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SUMMARY AND OUTLOOK

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1 Preamble

A little over a year ago I gave the *summary* talk at SUSY-95 in Paris. It was quite a dreadful experience. I came to the Conference with nothing in my hands, sat through all the talks but one, preparing my transparencies at night, skipping all social events. Very tired, I ended trying to summarize every talk, even the one which, not only I had missed, but also, as I learned later, had been cancelled. . . This is why, this time, I decided for a compromise, taking advantage of the fact that I was also asked to provide an *outlook*. I came with a number of prepared transparencies, yet sat through all plenary talks so that I could add something "on-line". The result is for you to judge: probably, the outlook side of the story somewhat overshadows the summary side: I apologize in advance for having skipped over many important new results presented at the Conference, in particular over those on new detectors[1] and on future accelerators[2], subjects which are far from my own expertise. Fortunately, you will find all the missing stuff in the preceding 27 plenary talks!

We are approaching the end of the century and we may ask ourselves: What can we learn from this century's experience in our field? It looks to me that a combination of

- new, solid experimental facts,
- sound, inspired theoretical thinking,

have paved the way to progress over and over again:

- The finite, constant speed of light led to Special Relativity via the equivalence of different Lorentz frames.

- The universality of free-fall gave General Relativity out of Einstein's generalized equivalence principle.
- The stability of atoms led to Quantum Mechanics through the uncertainty principle.
- The wealth of particle data we accumulated for many decades, and a theoretically sound way to combine special relativity and quantum mechanics, led to the Standard Model.

The outlook side of my talk should try to answer the question: What's next? You will see below how much/little I have dared to enter suchslippery grounds.

2 Listening to Nature

One does not have to be very biased to conclude that:

- Nature likes *local* symmetries.
- At present, *all* phenomena are well described by a *gauge* theory based on the group:

  $$G_{gauge} = SU(3)_c \otimes SU(2)_L \otimes U(1)_Y \otimes Diff_4 \,, \tag{2.1}$$

  where the last, perhaps less familiar, factor stands for the invariance group of General Relativity (differentiable, general coordinate transformations). The gauge bosons of G_{gauge} (a photon, a graviton, eight gluons, three intermediate vector bosons) mediate the four known fundamental forces with couplings:

  $$\alpha, G_N, \alpha_s, G_F \,. \tag{2.2}$$

- Although mass terms for the gauge bosons are forbidden, long-range gauge forces can be avoided either through the Higgs mechanism or through confinement. We believe that:
  - Strong interactions are realized (at zero temperature) in the confining phase.
  - Weak interactions are realized (again at zero temperature) in the Higgs phase.
  - Electromagnetic and gravitational interactions are realized in the Coulomb phase (i.e. with massless gauge bosons).

As a result, two of the fundamental interactions are short range, two are long range.

- The basic (left-handed spin 1/2) constituents of matter belong to a baroque (read highly reducible), *completely chiral* representation R of G_{gauge}:

  $$\begin{aligned} \Psi_L \in R \;\; &= \;\; 3 \cdot [(3,2,1/3,1/2) + (\bar{3},1,-4/3,1/2) + (\bar{3},1,2/3,1/2) + \\ &\quad +(1,2,-1,1/2) + (1,1,2,1/2)] \,, \end{aligned} \tag{2.3}$$

  where the numbers inside the brackets indicate the representations for the various factors appearing in (2.1) and the overall factor 3 stands for family repetition. The corresponding (right-handed) antiparticles automatically transform in the complex-conjugate representation $\bar{R}$.

  The physical meaning of a completely chiral fermionic representation is that no (gauge-invariant) mass term is allowed. Chiral fermions are thus naturally light, i.e. their mass can only emerge from the (spontaneous) breaking of the gauge symmetry.

- There is, *probably*, an elementary scalar system, the complex Higgs-boson doublet H, transforming as

  $$H \in (1,2,1,0) + \text{c. c.} \tag{2.4}$$

  This remains, to this day, the most uncertain element of the Standard Model.

3 The Standard Model at work

Before turning my attention to the experimental status of the Standard Model (SM), I would like to make a short digression. Today, more than ever, a sound equilibrium between theory and experiments appears to be crucial for our field to thrive. Precise data often fail to convey an exciting message, either because we lack any idea about the origin of a particular precisely measured number (e.g. the fine-structure constant), or because it just is very hard to compute such number with comparable precision (e.g. the proton mass). Similarly, a grandiose theory making untestable predictions (string theory?) remains forever in the realm of pure speculation.

From this point of view, the electroweak theory stands out, in the sense that theoretical and experimental precisions appear to match across the board. If anything is lacking there, it is a sign of discrepancy between the two, which makes the game somewhat boring. At the other extreme we have gravity, endowed with a beautiful (though only classical) theoretical framework, General Relativity (GR), whose predicted deviations from Newtonian gravity are often too tiny to be measured (e.g. gravitational waves). Things, however, are improving fast in that area, as we shall discuss below. Finally, with the advent of QCD, strong interactions have seen an enormous improvement, with theoretical predictions often matching experimental precision in the case of hard processes. For soft physics, however, strong interaction theory is still lagging much behind experiments.

How does the SM work in detail? This was the subject of many parallel and plenary sessions at this Conference. I counted eight plenary sessions on $SU(3)$, five on $SU(2)\otimes U(1)$, and one on GR, roughlyin proportion to the number of generators... I will limit myself to highlight what, in my opinion, was qualitatively new with respect, say, to a year ago.

3.1 Strong interactions

As nicely expressed by R. Brock, a common trend in this area has been: *"An increased awareness of the glue"*, something indeed reflected in many of the topics discussed below.

3.1.1 Hadron spectroscopy. My reaction to what I heard on this topic was: Glueballs at last? For many years, theorists have claimed that glueballs should exist, albeit within admixtures with $q\bar{q}$ states. It now looks[3] that a few doubtful previous candidates have magically turned into a single convincing one. We can write this as an equation:

$$G(1590) = f_0(1450) = f_0(1500) = \text{glueball ?} , \qquad (3.1)$$

where the three above gluon candidates have been reported, respectively, by GAMS, WA91 and the Crystal Barrel experiment. It seems that the apparent mass differences among these states can be explained in terms of experimental cuts and/or different acceptances so that the three could very well be one and the same particle. But why a glueball?

Landua[3] has discussed four glueball tests, which are all passed by a O^{++} glueball. I can add a fifth one, which actually goes back to an old paper[4] whose title is almost the same as eq. (3.1). Unlike what one would naively expect, a relatively pure glueball should decay more often into $\eta(\eta')\eta(\eta')$ than into 2π or $K\bar{K}$, up to phase-space effects. This is because the flavour-singlet pseudoscalar (a known mixture of $\eta\eta'$) gets its mass from the Adler-Bell-Jackiw (ABJ) anomaly, a phenomenon related to a purely gluonic channel. The $\sigma\sigma \to 4\pi$ channel should also be favoured, if the σ (the broad $\pi\pi$ structure around 700 MeV) is somewhat mixed with the lightest scalar glueball. All these expectations seem to be consistent with the data, suggesting that a positive answer to eq. (3.1) might finally be in sight.

3.1.2 Spin structure of the nucleon. The so-called spin crisis, rather than a real crisis of QCD, is yet another manifestation of the importance of the glue, being directly related to the just-mentioned ABJ anomaly. The latter, besides boosting up the η' mass (thus solving the famous $U(1)$ problem), should also suppress the flavour-singlet axial current (which is what the spin crisis is all about). On the experimental front, nice progress was reported[5] by the CERN and SLAC collaborations on the measurements of $g_1^{p,d,{}^3He}$ down to small x. Together with planned experiments on heavy-flavour production and on the structure of the hadronic final state, this should soon help clarify the issues.

3.1.3 Small-x physics, soft interactions, diffraction. We may call what has been happening in this area[6] the Pomeron's comeback. It reminds me of Sid Coleman teasing some of us in the late 60's: "At Harvard, we think of the Pomeron as a French wine ..." (for non-experts: the "Pomerol" is a rather famous Bordeaux wine). As often emphasized by Landshoff, the Pomeron is actually back today in more than one brand: the soft, the hard, the inclusive and the exclusive. What are they?

- The *inclusive* Pomeron, the one controlling total cross-sections via the optical theorem, becomes *soft* when one deals with soft interaction physics, in which case it has an intercept $\alpha \sim 1.08$, or hard, if it refers to Deep Inelastic Scattering (DIS) at very small x. In the latter case, one talks about the BFKL (from Balitsky, Fadin, Kuraev, Lipatov) *hard* Pomeron with a much larger intercept ($\alpha \sim 1.15 - 1.23$). Impressive experimental progress has been made at HERA[8] in measuring the latter accurately. At the theoretical level, progress has occurred by combining the old Lipatov model with large-N expansion ideas. Amusingly, this has led Lipatov, Fadeev, Korchemsky and others to reformulate the problem in terms of an Ising ferromagnet[9]!

- The *exclusive* Pomeron, which is measured in diffractive events. Again, this can be *soft* or *hard* depending on the nature of the events that are associated with a diffractive trigger. Much exciting experimental work has been going on at HERA[8] in measuring the so-called Pomeron structure function by looking at the Ingelman–Schlein process[10], hard scattering associated with large rapidity gaps. The data are parametrized in terms of the diffractive structure function $F_2^{D(3)}(x_P, \beta, Q^2)$, a particular example of a more general object, the "fracture function"[11]$M^i_{p,h}(z, x, Q^2)$, which describes a semi-inclusive hard process initiated by the parton i, associated with the detection of a final hadron h in the target (here proton) fragmentation region.

For the first time, there have been reports[8] of measurements of $M^i_{p,n}(z, x, Q^2)$ which should be related, in analogy with $M^i_{p,p}$ (the Pomeron case), to the pion structure function, an object already measured in the pion–nucleon Drell–Yan process. Another interesting piece of news from DESY[8]: the Pomeron seems to contain hard gluons; actually, maybe it just consists of a couple of hard gluons at low Q^2, with the rest simply following from Gribov–Lipatov, Altarelli–Parisi (GLAP) evolution. This could be interesting news for the Higgs boson search at hadron colliders [12]: triggering on (semi–) diffractive events should increase the gluon-to-quark flux ratio. Since gluon pairs produce Higgs bosons, while quark pairs give a two-photon background (the most promising decay channel for a "light" Higgs), increasing the gluon/quark ratio increases the signal/background ratio for Higgs production.

3.1.4 α_s. A new global average was given[13] at the conference:

$$\alpha_s(M_Z) = 0.118 \pm 0.003 \ . \tag{3.2}$$

The good news is that i) the DIS value is no longer "low"; ii) the EW/LEP value is no longer "high"; iii) lattice calculations are consistent with other determinations. A compilation of different determinations can be found in [13]. In that report one can also find a plot of each value of α_s against the scale at which the experiment was performed, and thus "see" the running of α_s.

Before leaving this part of QCD tests, I wish to mention that the difference between quark and gluon jets is coming out clearer and clearer from the data when one looks at various average properties of the jets e.g. $\langle n\rangle$, $\langle p_t\rangle$, $\langle x\rangle$. This without mentioning many other very fine tests of QCD based on the detailed analysis of hadronic final states [7].

3.1.5 Top and other heavy quarks. Progress has been reported[14] from CDF and D0 at Fermilab on the top quark, a crucial component of the SM. The production cross section no longer looks too high, while the mass determination $m_t = 175 \pm 6\mathrm{GeV}/c^2$ is becoming precise enough to become an important constraint on SM precision tests (see Section 3.2).

This is perhaps the right place to mention that the high-p_t jet cross section at Fermilab is no longer considered an embarrassment for QCD[15]. New, improved gluon distributions appear to be consistent with the data, thus pouring cold water on some original claims of new physics.

Concerning less-heavy quarks (c and b), theoretical progress is continuing[16] on the heavy-quark effective theory (HQET), leading, for instance, to a nice determination of $|V_{cb}|$ (see Section 3.2.2).

3.1.6 Heavy ions. Last but not least I am coming to the search for signatures of the quark–gluon plasma in heavy-ion collisions at CERN. Have we already seen the first glimpses of the plasma? The good news[17] here comes from NA50, which has recently studied Pb–Pb collisions at $E = 158\mathrm{GeV}$ per nucleon, evidentiating a sharp drop in $\frac{\sigma(J/\psi)}{\sigma_{DY}}$. This ratio had been proposed long ago [18] as a sensitive probe of a phase transition. Unlike the case of pp, pd, p—W, S–U collisions, in Pb–Pb collisions the drop cannot possibly be explained by the nuclear absorption cross-section. From various plots shown in[17], one can see not only the sharp drop in the Pb–Pb case, but also how such a drop becomes more and more pronounced as one looks at "centrality" bins corresponding to increasing E_T.

3.2 Electroweak Interactions

Before moving to the "pièce de résistance", the status of EW interactions, let me mention a few other interesting items that we heard about:

3.2.1 Top quark. Besides the already-mentioned top mass and production cross section, the mixing parameter $|V_{tb}|$ was also measured with some precision at FermiLab[14].

3.2.2 Heavy flavours. For the c, and even more for the b, HQET should be reliable. Indeed, good agreement has been found, particularly in the process $B \to D^* l\nu$, which allows for a good determination[16] of cb mixing from CLEO data: $|V_{cb}| = 0.0392 \pm 0.0027(\mathrm{exp}) \pm$

0.0013(th), a clear example of what I meant earlier by a good matching of theoretical and experimental precision.

3.2.3 Quark masses and mixing There have been observations of $B_d^0\bar{B}_d^0$ mixing at LEP, SLD, CDF, ARGUS and CLEO giving an interesting number[19] for the mass difference:

$$\Delta m_d = 0.464 \pm 0.012(\text{stat}) \pm 0.013(\text{syst})\ \text{ps}^{-1}, \tag{3.3}$$

while there are only bounds for $B_s^0\bar{B}_s^0$ mixing from ALEPH, DELPHI AND OPAL:

$$\Delta m_s > 9.2\ \text{ps}^{-1} \tag{3.4}$$

3.2.4 LEP2. I should not pass over the fact that, not long before the start of this Conference, we heard that W-pairs had been punctually observed in each one of the four LEP2 experiments and in a variety of leptonic and hadronic channels. It is hoped that, eventually, this will lead to a determination of M_W with a 30 MeV precision. At present, CDF and D0 (as well as νN DIS data) still provide the best determinations, giving a world average:

$$M_W = 80.356 \pm 0.125\ \text{GeV} \tag{3.5}$$

3.2.5 Status of EW interactions. Most of the discussion about the status of EW interactions[20] was based on the final LEP1 sample consisting of $1.5 \cdot 10^7$ hadronic and $1.7 \cdot 10^6$ leptonic events. The richness of the sample, together with:

- the precise LEP energy calibration,
- the normalization of luminosity via Bhabha scattering,
- the improved understanding of systematics,

has led to a spectacular improvement in our knowledge of the EW parameters.

We can assert that, with LEP1, we have definitely tested the SM at its very roots as a renormalizable QFT by achieving sensitivity to QED and weak radiative corrections. It is worth while noting that, with the increased precision of data and of theoretical calculations, the value of the fine structure constant itself at the Z mass has become an important source of (theoretical) error. Because of uncertainty in the running, $\alpha(M_Z)$ is only known, at present, at the 0.1% level.

Recalling that m_t was estimated to lie in the right range from EW precision data before its actual discovery at Fermilab, we may ask: Will m_H follow the same fate? Unfortunately, EW radiative corrections are only logarithmically sensitive to m_H. Yet, with m_t hopefully known to within 5 GeV from near-future Fermilab measurements, quantities like $\sin^2\theta_W^{eff}$ will have a chance of distinguishing a rather light Higgs from a rather heavy one.

Let us now go over the status of the main observables:

- $M_Z = 91.1865 \pm 0.0013 \pm 0.0015$. By its amazing accuracy, this observable clearly imposes itself as the third reference parameter of the standard EW theory after α and G_F. Thanks to the improvements mentioned before, M_Z is now known with a precision of three parts in 10^5, of which about half comes from the beam-energy calibration (which requires, as we heard, not only knowledge of the tides but also of the Geneva–Paris TGV schedule!). This is really an amazing precision for the mass of a particle, especially considering its width.

- $\Gamma_Z = 2.4946 \pm 0.0021 \pm 0.0017$. If M_Z defines the EW standard model, Γ_Z is crucial in testing it. From Γ_Z^{inv} we can now deduce for the number N_ν of light SM neutrinos:

$$N_\nu = 2.989 \pm 0.012\ . \tag{3.6}$$

- R_b, R_c. Until Warsaw, R_b was the major potential crisis for the SM, hinting perhaps towards low-energy supersymmetry, with R_c supporting the picture as well. The shaking news we heard is that it may have all been “much ado about nothing”. This is mainly due to a new analysis carried out by ALEPH, which uses a double impact-parameter tag for the b jets (one in each hemisphere) as well as four more tags to distinguish b, c and (u, d, s) jets.

  By measuring five single-tag and 15 double-tag rates, both R_b and several efficiencies are estimated. The new ALEPH value:

$$R_b = 0.2158 \pm 0.1\% \tag{3.7}$$

  is on top of the SM prediction... Incidentally, also R_c, which was never that badly off, is back to the SM value. We see from Blondel’s talk that, while the old values for R_b, R_c excluded the SM at 90% confidence level, the new world averages agree with the SM within 2σ. We have to realize, however, that a precise determination of R_b is quite difficult and not completely assumption-free. Under the circumstances, my own conclusion would simply be: if we want to look for a SM crisis, let's look elsewhere!

- A_b. This forward–backward asymmetry parameter appears to be the only observable where something like a 3σ problem can still be present if LEP and LSD data were properly combined. LSD, with polarized beams, directly measures A_b, while at LEP only $A_e \cdot A_b$ can be extracted. Taking both sets of data at face value, one gets:

$$A_b = 0.867 \pm 0.022 \; , \tag{3.8}$$

to be compared with the SM's 0.934. A_e can be converted into a value of 0.23165 ± 0.00024 for $\sin^2\theta_W^{eff}$, which could have interesting implications.

Indeed, from a table of the sensitivity of various observables to the top and Higgs masses[20], it is clear that $\sin^2\theta_W^{eff}$ is one of the most sensitive quantities to m_H (see[21] for a recent theoretical appraisal). This can be seen again in a plot[20], where the overlap of all the constraints defines a "central region" with $90 < m_H < 300$. A Higgs at the lower end of this range would point in the direction of SUSY, while a heavier one would be more akin to the non-supersymmetric SM itself. Unfortunately, this observable is also quite sensitive to $\alpha(M_Z)$, making it important to better determine the running of α.

3.3 Gravity

As already mentioned, gravitational effects are computable in the SM (here meaning General Relativity), but are usually tiny. Fortunately, through extremely accurate clocks, some of the very high precision experiments needed to test the theory have become available[22]. Time-intervals/frequencies have becomethe most accurately known (reproducible) quantities in physics, with one part in 10^{14} becoming quite standard. Although there are not many such tests, they are all quite striking, e.g.:

- the gravitational red-shift, measured via an H-maser on a rocket with a precision of 0.007%,
- the 250 μs delay of the μ-wave signal from the Viking's passage by the Sun,

- the binary pulsar PS1913+16, whose period's rate of change:

$$\dot{P}_b = -(3.2 \pm 0.6) \times 10^{-12}\ \mathrm{s} \cdot \mathrm{s}^{-1}, \tag{3.9}$$

  is in agreement with GR's prediction through energy loss by emission of gravitational waves (GW). This provides the first (indirect) evidence for this crucial prediction of GR. We also know that the speed of GW is the speed of light within 0.1%.

- There have also been claims[23] that the Lense–Thirring effect has been seen using laser-ranged satellites

4 Listening to theory

Since the Standard Model works so well, one should perhaps refrain from asking questions. Theorists, however, are well known for never being happy. Let us consider some of their typical questions:

- How come the lightest "observed" (inverted commas referring to quarks and gluons) fundamental particles have $J = \frac{1}{2}, 1$?
- Why are their mutual interactions described by a gauge theory?
- Why are fermions completely chiral with respect to G_{gauge}?
- Why is there no light ν_R?

For these typical questions, theorists provide typical answers:

- There is a basic difference between scalar, fermion, and gauge boson masses:
  - gauge boson masses are never allowed (by gauge invariance),
  - fermion masses are not allowed if the fermions are completely chiral w.r.t. G_{gauge},
  - bosonic masses are always allowed.
- Even when fermion masses are allowed (say in QCD), there is an approximate, global chiral symmetry protecting their small value from radiative corrections. Following 'tHooft, we express this by saying that fermion masses are "naturally" small (in the above-mentioned precise technical sense for the word "natural").

In order to better understand whether these are indeed good answers to the previous questions, let me digress for a moment on a shift in our attitude towards QFT, which occurred around 1980. The old attitude was that G_{gauge} and R *define* a fundamental theory, which, through renormalization, is made finite and valid at all scales in the limit of infinite cut-off. There is a price to pay for the infinities: any hope of computing a certain number of physical constants is lost forever. Such constants must be taken from experiments (e.g. the three coupling constants of the SM). This still leaves a lot of room for predictivity, as we saw in the previous section.

The more modern attitude consists in saying, more modestly, that QFT is just a low-energy *effective* theory, while the true fundamental theory is a *finite* theory endowed with a physical *finite* cut-off Λ above which QFT is no longer valid. Superstrings today appear as an "existence proof" for such an ultimate theory (see Section 8). In the more fundamental, finite theory, whatever could happen did happen, for instance all particles that could get a mass (e.g. the ν_R) got it.

Starting from this attitude, and invoking the most general principles of quantum mechanics and special relativity, one can convincingly argue [24] that the *effective* theory well below Λ *must* be a renormalizable QFT whose Lagrangian can be expanded in powers of $1/\Lambda$ as:

$$L_{eff}^{tree} = \Sigma c_n \Lambda^{4-n} O_n \ , \tag{4.1}$$

$$L_{eff}^{loop} \sim \text{QFT loops with UV cutoff } \Lambda \tag{4.2}$$

The operators O_n have dimensions $mass^n$ and, following a language borrowed from statistical mechanics, can be classified as follows:

- Relevant, for $n < 4$,
- Marginal, for $n = 4$,
- Irrelevant, for $n > 4$,

where the names come from their relative importance at low energies. Examples of each kind are:

- Relevant: bosonic and fermionic masses. These are so relevant that, if masses are unprotected and become $O(\Lambda)$ after radiative corrections, the corresponding particle decouples at low energy.This explains why we are left with chiral fermions in the low-energy domain.

- Marginal: kinetic terms and gauge couplings. In the absence of the relevant terms I just mentioned, they dominate at low-energy. This "explains" why the low energy theory is a chiral gauge theory!
- Irrelevant: typically operators of dimension 5 or 6. Although suppressed by one or two inverse powers of E/Λ, such terms, if present, can show up by contributing to processes that are otherwise forbidden by relevant and marginal operators ("rare" processes such as proton decay or FCNC). Indeed, various global symmetries (B, L_i for each family) are exactly conserved in the SM if one neglects irrelevant operators.

So far so good..., except for some puzzles:

- Some perfectly allowed marginal operators have not been "seen", in particular the famous Θ-angle term:
$$L_\Theta \sim \Theta_{QCD} \epsilon^{\mu\nu\rho\sigma} F_{\mu\nu} F_{\rho\sigma} \tag{4.3}$$
which contributes to the (not yet observed) electric dipole moment of the neutron.
- Some relevant operators have not been seen either. The most distinguished of them is the (in)famous cosmological constant:
$$L_{cosm} \sim \sqrt{-g}\Lambda_{cosm} \ , \tag{4.4}$$
which, unless infinitesimal (10^{-120} in Planck units!), would not allow our present large, almost flat Universe to exist. The smallness (vanishing?) of the cosmological constant is perhaps the deepest mystery in physics today.

Furthermore, following this new attitude towards QFT, we arrive at a striking conclusion:

The standard model is not sufficient!

Indeed, for any physical observable A, the measured value A_{phys} can be estimated theoretically as:

$$A_{phys} = A_{tree} + A_{loop}(\Lambda) \ , \tag{4.5}$$

where we have indicated explicitly the dependence of the loop correction from the UV cut-off Λ. Since Λ now does have a physical meaning, fine-tuning is strictly forbidden. This means rejecting any ad hoc cancellation between A_{tree} and $A_{loop}(\mathit{\Lambda})$, which would result in $A_{phys} << A_{loop}(\mathit{\Lambda})$.

Let us apply this general argument to the Higgs boson mass. For an elementary scalar particle mass M, in the absence of supersymmetry, we cannot escape the result:

$$M_{loop} \sim g\Lambda \tag{4.6}$$

which clashes, in our philosophy, with the experimental upper limit (from precision tests)

$$m_{H,phys} < O(1 \text{ TeV}) \tag{4.7}$$

unless Λ itself is around the TeV scale. This could mean, for instance, that the Higgs boson is actually non-elementary if studied at the TeV scale (see Lane's talk[25]). In the supersymmetric case, a special cancellation occurs in M_{loop}, so that Λ gets replaced by the supersymmetry-breaking scale M_{SUSY}. In this case, we expect supersymmetry to become manifest around 1 TeV. In any case, new physics is expected below (or around) the TeV scale!

It is perhaps worth while to make a digression here about a couple of "small" conceptual problems within the minimal supersymmetric standard model (MSSM). The first has to do with the fact that SUSY requires (at least) one extra Higgs doublet. The Higgs sector of the MSSM is thus described by the superfields:

$$H_1 : (1, 2, 1), \quad H_2 : (1, 2, -1) \ , \tag{4.8}$$

where, as in (2.3), the numbers indicate the representations of $SU(3)_c \otimes SU(2)_L \otimes U(1)_Y$. The above is obviously a pair of complex conjugate representations (since $2 \sim \bar{2}$). In other words, a (supersymmetric and gauge-invariant) mass term $\mu H_1 H_2$ is perfectly allowed, and we are back to the puzzle of why such a term is not extremely large (the so-called μ-problem). The situation here is only a little better than the one of the Higgs mass in the SM. By postulating a new global symmetry, such a mass term can be made "naturally" small (in the technical sense explained earlier).

The second "problem" is that the automatic (accidental) conservation of B and L_i of the SM is generically lost in the supersymmetric Standard Model (SSM). Some effort has to be made in order to avoid dangerously large FCNC and, actually, FCNC processes at levels close to present experimental limits are almost unescapable.

5 Hints of new physics?

There were a few meager hints of new physics before this Conference. Well, after Warsaw, none of them is a real hint any longer, perhaps with one exception. Let us go quickly through the list:

- R_b, R_c, A_{FB}, see section 3.2.5.
- α_s, see section 3.1.4.
- Large-p_t events at CDF: see section 3.1.5.
- Leptons plus photons plus missing E_t event at CDF[15]. No confirmation from D0. Although the a priori probability of such an event is small, one should not fall into the trap of confusing a priori and a posteriori probabilities: any event that has occurred had zero a priori probability of occurring, of course.
- Four-jet events seen by ALEPH at LEP1.5[20]. The arguments just made apply also to this case. Neither other LEP experiments, nor new data, have confirmed ALEPH's original findings to this date (the situation has evolved after the Conference: at the time of writing, ALEPH finds four-jet events with similar masses even at higher energies, but still without any backing from the other three LEP experiments).
- FCNC. Only upper limits so far, but sensitivity to SUSY-induced FCNC is getting closer[26].
- Searches for SUSY particles: lower limits are getting tighter and tighter [27]. The controversy continues on Farrar's suggestion of a light gluino.
- Excited quarks, leptons: higher and higher limits again.
- ν masses, oscillations: this remains the only area where evidence for new physics has been getting better and better[28],[29] instead of fading away. There are now three claims of "evidence" for oscillations (hence, indirectly, for non-vanishing neutrino masses):
  - Solar neutrinos (Homestake, Kamiokande II, III, Sage and Gallex)

- Atmospheric neutrinos (ν_μ deficit or ν_e excess?)
- LSND experiment at Los Alamos (hopefully to be confirmed by Karmen?)

The most exciting piece of news, however, is that[28] Super Kamiokande started operating at the beginning of April, and is working very well! This means collecting data at a hundred times Kamiokande's rate with checks of day/night and seasonal variations. Solar-model-independent tests of the oscillations thus appear feasible and the overall situation should become clear by the time of the next ICHEP, in two years from now.

To conclude this section I should remark, in all fairness, that much of the above lack of evidence for new physics is not necessarily bad for supersymmetry since, as is often emphasized, the SSM is the only known "gentle" modification of the SM, leaving its predictions unchanged below the scale of SUSY breaking. Yet, one feels a little frustrated about the lack of any "direct" evidence for SUSY and may start worrying about a possible overlook of alternative mechanisms for solving the Higgs-mass problem. After all, before discovering the Higgs mechanism or getting the idea of confinement, theory was at a loss for a complete QFT description of weak or strong interactions, respectively. In conclusion, the "if" appearing in a popular science article last spring: "If supersymmetry is true, we are suddenly going to hit a huge new area of discovery", still remains an "if" after Warsaw.

6 What's up in Quantum Field Theory

6.1 SUSY-breaking scenarios

The actual mechanism of SUSY breaking is closely related to the possibility of discovering supersymmetry. Two scenarios of SUSY breaking, distinguished by who is the carrier of SUSY breaking, are in competition at the moment[30].

For quite some time, people's favourite carrier has been gravity. In this case, in order to have an *effective* scale of SUSY breaking of about 1 TeV, one has to have a much larger scale of SUSY breaking in some "hidden" sector that communicates with us only through gravity. The advantage of this scheme is that, in the presence of gravity, SUSY breaking is quite straightforward. The disadvantage with a high scale of breaking is that parameters have to be fine-tuned very precisely in order to avoid problems (typically, too large FCNC) at low energy.

The new contender is gauge-induced SUSY breaking. Since gauge interactions are typically stronger than gravity, this means a *lower* scale of breaking (for the same 1 TeV effective scale) and less fine-tuning problems. The disadvantage is that the breaking of global SUSY (i.e. breaking in the absence of gravity) is harder (though certainly not impossible) to achieve.

The good news for experimentalists is that the two scenarios are not only theoretically distinct, they also make very different phenomenological predictions, e.g. on the nature of the lightest SUSY particle (LSP) which carries away missing energy and momentum. While in the supergravity scenario the LSP is typically the neutralino, in the gauge-mediated scenario it is the goldstino, the massless particle that originates from the SUSY analog of Goldstone's theorem (the goldstino actually picks up a small mass from gravity and becomes part of the massive gravitino, in the supersymmetric analogue of the Higgs phenomenon, but this is not very important phenomenologically).

6.2 Understanding non-perturbative SUSY dynamics

Turning now to a more theoretical development, I cannot avoid mentioning that recent investigations of $N = 2$ and $N = 1$ SUSY gauge theories have shed entirely new light on non-perturbative phenomena such as confinement and chiral symmetry breaking. I will illustrate the kind of progress that has been made by referring to the already classic paper by Seiberg and Witten [31] in which a complete (non-perturbative) understanding of $N = 2$ SUSY gauge theories was achieved. Consider, for simplicity, just the case of an $SU(2)$, $N = 2$ gauge theory without extra matter multiplets.

This theory has infinitely many ground states corresponding to expectation values of its complex scalar field. Two of these ground states stand out: they are characterized by the presence of massless magnetic monopoles in their particle spectrum. Upon perturbing the system away from the $N = 2$ limit (while preserving $N = 1$ SUSY), only these two special vacua survive and therein monopoles "condense". A magnetic Higgs phenomenon thus occurs, in which electric charges become confined by the *dual* of the Meissner effect in Type2 superconductors. In the latter case, an ordinary Higgs phenomenon (due to Cooperpair condensation) spontaneously breaks the gauge symmetry and confines magnetic-flux lines into thin tubes. The above results, which can be proved thanks to the properties of $N = 2$ SUSY, represent a striking confirmation of the scenario advocated quite a while ago[32] by 't Hooft and others for ordinary QCD confinement.

The $N = 1$ case has been tackled by Seiberg and collaborators[33],extending previous work done in the eighties. SUSY QCD with gauge group $SU(N_C)$ has been studied for a long time for a sufficiently small number N_F of quark/squark flavours. Seiberg et al.[33] have investigated the case in which $N_F > N_C + 2$ and found a correspondence between a strongly coupled electric theory at $N_C + 2 < N_F < 3/2N_C$ and a weakly coupled magnetic one at $3N_C < N_F$. Confinement in the electric theory is understood again as a Higgs phenomenon in the dual (magnetic) description. Finally, for $3/2N_C < N_F < 3N_C$, one predicts a new interacting non-Abelian Coulomb phase. The possibility of extending (some of) these results to actual (i.e. non-supersymmetric) QCD is still being explored.

6.3 Lattice gauge theory

Turning to lattice gauge theories, I should mention the following recent achievements[34]:

- Improvement in the determination of α_s, in agreement now with other determinations, as discussed in section 3.1.4.
- Mass of the lightest 0^{++} glueball at around 1600 MeV, i.e. in the right range for the candidate discussed under 3.1.1.
- Computation of $|V_{ub}|$, an area where HQET is not very useful.
- A new computation of f_B and of f_{B_s}/f_{B_d} giving:

  $$f_B = 175 \pm 25\text{MeV} \ , \tag{6.1}$$

  and

  $$f_{B_s}/f_{B_d} = 1.15(5) \ . \tag{6.2}$$

- Determination of various B-parameters, such as B_{B_d} and B_{B_s}/B_{B_d}.

On a more theoretical level:

- Non-perturbative matching of lattice and $\overline{MS}$ operators, which is crucial in order to relate quantities “measured” on the lattice to those measured in real experiments.

7 What’s up in astroparticle physics/cosmology?

A few subjects would be worth discussing in detail. Because of lack of time, I will limit myself to a few quick comments and refer you to [35] for details:

- CMB anisotropies and cosmological parameters.

  Data on the Cosmic Microwave Background anisotropy and on large-scale stucture are improving fast and will soon tell us much about the value of basic cosmological parameters such as H_0, the present value of Hubble’s constant.

- Microlensing and dark matter

  We heard that not many more microlensing candidates have been found recently. Yet, this remains one of our main windows on the “Dark side of the Universe” as M. Spiro[35] put it. The need for dark matter is still there today, and certainly forces us to think that the SM is not the end of physics…

- Gravitational waves: sources and detectors

  As already mentioned, we have so far only indirect evidence for the existence of GW, a robust prediction of GR. The progress in the field of direct detection, either by laser interferometers or by cryogenic resonating antennas, is slow but constant. According to experts, there is a definite chance that GW will be seen by the turn of the century.

8 What’s up in string theory?

In order to justify my spending some time on this esoteric subject, let me quote a sentence from the 12 July issue of the *Wall Street Journal* (this is not a typo!):

“We have the beginning of a new theory of fundamental physics –string theory– whose full elucidation could be as revolutionary as the discovery of quantum mechanics or relativity”. Now fasten your seat belts!

The main news in this field are related to novel uses of the duality symmetries and to the so-called *D*-Strings and *D*-branes. However, before giving you an idea of what it's all about, I have to recall three basic "miracles" of string theory (see e.g.[36] for a more detailed account).

The *first* miracle is that string theory, unlike field theory, inherits from quantization a fundamental length parameter λ_s. Its inverse,$M_s \sim \lambda_s^{-1}$, plays the role of the finite UV cut-off Λ discussed in Section 4. String theory, being thus free of UV divergences, not only goes over to an effective QFT at energies well below M_s, but also pretends to make sense at energies *above* it.

The *second*, equally crucial, miracle is that the massless spectrum of string theory contains states of all angular momenta up to $2\hbar$. This implies, by standard arguments[24], that gauge and gravitational interactions are automatically present, without having postulated either a gauge or a general covariance principle! Furthermore, gauge and gravitational couplings are unified at the string scale, i.e. Coulomb and Newton forces are automatically of the same order at the scale M_s. This fixes the string scale to be close to (perhaps one order of magnitude below) the Planck mass. Summarizing, we potentially have, in string theory, a finite unified theory of all interactions.

A *third* outstanding property is that QFT's unphysical, uncalculable, unmeasurable bare parameters become, in string theory, the fundamental constants of Nature. Their values are not arbitrary, God-given numbers, they are related to VEVs of fields. Unless they are as such dynamically fixed, there will be unacceptable violations of the equivalence principle through new scalar-mediated long-range forces.

After this telegraphic introduction, let me recall what T-duality is. It has been known for some time that closed strings moving in a space-time containing extra compact dimensions (which is a typical situation in string theory) tend to get… "confused". In particular, they cannot distinguish whether the extra dimension is a circle of radius R or one of radius λ_s^2/R. This property, known as T-duality, has to do with the interchange of *quantized* momentum (as usual, for a circle, in units $\frac{\hbar}{R}$) with the *classical* winding of a closed string aroud the circle (the energy associated with m windings is proportional, through the string tension, to mR). Note the *quantum* nature of the symmetry: there was no symmetry before quantization, since, classically, momentum in a compact direction is still continuous.

At first sight only closed strings can exhibit T-duality, since neither points nor open strings can have non-trivial winding. However, as noticed recently by Polchinski and others [37], even open strings do exibit T-duality, albeit of a new kind: a *conventional* open string moving on a circle of radius R is dual (equivalent) to a *different* kind of open string moving on a circle of radius λ_s^2/R. While the usual open string satisfies so-called Neumann boundary conditions (vanishing normal derivative at the end points), the strings of the dual theory satisfy Dirichlet boundary conditions. The ends of these D-strings (D for Dirichlet) can only move on a surface, defined by fixing some coordinates, which is called a D-membrane, D-brane for short. D-branes can also be seen as solitonic solutions of the low energy field equations.

Fine, but what do we get from all this game? At least two applications have emerged:

- Certain D-branes represent (limiting cases of) black holes. Now, the origin of the famous Beckenstein–Hawking formula for the entropy of a black hole:

  $$S_{bh} = \frac{A}{4l_P^2}\ , \tag{8.1}$$

  where A is the area of the event horizon and l_P is the Planck length, has long been a mystery. The exponential of the entropy should be related to the number of *microscopic* quantum states, giving a definite set of *macroscopic* quantum numbers, i.e. mass, charge, angular momentum of the black hole. So far nobody had been able to identify such microsopic states, let alone counting them.

  D-branes were shown to provide explicit examples of how that counting works. This could mean making progress in the near future on the famous information paradox of black holes the (apparent?) loss of quantum coherence when a pure quantum state collapses into a (thermically radiating) black hole, which eventually evaporates completely. Steven Hawking, who is the most convinced advocate of loss of coherence, looked nervous about the D-brane breakthrough at a talk he gave at Oxford last June...

- Until recently, there were five distinct –but equally consistent– superstring theories, which looked to be completely disconnected from one another. They were (and still are) going under the names of :

  $$TypeI,\ TypeIIA,\ TypeIIB,$$

$$Het(SO(32)),\ Het(E_8 \otimes E_8)\ . \tag{8.2}$$

It has become increasingly clear that these theories are actually related by duality symmetries. Some of these are of the T-type mentioned above, others, going under the name of S-duality, relate the strong-coupling limit of one theory to the weak-coupling limit of another[38]. This is similar to the case of the electric–magnetic duality of Maxwell's theory in the presence of both electric and magnetic charges (monopoles) satisfying Dirac's quantization condition:

$$q \cdot m = 2\pi n\ . \tag{8.3}$$

D-branes have also played a very useful role in establishing these links. As a result, the long-forgotten 11-dimensional supergravity (with a compact 11th dimension) has made an impressive come-back, reinterpreted as the strong-coupling low-energy limit of some 10-dimensional string theories. It is actually believed that a new 11-dimensional "M theory" should exist, which would be the "mother" of all known string theories according to a remarkable "genealogical" tree[38].

9 Conclusions

- The standard model's health, already excellent, keeps improving, as it gets rid of little colds (R_b) or of small head–aches (4jet events?). However:
- There will be no lack of very interesting new data in the years to come, and we are all eagerly waiting for both the expected and (even more so) the unexpected.
- Belief in SUSY appears unshakable in the theory community, and even dangerously contagious for experimentalists. Is it instead something completely unthought of that keeps m_H small?
- Particle physics has grown to finally encompass accelerator and non-accelerator experimental physics in an overall common effort.

- Similarly, understanding the mysteries of Astrophysics, Cosmology, and Quantum Gravity has become an integral part of our theoretical endeavour.

And indeed I wish to conclude my talk with a plea. If our field is to keep thriving, we cannot afford the luxury of ignoring *any* relevant scientific input, wherever it may come from, LEP , HERA, COBE, LIGO…or a conceptual problem in quantum gravity. We have to work hand in hand, theorists and experimentalists, accelerator and astro physicists, stressing to ourselves –and to the public opinion, whose moral and material support we seek– the

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where, to avoid any misunderstanding, I should stress that the word "fundamental" (like the word "natural" earlier on) is to be understood

in a technical sense,

that would take me too long to explain.

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