under control for Lineshape **Lep** data, what about **ROs** for heavy - flavours or non-Lep? A_{LR} realistic and its $\overline{\rm SM}$?

$$\sin^2\theta_{\rm exp}[A_L] = \sin^2\theta_{\rm exp}[A_H]\,\{1+(\frac{\alpha}{\pi})^2\,L^2\},$$

$L \approx$ 27 but we don't need a $3\,\sigma$ reduction!

$\boxed{\textbf{Conclusion:}}$

- $\smile$ The level of accuracy and the architectural complexity of **TH** calculations has no comparisons with the past;
  - $\frown$ Two-loop (heavy top) SL missing for **b** - sector;
  - $\frown$ Two - loop M_W **(Y)** $\not\equiv \sin^2\theta^l_{\rm eff}$ **(N)**
- $\smile$ we know **ROs** pretty well around the Z peak, reasonably well up to **LEP 2** energies;
- $\smile$ we have been constantly aware of our mistakes and limitations and they are under control at the requird level, at least for **Lep**;
  - $\frown$ $t - t$, $s - t \in \overline{\rm SM}$;
  - $\frown$ $\{m_t, \alpha_S, M_H\}_{\overline{\rm SM}}$ fixed;
  - $\frown$ default $RO = RO({\rm Re}PO)$
- $\frown$ one-loop **ROs** have not been generated beyond **2f** FS during the LEP cycle;
- $\frown$ Full two-loop EW is still in its early infancy;
- $\frown$ Here we are to bury theory and not to prize it. However, for those who survived so many anomalous events it is not so much different from the ending scene of Casablanca.