concerns, expressed in some quarters, that multiple scattering and defocusing might introduce serious distortions in Klug's reconstruction technique. Klug and Erickson were able to show that multiple scattering is not a serious problem, and that the purposeful defocusing employed in low-resolution (20 Å) electron microscopy to enhance contrast does indeed provide useful imaging information.

At the time Klug regarded this analysis as a largely academic exercise. But in 1975 Nigel Unwin and Richard Henderson at the MRC Laboratory made dramatic use of Klug and Erickson's analysis of defocusing. Defocusing is employed to produce electron-micrograph contrast when imaging transparent objects, because one cannot do phase-contrast electron microscopy. Klug and Erickson pointed out that computer processing of the images thus distorted can retrieve the "true" image.

Unwin and Henderson have developed new sample-preparation techniques that permit them to apply these defocusing ideas to high-resolution electron microscopy—dispensing with heavy-metal-salt stains and achieving striking contrast and resolution with very low electron beam intensities. Averaging over many unit cells, they have succeeded in reconstructing the three-dimensional configuration of proteins in a photosynthetic membrane with a resolution of only 9 Å. This is the first time that anyone has seen a membrane protein *in situ* with anything like this resolution, DeRosier told us. He regards this work as "a dramatic culmination of Klug's many years of work in electron microscopy."

"The great breadth of Klug's structural work on a number of very important molecular aggregates has contributed greatly to our understanding of how biological complexes are constructed," DeRosier said. "He had revolutionized the way such structures are visualized and their images interpreted." Casper points to "the dazzling range of his accomplishments, which bears the unmistakable imprint of his talent and insight in mathematics, physics, chemistry and biology. Conceptual barriers between different disciplines do not exist for him." —BMS

A look at the future of particle physics

US particle physicists have recently been debating where they will find the most exciting physics and what future facilities will best enable them to pursue it. The dialog intensified last summer at a study sponsored by the American Physical Society Division of Particles and Fields and continued through the Division's annual meeting at the University of Maryland at the end of October. About 150 high-energy physicists participated in the DPF Summer Study on Particle Physics and Future Facilities, which was held in Snowmass, Colorado (near Aspen), from 28 June to 16 July. Its stated purpose was to assess the physics topics that might be interesting in the future, to explore the limits of technological capabilities and to consider the nature of future facilities for particle physics in the US. Charles Baltay (Columbia University), chairman of the organizing committee, stressed to us that the role of the summer session was to study the physics but not to arrive at any specific conclusions.

Although the DPF Summer Study drew heavily on previous studies dealing with future physics and accelerators, it differed from them somewhat in content and structure: First, the study did not focus on any one particular facility but was more generally concerned with all future US facilities and experimental programs. Second, its organizational structure promoted interchanges among particle physicists from different areas of specialization. The entire atmosphere stimulated what Maury Tigner (Cornell) termed "free-wheeling" discussions.

Working sessions at Snowmass were determined by a matrix of topics. Each participant attended morning sessions in one row group and afternoon sessions in one column group. The four rows were labeled by topics in physics: testing the standard model, beyond the standard model, accelerator technology, and novel accelerator ideas and novel detector ideas. The columns denoted five types of facilities: lepton–lepton, lepton–hadron and hadron–hadron, colliders, fixed-target accelerators and non-accelerator experiments.

Existing and planned accelerators set the stage for discussions of physics still to be explored and facilities yet to be built. The US has a number of major facilities in operation, under construction or in the planning stages. Fermilab is in the midst of installing its Doubler/Saver, a ring of superconducting magnets below its existing main ring, and hopes to have all magnets in place and cooled by this spring. The project is the first of its size to use superconducting magnets. Fermilab will extract a 1000-GeV proton beam from this new ring and operate the machine as a fixed target project called Tevatron II. By 1985, Fermilab plans to have completed an antiproton source plus auxiliary cooling and storage rings so that it may operate the facility as a proton–antiproton collider. This project is called Tevatron I. TEV I will have a center-of-mass energy of 2000 GeV (or 2 TeV) and expected luminosities on the order of 10^{30} cm^{-2}sec^{-1}.

Brookhaven currently operates a 30-GeV Alternating Gradient Synchrotron but has had to slow construction drastically on Isabelle, its planned 400 on 400 GeV proton–proton collider (PHYSICS TODAY, April 1982, page 20). However, Isabelle, or a less expensive version of its original design, may resurface under the more generic name, the Colliding Beam Accelerator. Brookhaven has been asked to study several alternative designs and to narrow the choice to one by March. The leading candidate for this accelerator is a p–p colliding-beam facility with beam energies of 400 GeV and luminosities on the order of 10^{33} cm^{-2}sec^{-1}, according to Brookhaven's director, Nicholas Samios. Other contenders are a 20 on 400 GeV electron–proton or a heavy-ion collider. Paul Reardon (formerly associate director of the Princeton Plasma Physics Lab) recently became the project head of the Colliding Beam Accelerator and associate director for high-energy facilities at Brookhaven.

SLAC operates three lepton accelerators at present: the 33-GeV linac and two electron–positron colliders, SPEAR (4 on 4 GeV) and PEP (18 on 18 GeV). SLAC is conducting an R & D project on a linear electron–positron colliding-beam machine with 50 GeV in each beam that could be built by 1986 or 1987. This SLAC Linear Collider will help determine the feasibility of using linear e^+e^- accelerators to attain still higher-energy lepton collisions.

Cornell University has an 8 on 8 GeV lepton collider called CESR. Cornell has proposed building a 50 on 50 GeV successor, but in October Cornell announced that this project is not being pursued any longer at an Ithaca site.

In Europe, two major new facilities are making their debut at CERN. That center recently began running its proton–antiproton collider, which has a center-of-mass energy equal to 540 GeV and a design luminosity of 10^{29} cm^{-2}sec^{-1}. CERN is also constructing an e^+e^- colliding-beam machine (LEP) that will initially have beam energies of 50 GeV and that will eventually be upgraded to produce beams of 130 GeV each. At DESY two e^+e^- colliding-beam devices are in operation. DORIS has 5 GeV on 5 GeV. PETRA is just starting to run at 20 on 20 GeV and is expected to run at 22.5 on 22.5 GeV next spring. DESY is seeking approval for its plans to build HERA, a 30 on 800 GeV e–p collider.

Future facilities. Of the accelerators now funded, approved or under construction, the major ones to be operating in the US by the end of this decade will probably be the Tevatron and SLC. What facility will take the US beyond 1990? Particle physics could pursue several paths but, in times of increasing costs and decreasing funding, cannot travel them all simultaneously. A key debate thus becomes what will be *the* next generation facility. Advocates are already lining up in the camps of both lepton and hadron colliders, and some are endorsing e–p machines. Other voices are speaking up for nonaccelerator facilities to conduct low-energy and cosmic-ray tests of particle physics.

One group at Snowmass studied both the technical and economic feasibility of a p–p collider whose center-of-mass energy would be a factor of twenty greater than that of TEV I. They considered two extreme designs for a 20 on 20 TeV accelerator. One featured 10-T magnets arrayed in one or two rings with circumferences on the order of 50 km. The other was based on 2.5-T "superferric" magnets, which would be less costly but would require a ring circumference of about 200 km. Accelerators of such immense size would require a large area of flat land. When some participants suggested a site in the southwest, the concept became dubbed the Desertron. Because superferric magnets may lend themselves more readily to automated production, the participants felt that this second design might be cheaper. However, even when the studies assumed a high degree of automated production and installation, the cost estimates for both designs were significantly above a billion dollars. The study also concluded that a 10 TeV on 10 TeV p–p accelerator could be built for under a billion dollars.

Regarding e^+e^- colliders of the future, experience at LEP and SLC may help determine whether either circular or linear configurations show promise for escalation to still higher energies in lepton machines. A central issue concerns the dependence of luminosity on energy in the two approaches.

The Snowmass working group on fixed-target accelerators explored the feasibility and value of fixed-target machines that were either modifications of high-energy hadron colliders or very intense, moderate-beam-energy facilities. Yet another group studied the experiments that could be done at non-accelerator facilities such as an underground lab. They felt that the definitive tests of grand unified theories, for example, would approach in complexity and cost the major experiments at high-energy accelerator labs.

Tigner commented that the entire particle-physics community needs to become aware of the tremendous technical problem in building next-generation facilities. He urged an active accelerator R & D program to find a timely solution that would permit particle physics to go forward with reasonable expectations for new discovery.

The experimental programs at the existing and near-term accelerators should test thoroughly the so-called "Standard Model," according to the Snowmass group studying that topic. The Standard Model comprises the gauge theory of the electroweak interaction (the Weinberg–Salam–Glashow model) and of quantum chromodynamics. Summer-study participants produced an exhaustive list of possible experiments to test this model, complete with estimated cross sections, event signatures and so forth, that are expected in the various US facilities.

Several tests of the Standard Model concern its prediction of various particles. Foremost among these are the charged and neutral vector bosons $W^\pm$ and Z^0, anticipated at center-of-mass energies of around 80 and 90 GeV, respectively. Next is the Higgs boson of unknown mass. (This boson is associated with a scalar field added to the gauge theory to produce spontaneous symmetry breaking at low energies to give finite masses for the gauge particles.) Others are the top quark, the τ neutrino and the bound states of the gluon (the field quantum of QCD). The Standard Model further predicts the spectroscopic levels of the bound states of the quarks; measurement of the spectroscopy of the bottom quark appears most promising for thorough testing of these predictions. The list goes on and on. For the items on this list, hadron–hadron, lepton–hadron and lepton–lepton colliders could all yield significant results. Although hadron colliders offer higher beam energies and data rates, the lepton colliders would yield cleaner events.

Despite the success of the Standard Model, it is incomplete. Its main shortcomings are that it does not explain the origin of particle masses or of flavors, and it does not address the possible grand unification of quarks and leptons.

One group of modifications to the Standard Model concern attempts to eliminate the Higgs scalars, which are considered to be ugly additions that bring with them many arbitrary parameters. Two possible approaches have been suggested: One is technicolor, in which the Higgs fields are replaced with bound states of new fermions. The other is supersymmetry, in which the scalars are put into multiplets with fermions, so that most of their couplings are determined by symmetry considerations.

Both technicolor and supersymmetry predict new effects at a mass scale of about 1 TeV or less. Several independent arguments can be made for expecting at least some particles with masses much lighter than this typical mass scale—somewhere between 10 and 250 GeV. For example, technicolor may yield the technipions or technietas in analogy with the low-lying mass states of the 1-GeV mass scale. Supersymmetry might produce partners of known particles, such as the smuon or selectron. Although these two theories are both attractive, neither has been made to work and they may well be wrong. Even so, the group studying physics beyond the Standard Model still expected to see some mass structures in this energy range. Yet other suggestions for new particles include composite leptons and quarks, new members of the quark or lepton families and charged Higgs bosons.

Also beyond the Standard Model lie the grand unification theories. Their predictions include the decay of the proton and the existence of a massive magnetic monopole. All these predictions rely at least to some extent on the Standard Model as a guide to going beyond itself. Martin Perl (SLAC) spoke at the DPF annual meeting about the exploratory type of experiment that searches for phenomena beyond our current ability to predict.

The difficult choice before particle physicists now is to match the future physics with the future facility. Clearly any new facility must enter a new region, but should it be one of higher energy (the traditional direction) or higher luminosity or some combination of both? Luminosity has become increasingly important because the cross sections tend to decrease as energies increase. The tradeoff is complex and depends on the total energy and on the reaction of interest. A rule of thumb used by some of the participants is that for constant event rates a factor of ten in luminosity is about equal to a factor of three in energy. Kane estimated that a good many of the effects considered by the group studying physics beyond the Standard Model would require luminosities of about 10^{33} $cm^{-2}sec^{-1}$.

The energy-luminosity tradeoff is influenced by constraints on detectors. If detectors can be made to be more efficient, the luminosity requirements could be correspondingly reduced. Another factor is that the high luminosity at a hadron collider produces a large background that poses a severe challenge to detectors to discriminate events and to withstand the high radiation. It furthermore places great demands on computerized data acquisition and analysis systems. The group studying new detector technology estimated the limits to be 10^{32}–10^{34} $cm^{-2}sec^{-1}$. The important detector types will be high spatial resolution vertex detectors for detection of decays in flight, finely segmented hadron calorimeters for analyzing jets, and new devices for particle identification.

The energy-luminosity question also affects the choice of future particle facilities. The lepton colliders have much higher ratios of event cross section to total cross section and thus permit cleaner experiments and more precise tests. However, lepton colliders may be restricted from attaining the high energies available in hadron colliders. On the other side of the coin, the costs of building p–p colliders rise rapidly with energy. Accelerators with $\bar{p}$–p colliding beams may reduce costs but at a sacrifice in luminosity. The structure of the machine—the number of interactions, the addition of more rings—may alter these tradeoffs in one way or another. —BGL

Theory points to pulsating white dwarfs

Astronomy is, in a fundamental sense, an observational science. Unlike some areas of modern physics, for example, where theoretical developments are often the driving force behind experimental progress, new discoveries in astronomy most often spring from new instrumentation for the observer; theoretical predictions of yet-to-be observed phenomena are rare.

An elegant exception to this rule has recently emerged from the study of white dwarf stars. Rapid development in the theory of pulsating white dwarfs over the past three years led to the publication[1] early last year of a prediction of a new class of variable stars. A few months later, an object matching the theoretical description was found. Donald Winget, whose dissertation research at the University of Rochester formed the basis of the prediction and who was part of the observing team at the University of Texas where the new star was found, told us, "This is the first instance in which the existence of a new type of variable star was predicted from theoretical considerations." His thesis adviser, Hugh Van Horn, added that the new star's discovery "provides an important confirmation of the underlying theory of pulsating white dwarfs."

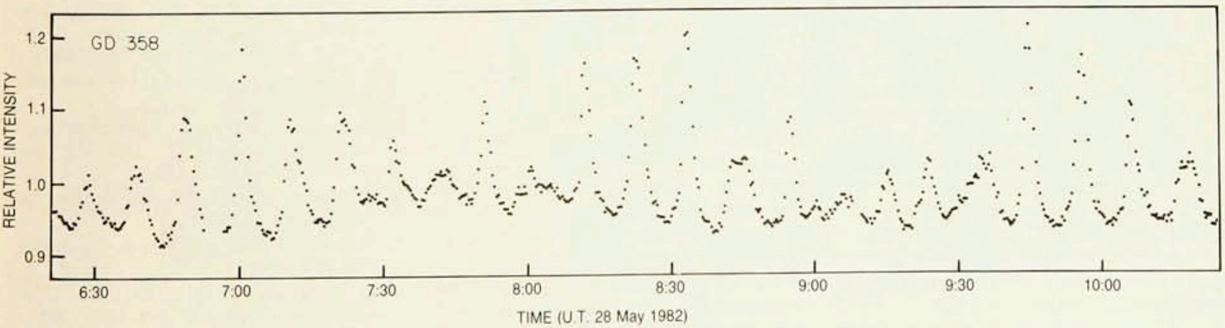
The white dwarf phase is the final state for most of the stars in the universe (PHYSICS TODAY, January 1979, page 23). With its supply of nuclear fuel exhausted, a star will begin to contract. For stars with masses less than a few times that of the Sun, the stellar envelope is subsequently ejected, and the central contraction is stopped by electron degeneracy pressure before the core temperature becomes high enough to initiate the next set of fusion reactions. The result is a solar-mass star with a radius comparable to that of the Earth and a mean density of about 10^6 gm/cm^3; most of the star's mass is contained in a degenerate carbon core. Roughly 80% of known white dwarfs have spectra showing an overlying atmosphere of pure hydrogen, whereas most of the remainder exhibit only helium lines. The origin of the distinction between these so-called DA and DB dwarfs is not well understood.

With gravitational contraction prevented by the pressure of the degenerate Fermi gas and a central temperature too low for exothermic nuclear reactions to proceed, the only energy reservoir remaining is the thermal energy content of the star. From an initial blackbody temperature larger than 10^5 K, the temperature and luminosity of the star decline monotonically as the heat is gradually radiated away. Recent theoretical cooling calculations suggest that the star will reach a surface temperature of about 5000 K in $5–7\times10^9$ yr; the coolest white dwarfs known are at about this temperature. The current population of white dwarfs, then, may be viewed as a fossil record of the entire history of star formation in the Galaxy.

In 1968, the discovery of quasi-periodic (with a period of about 750 sec) light variations in the white dwarf HL Tau-76 by Arlo Landolt (Louisiana State University) stimulated a number of observers to search for other examples. An extensive survey by James Hesser and Barry Lasker at the Cerro Tololo Interamerican Observatory in Chile uncovered the white dwarf pulsator ZZ Ceti after which this class of stars has been named. Others of the 16 stars found to date were detected by Brian Warner (now at the Capetown Observatory) and Edward Robinson (University of Texas) and John McGraw (University of Arizona).

All stars have hydrogen envelopes (type DA), and all are multiperiodic, with as many as six or more separate frequencies apparent in their light-curve power spectra. The low-amplitude pulsations have remarkably stable periods, with upper limits to a period derivative $\dot{P}$ as low as 6×10^{-14} sec/sec. Perhaps the most remarkable aspect of the stars, however, is their temperature distribution: Despite the large range of known white dwarf temperatures (5000–100 000 K), all of the pulsating stars lie in the range 11 000–13 000 K. Furthermore, recent studies of large samples of pulsating and nonpulsating dwarfs by Jesse Greenstein[2] (Caltech) and Gilles Fontaine, Pierre Lacombe (University of Montreal), McGraw, David Dearborne and J. Gustafson[3] (University of Arizona) conclude that most, and possibly all DA stars in this temperature range pulsate. One is thus forced to a single-parameter model in which every white dwarf starts oscillating when it cools to 13 000 K and stops again after cooling another 10%.

One question that must be addressed in the pursuit of such a model is the identification of the modes of oscillation these stars undergo. Simple radial pulsations (such as those seen in classical Cepheids and RR Lyrae variables) are ruled out; spherically symmetric radial oscillations of a white dwarf have periods on the order of the time



The light curve of the helium white dwarf variable GD 358 showing the complex multiperiodic behavior characteristic of nonradial pulsators. A total of 29 oscillation modes with periods ranging from 142 sec to 952 sec can be indentified from an analysis of this light curve. (From ref. 7.)