A GENERAL INTRODUCTION TO TEVATRON II PHYSICS

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Before the initial round of beams and detectors for the Fixed Target Tevatron Program (which will sometimes be called TeV II in the following text) are firmly determined, it is useful to look more closely at what important physics questions can be addressed there. The workshop whose results are summarized in the following papers was arranged for that purpose. The goal was to go beyond broad statements such as "Test QCD" to give detailed descriptions of significant physics problems which are in the realm of TeV II.

Whenever a new accelerator facility is turned on, it may be that its main contribution will be in surprising areas that no one has thought of. Although that is certainly possible, it is considerably less likely today, now that QCD may be the theory of strong interactions and SU(2)×U(1) the theory of electroweak interactions. Both of these have been formulated and partially tested in the energy range available to TeV II, and it is perhaps even probable that they will remain valid there. One important role of TeV II will be to further test these theories. On the other hand, there are fundamental unsolved problems in particle physics today, and there are a number of ways in which breakthroughs in providing experimental input to grand unification, the flavor problem, and spontaneous symmetry breaking could come from TeV II.

From the perspective of possible experimental input to solving fundamental problems in particle physics one might list the main problems as:

1. Is QCD really the correct theory of strong interactions? Further tests are needed to confirm every aspect of QCD predictions. Is experimental input useful for soft QCD, for helping to solve the confinement problem, and for deciding if quarks and gluons interacting via QCD can account for the observed hadrons and their interactions?

2. Is SU(2)×U(1) really the correct electroweak theory?

3. What is the physics of spontaneous symmetry breaking? Are there fundamental Higgs bosons, or is there dynamical symmetry breaking giving composite bosons, or perhaps no particle states below the TeV scale?

4. Is there a grand unification of QCD and SU(2)×U(1)? What is it?

5. Why are there several families of quarks and leptons? How many? Are they really copies or do heavier families show some different properties?

6. Are there unexpected discoveries to make? These could come in two kinds. First, there could be truly unexpected findings, such as a fourth family, heavy neutral leptons, light colored Higgs, and (obviously) unspecifiable things. Second, there could be results that fit within the framework of the theories we have. Is the weak isospin eigenvalue of the right-handed muon really zero? Are the charged currents all really V-A? Many of these kinds of questions can be checked at TeV II.

For the rest of this summary I list a number of questions which provided the lines along which the working groups were organized. Some of these questions were considered by them (as well as many others) and some have not yet been considered in detail.

ν Masses and Oscillations

Whether ν masses are zero, and their values if they are not, will tell us a great deal about grand unification and theories in which lepton number is not conserved, as well as about cosmology. Present experiments are suggestive of effects of non-zero masses and are stimulating much more work; theoretical arguments have been discussed for several years.

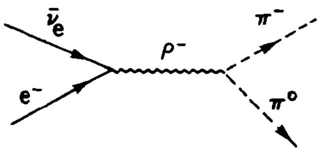
At TeV II, the effects of ν masses would show up as oscillations from one ν into others. The probability of an effect is proportional to

$$\sin^2 2\alpha \sin^2 \frac{(m_{\nu}{}^{2} - m_{\nu}{}^{2})L}{2E} .$$

At TeV II only mass differences $\gtrsim$ 10 eV are likely to be observable. This is an attractive range as it can arise in theories and is the range needed if neutrino masses are relevant for solving cosmology problems (see recent work of E. Witten).

At the same time, the mixing angles α might be expected to be rather small, e.g., $\alpha \lesssim \theta_c$, so $\sin^2 2\alpha \lesssim 0.1$. Many experiments cannot deal with such a small effect, and no present oscillation data touch this range.

One amusing possibility for an experiment which would be unique to TeV II and might be able to see a small effect is to take advantage of the well-known result that at E_ν = 600 GeV one has $s = m_\rho^2$ for $\bar{\nu}_e e$ scattering, so that the ρ resonance is formed in the center of mass.



This only occurs for $\bar{\nu}_e$ in the standard theory (which allows a sensitive test for lepton number violation by checking if it happens for $\bar{\nu}_\mu$, ν_μ, ν_e). Thus by starting with beams with little $\bar{\nu}_e$ contamination and looking for some $\bar{\nu}_e$ by finding a ρ^- in the center of mass one has a sensitive probe. The peak cross section is about 1.5×10^{-38} cm^2; it is enhanced from the off-resonance contribution by about $(m_\rho/\Gamma_\rho)^2 \simeq 30$. Since the peak cross section is known it also can serve to normalize beams.

ν_τ

The question of the existence of ν_τ is, in a sense, especially important. If the existing SU(2)×U(1) theory is right it is already known, from the absence of $\tau \to eee$, $\tau \to \mu ee$, $\tau \to \mu\mu\mu$, $\tau \to e\mu\mu$ at the few per cent level, that τ must have its own light ν (see Alterelli et al., Horn and Ross, Fritzsch, 1977). So the absence of a ν_τ would have an extraordinary impact on the theory, essentially establishing that SU(2)×U(1) was not valid for the 3rd family. Since τ decays are as expected in the theory, that is very unlikely.

It is very important to work toward checking that the charged and neutral current ν_τ interactions are as expected. While the neutral current may not be separable from others, the τ charged current may be reasonable for TeV II study in later experiments.

Quark Mixing Angles

The eigenstates of weak interactions are not the same as the quark mass eigenstates, so there are mixing angles. There are fundamental parameters like the Cabibbo angle (which is one of them) that need measurement. From the b-quark lifetime, its decays, and its production, some mixing angle measurements can be made. Although production of a t-quark may be difficult, it would be easy if mixing angles were large, so limits on t production in ν reactions could provide useful limits on the angles.

CP Violation

Does TeV II allow experiments to study the origin of CP violation that are as useful as those at other machines, or more useful?

t-Quark

Apparently a t-quark is not seen at PETRA. Before TeV II it will be known definitely from b-quark decays whether there is a t-quark, but its mass will be unknown. The next chance to find it will be at TeV II.

Further, if there is a sufficiently light, charged, Higgs-like boson $a_T^{\pm}$ (see the Higgs physics section below), e.g., as expected in the technicolor theory, where it has a mass of about 8 GeV (Dimopoulos, Raby, Kane), then the usual decays $t \to bq\bar{q}$ or $t \to b\ell\nu$ will not dominate. Instead, $t \to ba_T^{\pm}$ will dominate because it is semi-weak. This is a real possibility that must be considered. If t is found and does not decay this way, it gives an important limit on the mass of a_T.

Rare Processes

Current theoretical ideas such as technicolor or flavor unification now often lead us to **expect** rare decays with typical gauge couplings and gauge boson masses in the 10-50 TeV region. This suggest that many rare proceses such as $K \to \mu e$; $K \to \pi\mu e$; $\Sigma^+ \to p\mu e$; $\Xi \to \Lambda\mu e$; $\tau \to eee$, $ee\mu$, $e\mu\mu$, $\mu\mu\mu$; $D \to \pi\mu e$; $F \to K\mu e$ will occur with branching ratios at the 10^{-9} or 10^{-10} level. It is no longer a random "shot in the dark" to expect a non-zero result in such a search. Good limits will now restrict theories in a significant way. The process $\Sigma^+ \to p\mu e$ may be very nice as it is non-zero in essentially all models, and no limit is published at present.

Left-Right Symmetry?

Often theory arguments lead to the expectation that right-handed charged currents should be significant. It is extremely important to check experimentally for such effects. This can be done in the usual way at TeV II in ν reactions, by examining y distributions. Possibly it can be checked for ν_τ, if it is not already shown to be V-A there from τ decays. A very good TeV II test is the comparison of $\mu N \to \nu X$ for left- and right- handed muons. A test for the (c,s) current is available in $D \to K\pi\ell\nu$ and for the (b,c) current in $B \to D\pi\ell\nu$; with of order 10^4 examples of $D \to K\pi\ell\nu$ per day, it may provide a good place for testing both the Lorentz structure of charm decay and studying CP violation in a new place.

Higgs Physics, Technicolor

Since spontaneous symmetry breaking occurs, some Higgs physics must occur. It is there waiting to be found. Although the masses of fundamental Higgs bosons are not yet calculable, experiments could put useful contraints on ranges of masses and couplings. In the technicolor theory it is possible (Dimopoulos, Raby, Kane) to calculate the masses of the higher states (pseudo Nambu-Goldstone bosons). One expects a charged pseudoscalar with $m \simeq 8$ GeV and two neutrals with $m < 2.5$ GeV. Experimentally, the charged Higgs is probably heavier than the b-quark or the b would decay into it semiweakly. There are essentially no known constraints on the masses of neutral scalars or pseudoscalars.

The charged technicolor pseudo decays mainly to $\tau\nu_\tau$ (about 40%), $c\bar{s}$ (about 40%), and $c\bar{b}$ (about 20%). The neutrals decay mainly to $\mu^+\mu^-$ (about 1/3) and to $\phi\phi$, $K^*\bar{K}$ (about 2/3 total). They may have an important $K\bar{K}$ (parity violating) decay.

Detecting $W^\pm$?

Can one find explicit signs of charged vector bosons at TeV II? The energy is too low to produce one, so it must be an indirect signal. One hope is obviously to study the total ν cross sections to see a departure from the straight-line rise. Another, perhaps more favorable, is to study the y dependence of $d\sigma/dy$, looking for departures from a flat distribution. Scaling violations affect these tests, but by the time they are performed it is likely that we can reliably correct for them. It is hoped that $W^\pm$ will have been found by then and one will be confirming their interactions.

Charmed Baryons

Studying the decay systematics and mass spectra of charmed baryons (and b-quark baryons) could be of great interest. In photon beams and hyperon beams it may be possible to produce large quantities of charmed baryons with good signal to noise. Perhaps even some rare modes could be found.

Neutral-Current Measurements and $\sin^2\theta_W$

In the future, measurements of neutral-current interactions may play a role comparable to that of proton decay in helping probe experimentally into grand unification and the family problem. If the Weinberg-Salam theory provides a complete parameterization of neutral current interactions there can be no additional U(1) invariant subgroups (i.e., no additional Z boson). Conversely, if there are additional U(1) invariances left over from a larger group structure which is broken down, there will be additional Z's and additional parameters needed to describe neutral currents. See recent preprints of Barr and Zee, and of Deshpande, for discussions.

Careful (± 0.01?) measurements of $\sin^2\theta_W$ may be one of the main ways to probe grand unification. That $\sin^2\theta_W$ is predicted to 20% accuracy or better by the simplest grand-unification models is a great accomplishment. Confirming any discrepancy between experiment and the simplest theory, as accurately as possible, is important. If there is a discrepancy it will help tell us what form of grand unification is correct. It is also important to measure $\sin^2\theta_W$ well in both low Q^2 and in high Q^2 interactions to test radiative corrections to the lowest-order electroweak theory.

Our present knowledge of ν_e interactions, and test of μ/e universality in the neutral current interactions, is not very good. Differences in family structures could emerge here.

QCD Tests

Among the significant tests of QCD one can emphasize a few. Measuring σ_L/σ_T in deep inelastic reactions at larger Q^2 is very important. Measuring the strong coupling $\alpha_s(Q^2)$, and confirming that it agrees with what is found in e^+e^- is very important. Using the larger lever arm provided by TeV II (because $Q^2 \lesssim 20$ GeV^2 is not really available to test scaling-violation predictions) will be very important. And sorting out the situation in large p_T hadron reactions where perturbative QCD is not necessarily under control (Ellis, Furman, Haber, and Hinchliffe) may be important. Heavy quark production will provide important QCD tests; see later sections.

There are two kinds of spin dependent QCD studies. First, there are a series of perturbative predictions for quark, gluon, and photon polarizations. These have the same level of rigor as QCD jet predictions. Indeed, as has happened before historically, they can serve as an important check on whether the theory is valid even when cross-section results appear to be as predicted (see a series of papers by Pumplin, Repco, and Kane). The measurements are not easy ones.

Second, there are model-dependent spin effects involving measuring or making assumptions about the polarization of quarks or gluons in polarized hadrons, such as polarized beams or targets. These provide new knowledge about hadron wave functions, and allow study of behavior expected from but not rigorously calculable from QCD.

Using Jets

In addition to testing jet predictions in QCD, we have to learn to recognize quarks and gluons as jets. Probing many new things, such as t-quark physics, technicolor, Z decays and width, may require working with jets. It will be necessary to learn to do effective mass physics with jets, to identify jet quantum numbers, etc. While new kinds of physics may not come at TeV II from using jets, it may be possible to learn there the techniques that will be very valuable at the Tevatron Collider.

Summary

As can be seen from the above list, it is clear that the physics results expected from the fixed-target Tevatron program will be among the most exciting of the next decade. One clear outcome of the study was that some of the most important experiments should be dedicated ones rather than multipurpose detectors. An example was $\Sigma^+ \rightarrow p\mu e$, where a dedicated experiment might hope to gain more than 10^3 in sensitivity to such a rare decay. The high-resolution detectors are multipurpose, except for triggering devices that get very specific. Another result was that groups of experiments with a common program could be of great value, and would require planning and foresight on the part of experimenters and the Laboratory; scaling violation tests, full determination of the neutral current interactions for a given family, or measuring the Q^2 dependence of $\sin^2\theta_W$, are examples.

Over a few years, the fixed-target Tevatron will produce perhaps 10^9 charmed particles and 10^6 b-quarks. Using these as probes of new physics will allow discoveries that are hard to predict now and will leave room for clever experimenters to do important experiments.