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Directions in High Energy Physics

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Abstract

The future goals of particle physics are classified from a theorist's point of view. The prospects of mass and mixing angle determination and of the top quark and Higgs boson discovery are discussed. It is shown that the most important progress will come from LHC and NLC. These machines should be planned and developed as quickly as possible.

1. Introduction

Physics is the science in which matter and ratio are most tightly connected. High energy physics is its most fundamental branch because we try to find ever smaller constituents of matter. It was born from nuclear physics as higher energies became available. Its goal has always been to discover new particles and determine their properties in high energy collisions. The properties to be determined are mass, quantum numbers and couplings to other particles. When it comes to still higher energies further questions can be examined, like compositeness or more fundamental symmetries. Finally, a complete theoretical model for the particle can be developed, based in the best cases on simple and fundamental mathematical principles.

I am sure that this concept of high energy physics will successfully persist in the future although new experiments become increasingly expensive. Most important items are the discovery and examination of the top quark and of the Higgs particle because this will open new frontiers in our understanding of the fundamental interactions. It would be desastrous if we would wait and withdraw to cheaper but less important projects. Less important projects also cost money, bind energy and, most of all, they distort the direction of research. I can see a tendency for this in some recent decisions and I want to warn that this could jeopardize the future of particle physics. Once money is lost in wrong directions it is difficult to attain new money, even for important experiments. It is the main aim of this article to discuss these issues in detail and to rate the various projects according to their priority.

A standard popular objection to basic science is that the number of physical laws is limited and most of them are already known. I cannot see that this is an argument against high energy physics. As will be discussed in the next section it is absolutely clear that the deepest physical laws have not yet been found. In fact, the next section will start with a summary for and against the so-called standard model of elementary particle physics. Afterwards, a set of experiments as suggested by the standard model will be valuated. We shall see that the heavy particles of the standard model are at the centre of interest and should be treated with the highest priority. Finally, I shall come to physics beyond the standard model and to the question how reasonable certain specific null experiments are which search for deviations from the standard model. Usually, the theoretical ideas on which such experiments are based are rather exciting but the performance is boring when nothing is found.

Note that this paper is not intended as an exhaustive review but as a grading of present and future high energy physics experiments. Its aim is to initiate discussions about the future directions and to sharpen the view for what is important and what not.

The known elementary particles do not form a very complex system. As compared to biological systems, for example, they are remarkably simple. Complex systems are always multiparticle character whereas high energy physics is eventually searching for the most fundamental entities and their interactions and is thus different from other branches of science like chemistry, astrophysics or even mathematics. This should be kept in mind when the significance of experimental and theoretical ideas is reflected.

2. The standard model

The standard model of elementary particle physics describes the interactions of the quarks and leptons as mediated by the vector bosons of the strong (gluons), weak ($W^{\pm}, Z$) and electromagnetic (photon) interactions, c.f. fig. 1. Quarks and leptons are fermions (= spin$\frac{1}{2}$ particles) whereas the vector bosons have spin 1. Finally, there is the Higgs boson, a spin 0 particle, which is associated with the generation of all particle masses. It is certainly the most mysterious object of the standard model because it is as yet undiscovered and relies on a purely theoretical construction about symmetry breaking. Once discovered, the nature of the Higgs particle will be established by demonstrating that its couplings to other particles grow with their masses.

The standard model has been very successful phenomenologically. I know of no experiment which definitely contradicts the standard model. However, it is not believed to be the ultimate theory of nature, because it has too many free parameters, namely, all the fermion masses. The standard model describes particles whose masses range between 0 (neutrinos and photon) and 100-1000 GeV and one would like to understand better, how these vastly different masses arise and whether there are other important mass scales at higher energies. Some related questions are: the masslessness of the neutrino and the stability of the proton. These items will be discussed in later sections.

The gauge sector of the standard model is remarkably clear and extremely restricted, with only 3 dimensionless coupling constants describing the interactions of the vec-

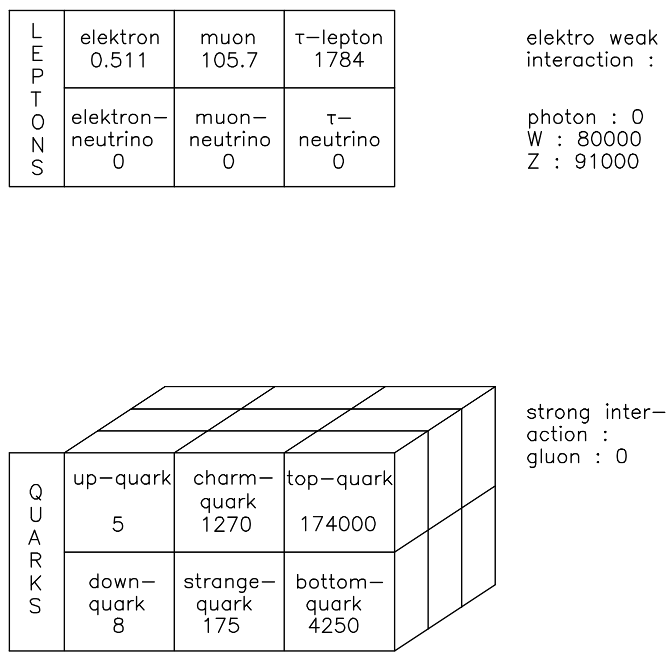


Figure 1:

tor bosons with themselves and with the fermions. Through this one was able to predict many properties of c-, b- and t-quark and the τ-lepton long before these particles were actually discovered. It is even possible that the standard model gauge group is embedded in a larger more simply connected group. This is the “Grand Unification” scenario first suggested by Georgi and Glashow.

3. Vectorboson self couplings from LEP1 and LEP2

The direct self coupling of vector bosons is a firm and pronounced prediction of nonabelian gauge theories. It makes the behavior of the gluon, the W and the Z much different from that of the photon. For example, a large amount of the 4-jet cross section in e^+e^- annihilation should come from processes in which one gluon splits into two (fig. 2). This effect can quantitatively be tested because the LEP1 experiment has reached a rather high precision concerning its 4-jet cross section. On the theoretical side the calculation of higher order corrections to $d\sigma_{\rm 4jet}$ is still missing. This is unfortunate because this quantity offers the cleanest possibility to check the triple gluon coupling. Information can also be obtained from proton collisions – but with much higher uncertainty (due to our ignorance of the gluon

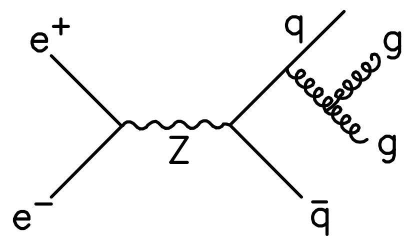


Figure 2:

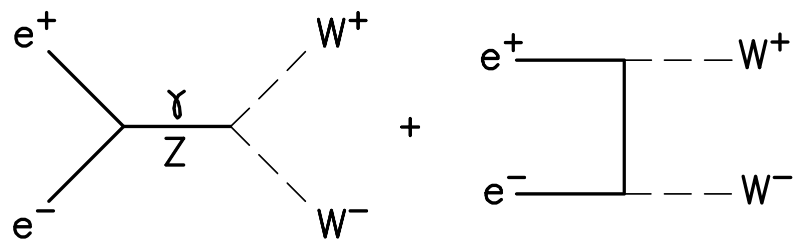


Figure 3:

distribution at small x).

In spite of the high precision of the LEP1 experiment the 3-gluon self coupling will always remain a little in the dark. This is because of the usual problem of QCD, the washing out by hadronization effects. In this respect the situation is better for the ZW^+W^- interaction which will be studied at LEP2 [1] via the process $e^+e^- \to W^+W^-$ (fig. 3). I consider this experiment very important. It is a fundamental experiment, in the sense that one can hope for deviations from the standard model, and it must be made by all means to fix once and for all any doubts which are left concerning the description of the standard model gauge sector. It can be shown that the sensitivity of the experiments to the ZW^+W^- coupling increases with energy so that an e^+e^- machine at total energy 500 GeV would be really superior to LEP2 ($\sim$ 200 GeV). This is a first strong argument for an e^+e^- collider at ultrahigh energies whose prospects will be discussed in section 7.

4. Determination of parton densities at HERA

Proton collisions play an eminant role for the discovery of new particles. However, cross section predictions for proton colliders are plagued by a number of uncertainties, in particular concerning our ignorance of the internal structure of the proton. Deep inelastic lepton nucleon scattering can be reliably used to determine the structure of the proton in terms of its quark and gluon constitutents, i.e. the parton distribution functions. If these functions are determined accurately, one can obtain quantitative predictions for all sorts of cross sections in proton proton collisions, like top quark or Higgs boson production or the production of exotic particles. At LHC with its high beam energy the behavior of the parton distributions at small x becomes essential because partons with a relatively small amount x of the proton momentum can produce the new particles. The dominant parton distribution at small x is the gluon distribution. The HERA experiment at DESY is able to measure in the small x region ($x \sim 10^{-3}$ to 10^{-2}) whereas pre-HERA data on deep inelastic scattering could not resolve the gluon density function at small x. In this sense HERA is a very reasonable experiment whose result will be used until the next century. Unfortunately, the theoretical handling of the very small x ($< 10^{-3}$) HERA data is not fully clear. At very small x, there are nonperturbative gluonic effects besides the perturbative ones. A complete theoretical picture of how to combine these effects is still missing.

It should be stressed that the determination of parton densities is the only important fundamental contribution which can be expected from HERA. Certainly, a lot of interesting physics like photoproduction, jets or heavy quarks can be done and I don't want to lessen their merits. But the real justification for the large and expensive HERA project comes from the structure function determination. It is very important that this is done as precisely as possible.

It should also be stressed that HERA is completely a standard model machine. It is unlikely that nonstandard physics will be found at this experiment.

5. Light fermion masses and CKM matrix elements

The masses of the standard model particles can be deduced from experimental observations and at present cannot be explained theoretically. Due to QCD effects, the quark masses are much less accurately known than the lepton masses. In fact, there is still a discussion going on about the values of the light quark masses m_u, m_d and m_s, and it is questionable whether they can ever be determined to better that 20 %.

An additional complication arises in the quark sector because the various flavors mix, i.e. the physical “mass” eigenstates are different from the interaction eigenstates. As a consequence, a nondiagonal 3×3 matrix V appears in the charged current. V is called the CKM matrix and it can be shown theoretically that it has four independent parameters of physical significance. A lot of physicists in various experiments work on their experimental determination. It is reasonable to say that rather precise values for all four parameters will be available around the beginning of the next century. Very precise information will come from the two “b-factories” [2] built in America and Japan which use the advantages of the process $e^+e^- \rightarrow b\bar{b}$ to determine $V_{ub}, V_{cb}, m_b, m_c, \ldots$. This is an important issue because the masses and mixing angles are the largest subset of free parameters in the standard model and should be determined very precisely. It will be the basis for future physical analyses, theoretically and experimentally, within and beyond the standard model.

However, I do not understand why two (or even three?) b-factories are built where one would be sufficient. The political explanation for this wrong decision is that in the various countries one is afraid to approve very large projects (like the NLC). I am sure that in the long run this will prove harmful because it misdirects tight ressources and could even jeopardize the future of high energy physics.

6. LHC, the top quark and the Higgs boson

After the cancellation of the SSC the LHC (“Large Hadron Collider”) at CERN [3] is the only remaining high energy proton collider project. It will operate at an energy of about 16 TeV and will allow to discover the Higgs particle as well as solidly establish the existence of the top quark. The machine also provides unique opportunities to search for new heavy particles as predicted by theories beyond the standard model. The LHC will probably become one of the most rewarding project in the history of high energy physics.

The $\bar{p}p$ colliders which have operated in the last decade at CERN and Fermilab have been very successful in discovering heavy fundamental particles (the W and Z boson and the top quark). The LHC with its much higher energy will continue this success because it allows to produce a large number of top quarks and even Higgs bosons so that the decay properties of these particles can nicely be studied. Furthermore, as compared to high energy e^+e^- machines it has a better capacity

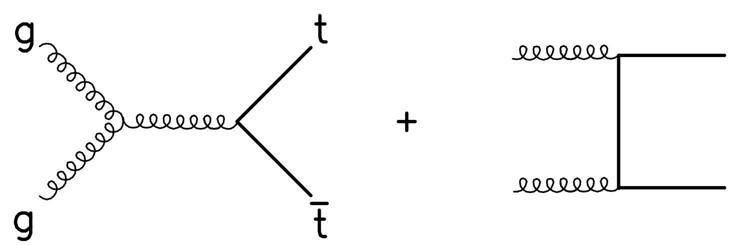


Figure 4:

to produce and discover very heavy particles with masses of order 1 TeV. Heavy particles, within and beyond the standard model, offer the best possibility to open really new frontiers in physics, because they have never been produced on earth and some suprise is likely to arise when they are discovered and examined. Therefore the LHC project is of extreme importance for the future of physics.

It is not so bad that the SSC plans have been cancelled because the physics potential of LHC and SSC is rather similar. Furthermore, LHC costs are much smaller than SSC, because the experiment will take place in an existing tunnel (the LEP tunnel).

The main production mechanism of top quarks at LHC is gluon-gluon fusion $gg \to t\bar{t}$ (fig. 4). Estimates of cross sections are shown in fig. 5. They correspond to a number of about 100000 top quarks to be created per year (for a top mass of 175 GeV). The main uncertainty in these cross section estimates arises from ignorance about the parton distributions.

The main decay mode ($> 99.8\%$) of the top quark is $t \to b + W^+$ and it leads to a width of about 1.5 GeV (see fig. 6). The semileptonic branching ratio is $BR(t \to \mu^+) = \frac{1}{9}$ leading to the emission of clearly visible hard isolated leptons. Additional leptons from semileptonic b-quark decays are softer and non-isolated, i.e. associated with hadronic jets.

Within the standard model, the only parameter for the top quark to be fixed is the mass. It can be determined by studying the invariant mass distribution of the jets recoiling against the lepton in one-lepton events. This way a top quark mass value of about (175 ± 20) GeV has been determined at the Tevatron and an error of 1 GeV or less can be envisaged at LHC. Beyond the standard model there is a variety of possibilities, like deviations from the V-A coupling, effects of charged Higgs bosons in top decay, supersymmetry etc. which can all be searched for.

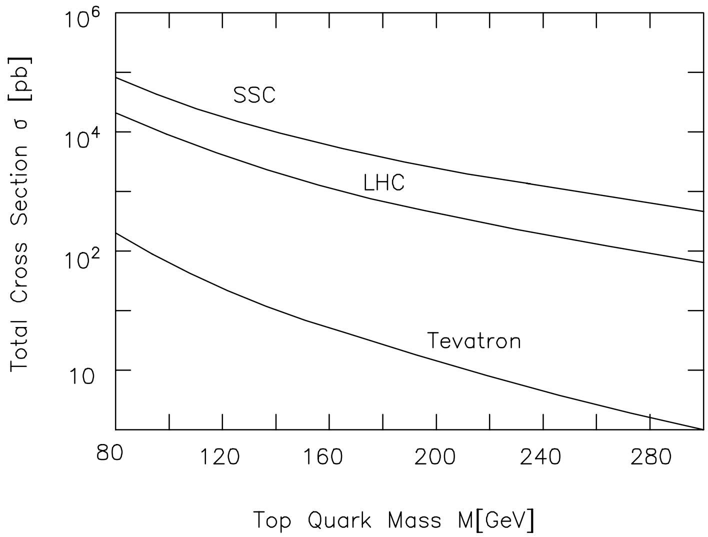


Fig. 8

Figure 5:

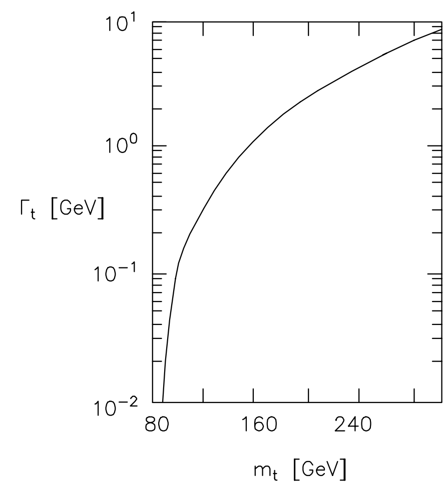
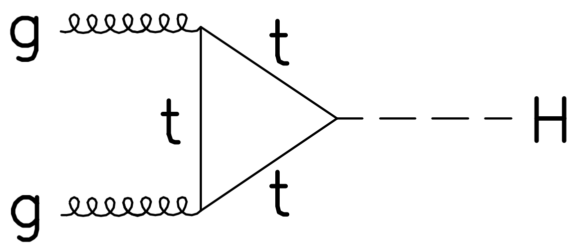


Fig. 5

Figure 6:



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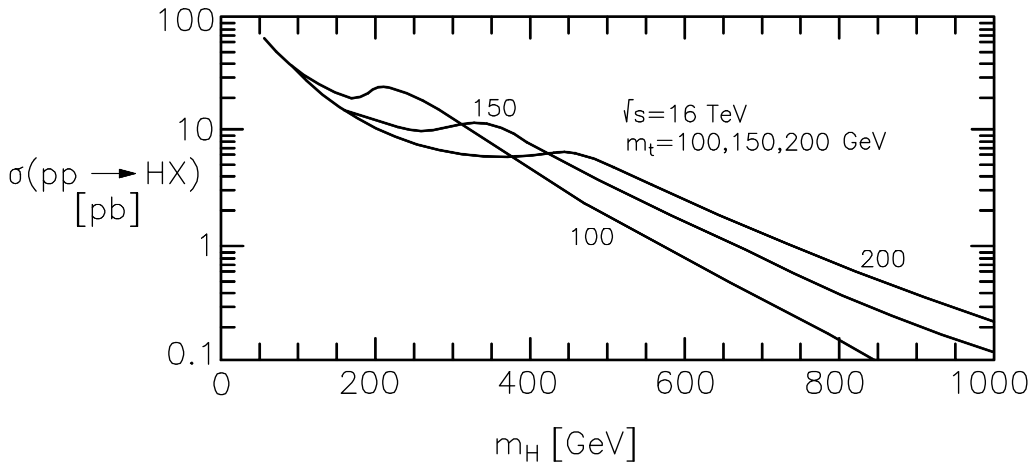


Figure 8:

The main production mechanism of the Higgs boson at LHC is the fusion mechanism $gg \to H$ with an intermediate top quark triangle (fig. 7). Estimates of the cross section are shown in fig. 8. We see that the cross section is typically a factor of 100 smaller than for the top quark, but still a lot of Higgs particles should be produced at LHC.

The main decay modes of the Higgs boson are $H \to b\bar{b}$ resp. WW/ZZ depending whether $m_H < 2m_W$ or $> 2m_W$ and they lead to a width shown in fig. 9. Due to large backgrounds from ordinary $b\bar{b}$ or WW/ZZ events the search for the Higgs boson at LHC is difficult. – But it is not hopeless because it should be possible to see the Higgs events as bumps on invariant mass distributions for those processes. A Higgs boson with mass of 0 (100 GeV) seems to be particularly difficult to detect because of the large $b\bar{b}$ background. I am optimistic that even that case is tractable because proton collisions are usually more successful than anticipated.

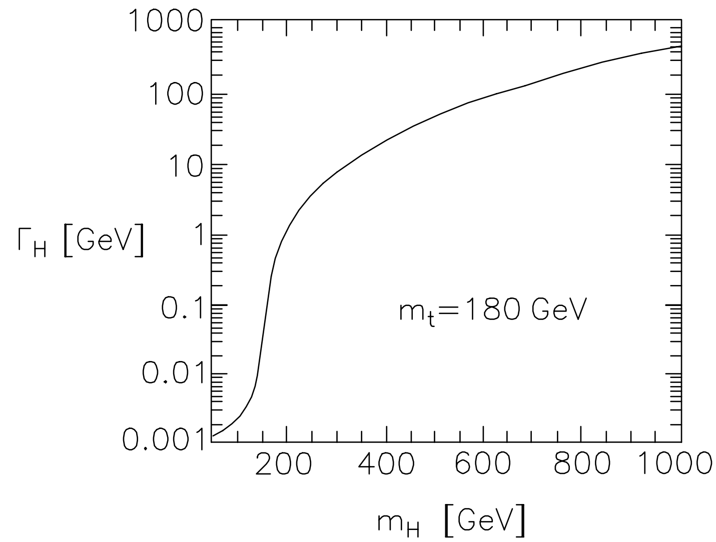


Figure 9:

Within the standard model the only parameter for the Higgs search experiment to be fixed is the Higgs mass. m_H is unknown but should be well below 1 TeV, for theoretical reasons. Furthermore, LEP1 results restrict m_H to $m_H > 65$ GeV. At LHC, m_H can be determined from the location of the bump in those invariant mass distributions.

Together with the Fermi constant the Higgs mass completely fixes the form of the Higgs potential in the standard model. It is very important to know m_H and to check the high energy tail of the standard model. Of course, more than one Higgs field and correspondingly more free parameters in the Higgs potential would complicate the situation. In this case the NLC (next linear e^+e^- collider) would be extremely useful for clarification.

7. NLC, t and H

The next linear e^+e^- collider (NLC) [4] expected to operate in the energy range between 300 and 800 GeV will allow to perform very precise studies of the heavy particles in the standard model, the top quark, the electroweak bosons and the Higgs particle. The machine will also provide unique opportunity for new physics searches.

The e^+e^- colliders which have operated over the past two decades have been outstandingly successful in exploring the fundamental interactions and constituents of matter. The charm quark, the τ lepton, the gluon and the bottom quark were discovered and established by SPEAR, PEP1 and PETRA, and their properties could be studied in a clean experimental environment. Later on, the precision analysis of the Z-boson and its decay modes at LEP has established the validity of the standard model to a very high level of accuracy.

The next generation of e^+e^- colliders will undoubtedly continue this success story and reveal much about the properties of the Higgs boson and the top quark. As compared to a high energy proton collider the production of these particles in e^+e^- annihilation can be studied at a much higher level of precision. This way the standard model predictions can be nicely checked and possible deviations from the standard model could be established. I have in mind here the pointlike $V-A$ couplings of the top quark and the masslike couplings of the Higgs boson, and, more in general, a precise examination of the top Higgs connection, symmetry breaking mechanism etc.

In my opinion the NLC is an absolutely necessary project to complement the LHC. No time should be wasted to start on. As time goes by, with smaller projects being approved, we might be tempted to concentrate on them and withdraw from NLC and its important questions. This would be desastrous because it would jeopardize the future of particle physics.

The Higgs boson has not been found at LEP1. From this fact a lower limit $m_H >$ 65 GeV can be deduced. At LEP2 and NLC the m_H range up to 90 GeV, resp. O(200 GeV) will be covered using the Higgs-strahlung process $e^+e^- \to ZH$ (fig. 10). This can be deduced from the production cross section fig. 11. NLC will be an ideal laboratory for the discovery of an "intermediate" O(100 GeV) mass Higgs field because it would show up as a spectacular peak in the distribution of the recoil mass of the Higgs-strahlung process. After the discovery the properties of the Higgs boson can be accurately determined and many informations not available at LHC will be obtained. This will be the basis for further tests of the standard model Higgs sector.

Top Physics is also very interesting at the NLC. From the behavior of the integrated cross section $\sigma(e^+e^- \to t\bar{t})$ in the threshold region one will be able to obtain an extremely accurate value of the top quark mass (see fig. 12). Now, m_t is not a prediction of the standard model but the couplings of the top quark to the vec-

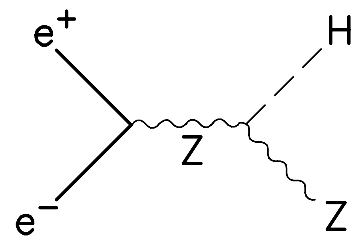
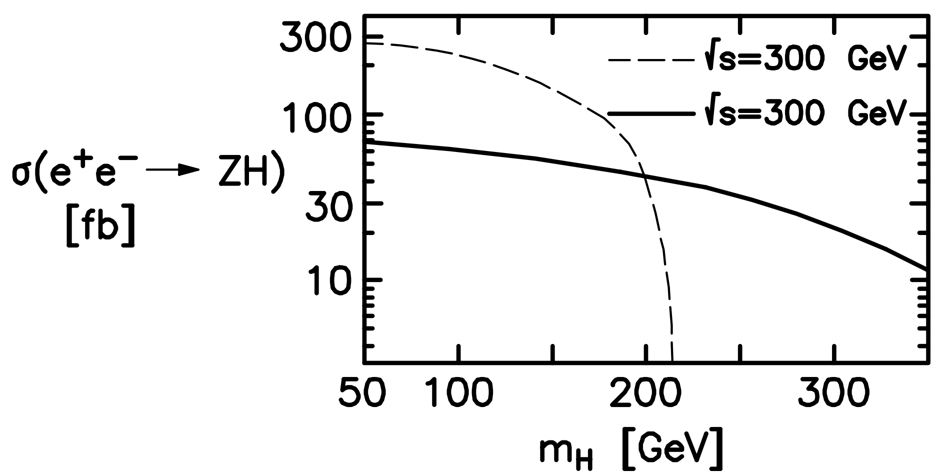


Figure 10:



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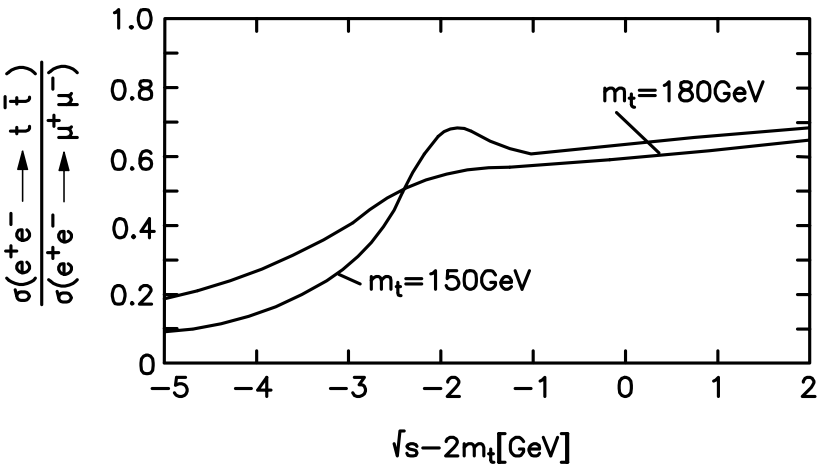

Figure 12:

tor bosons (W, Z, γ) are. These couplings can be determined very precisely from differential distributions of the $t\bar{t}$ decay products so that a high level check of the standard model will be possible.

In summary, I would say that the NLC is the most interesting high energy physics project for the next century. The problems related to the NLC are not on the physical but on the technological and financial side. It is not fully clear whether e^+e^- beam energies of 500 GeV can technically be reached but it will be worth-while to overcome these difficulties because the physics prospects are really fantastic.

8. Physics beyond the standard model

Theoretical developments beyond the standard model are usually in a rather weak position because of the lack of any experimental indication. The most common are extensions of the standard model, in the sense that these theories have a slightly enlarged symmetry or particle spectrum. For example, grand unified theories (GUT) are based on gauge groups like SU_5 which contain the standard model gauge groups $SU_3 \times SU_2 \times U_1$ as subgroup. Consequently, the three running coupling constants g_3, g_2 and g_1 should converge to a common value g_5 at some large "grand unification" scale $M_X \approx 10^{14}$ GeV. The measured values of g_3, g_2 and g_1 seem to indicate that this may happen only if there is additional new physics at scales $\Lambda \ll M_X$, let's say $\Lambda \sim$ O(1 TeV) which would modify the evolution of $g_{3,2,1}$ in the right way corresponding to somewhat larger scales $M_X \approx 10^{16}$ GeV. Many theorists believe that this is the so-called supersymmetry which would show up in the form that to each existing particle a "supersymmetric" partner with spin shifted by $\pm\frac{1}{2}$ and mass of order 1 TeV exists. Personally, I see no compelling reason to believe specifically in the supersymmetric scenario. Nevertheless, it is an important issue to go to energies in the TeV range and try to find new particles there. Unfortunately, the GUT ideas do not give conclusive evidence whether the new physics scale is 1 TeV, 10 TeV or even 1000 TeV.It is not at all clear whether experiments at energies larger than 100 TeV will ever be made. This is a question of the far future which I cannot answer. They should be made, if they are technically and financially possible.

A specific low energy prediction of GUT's is proton decay. It arises through the existence of new superheavy SU_5 gauge bosons, of mass M_X. The exchange of virtual SU_5 gauge bosons induces baryon number violating processes, like $p \to e^+\pi^o$. As compared to normal weak boson exchange processes they are suppressed by a factor $(M_W/M_X)^4$.

Proton decay experiments have been made in the last two decades with increasing effort and without success. From the present limit on the proton lifetime, $\tau_P \geq 10^{34}$ years one can deduce a limit on the grand unification scale $M_X \geq 10^{15}$ GeV [5]. Correspondingly, supersymmetric GUT's are in a somewhat better shape than standard GUT's although I would not call this evidence for supersymmetry.

More in general, it is quite difficult to tell where nonstandard physics will first be seen. It can either appear in the form of new unexpected particles or in the form of unexpected behavior of one of the known particles. Up to now, all experiments in these directions have turned out to be rather frustrating because they are null experiments looking for tiny deviations of the standard model (e.g. proton decay, neutrino masses etc.). From a theoretical point of view high energy experiments are generally superior to low energies because the effect of new physics typically grows like a power of $\frac{E}{\Lambda}$, E being the energy involved. This is a strong argument for all ultrahigh energy colliders, in addition to top quark and Higgs physics.

Within the standard model, the Higgs boson is the most speculative particle. In my opinion, it is possible that it does not exist at all, so that the standard model would have to be replaced by another theory with different high energy phenomenology. In any case, once the Higgs particle is found, large deviation from the standard model might quickly show up (in the form of several Higgs fields or else). As discussed before these could be studied in high energy collisions.

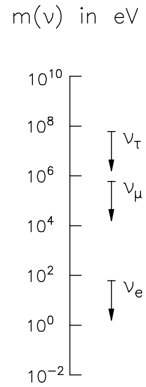

Figure 13:

Together with the top quark and the vector bosons the Higgs field is the heaviest of the known particles (with masses of order 100 GeV). Within the standard model all these particles are pointlike. In my opinion it is difficult to imagine how particles as heavy as a large nucleus can be pointlike to the same extend than the light fermions (electron and up and down quark). Although I cannot prove it, I am expecting deviations from pointlike behavior to be seen at the level of TeV energy experiments. This view is supported by the fact that m_t and m_H are of the same order of magnitude as the new physics scale $\Lambda \sim O$ (1 TeV) mentioned above. If new physics exists at scales of order 1 TeV, this will be seen first by the experimental analysis of top quark and Higgs boson properties.

In summary, all theoretical developments beyond the standard model are not very definite. Experimenters are well advised to keep their eyes open for a wide variety of possibilities. High energy precision experiments at LHC and NLC seem to be most promising.

9. Neutrino masses and mixings

Experimentally, all neutrino masses are tiny, if not zero. The present experimental upper bounds for the 3 neutrino species are given in fig. 13.

In the standard model all neutrinos are massless. However, there is no fundamental theoretical reason for that and, in fact, the standard model can easily be extended to include small neutrino masses. These masses can be either of Dirac or of Majorana type, because the neutrino is electrically neutral. The main question from the theoretical point of view is to understand the small masses of neutrinos as compared to other fermions. As yet no real answer to this question has been given. Therefore, neutrino masses may have any value, from extremely tiny to the upper limits given in fig. 13. The ignorance about m_ν may be parametrized in the form of $m_\nu = \frac{m^2}{M}$ where m is a typical fermion mass of order GeV and M a large unknown scale.

Very small neutrino masses cannot be determined directly but show up in the form of oscillations between the various neutrino species. Thus a large number of experiments searching for $\nu_e - \nu_\mu$ oscillations has been done, sensitive to neutrino mass differences down to the eV range. However, the present experiments provide no evidence for neutrino masses. In addition, astrophysical observations and cosmological considerations have led to no conclusion about neutrino masses which I would take seriously.

There is only one statement about neutrino masses which I consider most probably true: namely, if the neutrinos have masses, the τ-neutrino will be the neutrino with the largest mass. Any experiment which aims at $m(\nu_\tau)$ should have priority to other neutrino-experiments. Thus NOMAD and CHORUS at CERN and P803 at Fermilab are important and reasonable projects [6].

10. Conclusions

Since I have a clearcut message I will make my conclusions very short. We have seen that there are several interesting items in future elementary particle physics. By and large, one can be content with the direction high energy physics takes. Many interesting experiments (HERA, LEP2, LHC, P803, ...) are under way and the SSC cancellation is not unwise because most of the SSC physics will be covered by LHC, at a much lower price. However, I have objections at certain specific points. For example, one should not build two b-factories where one machine would be enough. Furthermore, I do not understand the widely spread attitude of hesitation towards the really new large projects, like the NLC.

Among all the items discussed, I would classify only one as being *extremely* urgent and important. This is the question about the heavy sector of the standard model, i.e. the top quark and the Higgs boson. I am quite sure that in the behavior of these particles physics beyond the standard model will show up and that one can get insight into more fundamental laws of nature. Therefore every effort should be made to study top quark and Higgs boson precisely, by means of LHC and NLC. This is certainly a very expensive program. However, I see no reason to wait and to do less important but cheaper experiments. I may not be fully objective here and admit that I am impatient. I am impatient to learn everything about the smallest distances, and I passionately believe that the real problems are still in front of us.

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