

Report of the
Nuclear Science Advisory Committee
Subcommittee on Nuclear Theory

May 11, 1988

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Dear Karl and Dave:

At its meeting of May 11, the Nuclear Science Advisory Committee unanimously accepted the Report of the Subcommittee on Nuclear Theory. We believe that there is wide acceptance among the community of nuclear physicists of the need to strengthen nuclear theory. The report is transmitted herewith with a high recommendation for implementation, particularly of its primary recommendation, which we urge you to give high priority. Subsidiary recommendations are viewed by us as a possible plan for implementing the primary recommendation. Opportunities exist for getting high quality young physicists into the field, and it is important to take advantage of them as soon as possible.

Sincerely yours,

Ernest M. Henley
Chairman, NSAC

cc: G. Crawley
W. Hess
NSAC Members
Subcommittee members

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I. Introduction and summary

This is the report of the Nuclear Science Advisory Committee (NSAC) Subcommittee on Nuclear Theory, submitted to NSAC on May 11, 1988. The subcommittee, appointed in December, 1986, consisted of

R. Amado	University of Pennsylvania
G. Baym	University of Illinois
C. Dover	Brookhaven National Laboratory
W. Haxton	University of Washington
F. Iachello	Yale
S. Koonin	Caltech (Chairman)
A. MacDonald	Princeton
E. Moniz	MIT
P. Siemens	University of Tennessee/Oregon State University
G. West	Los Alamos National Laboratory

I.1 Charge to the subcommittee

The formal letter from NSAC Chairman E. Henley establishing and charging the subcommittee is attached as Appendix A. Our charge was as follows:

1. By considering the likely key research areas over the next five to ten years, the Committee shall determine the needs and opportunities which exist to strengthen the vitality of nuclear theory, and outline resources (e.g., special computing needs) required to support theoretical research in key areas.
2. The Committee shall make suggestions for ways to strengthen the nuclear theory presence at universities, and determine if any special efforts are required to attract outstanding students to the field.
3. The Committee shall determine the advantages and disadvantages of developing special mechanisms to strengthen nuclear theory such as one or more national nuclear theory centers, summer schools, summer institutes, national laboratory workshops, or institutes, etc.
4. The Committee shall advise on ways to optimize the synergism between theory and experiment.
5. For areas of research which overlap other fields, (e.g., particle physics), the Committee shall determine in what manner the research quality can be optimized.

I.2 Operation of the subcommittee

The subcommittee met four times during 1987 in the course of responding to its charge. These meetings were held on February 23-4 in Pasadena, on May 7 in Washington, D.C., on July 23 in Chicago, and on October 15 in New Brunswick. Agendas for the February and May meetings are attached as Appendix B. The July meeting was devoted to refining an early draft of this report and the 2-hour October meeting was to prepare a preliminary report to NSAC, delivered at its October 16 meeting.

We made efforts to solicit input to our deliberations from the nuclear theory and broader nuclear science communities. We received and considered letters from concerned individuals and devoted virtually all of our May meeting (widely publicized by NSF and DOE) to public comment. Draft versions of this report were circulated to several individuals for their comments, as well as to the members of NSAC.

Officials of the funding agencies were of great help to us in carrying out our charge. In particular, Bruce Barrett and William Thompson, rotators in nuclear theory at the NSF and DOE, respectively, provided us with many of the statistical data we considered, as well as their own perspectives on the field and the funding process. Sherman Fivozinsky and David Hendrie of DOE and Karl Erb and William MacDonald of NSF were also helpful in generating manpower statistics.

I.3 Perspective

The overall state of nuclear theory can be summarized in the following four statements, which we document and amplify below.

1. Nuclear science has undergone a great revolution in subject matter and technique in the last decade. New experiments continue to test the limits of our understanding of the nucleus. These advances demand new descriptions, at both the phenomenological and theoretical levels. Among the most challenging tasks for the future are:
 - Understanding hadronic structure and interactions in terms of QCD, the fundamental theory of the strong force, defining the limits of an hadronic description of nuclei, and studying new states of matter based upon quark-gluon degrees of freedom.
 - Clarifying the connection between nuclear phenomena and the underlying many-body Hamiltonian and developing general techniques for describing strongly interacting few- and many-fermion systems. The formulation and application of a relativistic many-body theory remains an outstanding problem.
 - Predicting the behavior of hadronic material under extremes of density, temperature, isospin, mass number, deformation, and angular momentum.
 - More fully exploiting nuclear systems as laboratories for testing the fundamental symmetries and interactions of nature.
 - Exploring phenomena and theoretical techniques for interdisciplinary problems involving nuclear science and other fields, including astrophysics, elementary particle physics, atomic physics, and condensed matter physics.
2. The nuclear theory community has been hampered in playing its proper role in the progress of nuclear science by palpable shortages of money and people.
3. The number of talented students and postdoctoral fellows interested in doing nuclear theory is now larger than it has been in recent years and is growing. This is an exceptional opportunity for strengthening our field and assuring a vital future for nuclear science.
4. The consistent underfunding of theoretical nuclear science has been noted and decried eloquently by repeated reviews of the field during the last decade, including the *Ad*

Hoc Panel on the Future of Nuclear Science (1977), the 1979 *NSAC Long Range Plan*, and *Physics Through the 1990's* (Brinkman Report, 1986). Our own analysis is in full accord with these earlier findings. Restricted operating funds and the resultant difficulty in bringing young people into the field are taking their toll. If this situation is allowed to continue, it will inflict further damage on all of nuclear science, particularly in view of the major experimental initiatives upon which the field has recently embarked. The strengthening of nuclear theory must become an immediate priority for the entire field of nuclear science.

I.4 Recommendations:

Significant changes in both the quantity and modalities of nuclear theory funding are needed urgently; there are many opportunities for improvement. Our recommendations are summarized below and are discussed in detail at appropriate points in the following text. They take the form of a principal recommendation and several subsidiary ones, the latter pertaining to the implementation of the former.

Principal Recommendation: We recommend a five-year plan to strengthen nuclear theory, reversing the declining trends of the past decade. An important aspect of this plan would provide 60–65 additional Ph.D. level personnel, bringing the fraction of nuclear scientists who are theorists in the range of 26%. This plan is based on our analysis of the immediate needs of nuclear science for theoretical leadership, diversification, and support and the quality of new personnel that would be supported. It is consistent with the field's capacity for absorbing additional faculty and staff, producing new postdoctoral fellows, and training new students. The plan would require an additional \$9.0 M in FY93 (FY88 dollars). Such an increment would increase theory funding from the present level of about 6% of the nuclear science operating budget to about 10% in FY93, in accord with the increase recommended by the 1979 *Long Range Plan*. A key part of our plan is an enhanced number of theorists at universities with high-quality graduate programs, and the funding to provide for the training of students in nuclear theory. This is essential for the future of the field.

To implement this plan, we make the following recommendations.

Recommendation: To increase the theory presence and strengthen the interaction between theory and experiment, we recommend that funding agencies encourage and support quality theoretical research faculty at universities with active experimental groups but few or no professorial theory faculty.

Recommendation: We recommend the creation of one or more nuclear theory centers. Such centers must be truly national in character, must have a significant interdisciplinary component, and must be viewed and funded as an important complement to the strengthened individual programs discussed in our Principal Recommendation.

Recommendation: We recommend that the agencies include appropriate theoretical funding as an integral part of large experimental projects. A strong theoretical component is essential to the success of major facilities and we endorse initiatives to create theory groups at CEBAF and, at the proper time, RHIC. These new groups are part of the growth needed in nuclear theory to meet the challenges facing our science. Groups consisting of a small core of permanent staff, with vigorous programs for long- and short-term visitors and

for postdoctoral fellows, should be considered as alternatives to a larger permanent staff. Where possible, permanent staff in these groups should be associated with, and perhaps hold joint appointments at, neighboring universities.

Recommendation: Adequate access to supercomputers and workstations is increasingly important to progress in theory. We recommend that the funding agencies ensure such access to the researchers they support.

Recommendation: We recommend that the funding agencies be receptive to low-cost proposals that are important in fostering interaction between nuclear theorists, and more generally, between nuclear theory and the broader physics community. Such measures include the funding of summer schools, an increased nuclear presence at the ITP and Aspen, and the funding of temporary working groups of nuclear theorists to attack specific problems.

Recommendation: We recommend that the funding agencies add nuclear theorists as permanent program officers.

II. The role of theoretical nuclear physics

There is much about the atomic nucleus that excites a theoretical physicist. At the most fundamental level, there are unique opportunities to study the largely unsolved Quantum Chromodynamics in its several manifestations, ranging from the internucleon potential through hadronic structure and interactions to new forms of matter based upon quarks and gluons. However, even in the conventional picture of nucleons *cum* potentials, there are great intellectual challenges: a finite many-fermion system with spin-dependent strong forces, a theater of operation for the electroweak force, and a diverse spectroscopy. The scope of nuclear science has expanded greatly during the past decade, as experimentalists produce and study nuclei with extremes of angular momentum, isospin, excitation energy, mass number, deformation, and density. Phenomena like these, which require theorists to be adept with a variety of tools and models, are at the center of our field. Addressing them adequately is integral to society's goal of understanding the natural world.

The importance of theory to the success of the nuclear science enterprise as a whole cannot be overemphasized. Theory, and its interplay with experiment, is an essential element in the synthesis that defines our understanding and signals new directions for research. It is the intellectual underpinning of the field, a framework in which to think about nuclear phenomena, and plays an important role in representing the field to the rest of physics.

Although the nature of theory and its relation to experiment varies among the sub-fields of nuclear science, the role of theoretical considerations in motivating and guiding experimental activities is increasing. As the scale of nuclear science facilities and experiments grows, there are fewer of each, and well-considered decisions become ever more important. In view of the growing sophistication of our understanding and the advent of complex multi-parameter measurements, deciding *what* to measure has become as important as *how* to measure it. Theoretical guidance (and the theorists to provide it) are therefore essential to the choice and success of major facilities and experiments. Of course, it is then imperative that theorists be knowledgeable about what can be measured. Nuclear science is fortunate to have maintained a tradition of close contact between theory

and experiment; we see this as extremely healthy and likely to increase even further in the future.

In training theorists, nuclear theory offers many advantages to students. As the focus of elementary particle theory becomes more abstract, students who are interested in subatomic physics and who want to grapple with the physical world are turning to nuclear theory. They are trained broadly, exposed to methods ranging from quantum field theory to statistical mechanics to classical mechanics. They are taught to deal with complex, real-world systems that offer manifold possibilities for exercising physical intuition. Most also learn how to compute, as well as the subtle balance between analytical work and computation required to efficiently use that skill. And, as they often deal directly with data, they acquire a more thorough knowledge of experiment than do their peers in other fields. Indeed, there are few other subfields of physics in which the education of a theoretical physicist can be as thorough.

III. Key research areas

The scope and techniques of theoretical nuclear science have expanded considerably in recent years. There is now a real theory of the strong force between quarks, though it poses many fundamental, unsolved problems and its consequences for hadrons and multi-hadron systems beg to be explored. Novel nuclear structure concepts and more powerful many-body techniques are being developed in pace with increasingly precise experimental characterizations of nuclear states. These, in turn, are allowing new and sophisticated nuclear tests of fundamental interactions and symmetries. The properties of nuclear matter under extreme conditions, both in the laboratory and in astrophysical settings, are being determined with improved confidence, and there are expanding interfaces with other subfields. The development of these themes will be the task of nuclear theory during the next decade.

In this section, we outline the challenges and opportunities for nuclear theory in the coming years. Each of five broad tasks we noted in Section I is, of course, realized to varying degrees in the specific research studies we discuss below. We emphasize that our discussion is by no means exhaustive, but rather is meant to indicate the principal challenges that the field must confront during the next decade. We also note that major accomplishments in understanding the diverse and complex phenomena nuclei present will occur only through a theory effort growing in quality and quantity; the need for this growth has never been greater.

III.1 Quantum chromodynamics

The traditional view of the nucleus as a collection of nucleons bound by meson-mediated two-body potentials is useful in correlating an impressive array of nuclear properties, but a modern frontier is the search for a more microscopic description in terms of the quarks and gluons of the underlying gauge theory of strong interactions, Quantum Chromodynamics (QCD). This will lead to a fundamental understanding of the hadronic interaction and to the prediction of new phenomena based on quark-gluon degrees of freedom.

Potential models can often be adjusted to fit data, but at the expense of introducing parameters. Extrapolation of these models to new physical regimes is therefore uncertain,

and nagging problems persist when they are subjected to precise tests (e.g., structure functions for quasielastic electron scattering, three-body form factors, Coulomb energies). It is, of course, premature to interpret all of the outstanding problems of "conventional" nuclear physics as signatures of quarks and gluons, but the evolving discussion of subnucleon degrees of freedom in nuclei is undoubtedly an important direction for nuclear theory.

Processes transferring large amounts of energy and momentum test a regime where QCD is asymptotically free and perturbative calculations are useful. It is here, where the effects of quark confinement are minimized, that the theory is best understood. However, the static properties of nuclei, or the dynamic nuclear response to low and medium energy probes (leptons, pions, kaons, and protons) is governed by the largely unexplored non-perturbative confinement regime, where the effects of quarks and gluons are likely to be subtle.

Non-perturbative calculations based on the exact QCD Lagrangian are prohibitively difficult. The present method of choice is to discretize the theory on a lattice in space-time and employ Monte-Carlo methods to calculate observables. While much progress has been made along these lines, the present state-of-the-art makes it unlikely that these calculations will soon address multi-nucleon problems with sufficient accuracy. A prime task for nuclear theorists is thus to develop sensible and tractable models that preserve the essential features of QCD.

The Skyrme model, in which baryons are described as topological solutions of non-linear field equations, is an example of a non-perturbative approximation, motivated by QCD in the limit of many colors. It enables a semi-quantitative understanding of many aspects of hadronic structure. The original model is being extended in important ways, including the introduction of vector mesons as gauge particles of a hidden symmetry, and the recovery of asymptotic freedom through the topological chiral bag model. The properties of the many-baryon problem in the soliton approach need more study.

Hadronic structure and dynamics within the nuclear environment are another focus of attention. The EMC effect shows that quark correlations in a nucleus differ from those in a free nucleon. Attention must be focused on delineating those phenomena that require a quark description, and those that can be understood in more conventional terms, such as binding, Pauli, or mesonic exchange current effects. Drell-Yan processes, which probe antiquark distributions in nuclei, could well provide a signature, as might hypernuclear phenomena, which involve one or more strange quarks embedded in a nucleus. The existence of a stable strangeness -2 dibaryon (the H) would signal the need for an explicit treatment at the quark (or soliton) level, as opposed to a meson exchange picture.

In summary, we foresee a strong theoretical effort, whose goal is to implement QCD in a domain where the quarks are neither asymptotically free (as in a quark-gluon plasma, for instance) nor are completely condensed into color singlet clusters (nucleons and mesons). This necessarily involves a parallel study, in QCD language, of the dynamics by which medium- and high-energy probes elicit the response of a many-body system. Attaining this goal would likely have enormous impact on nuclear science, much as the elucidation of electronic structure had on our understanding of molecules and condensed matter. However, further breakthroughs in computational techniques or clever approximations to the strong coupling (non-linear) problem are likely required before quantitative predictions can be made.

III.2 Few-nucleon systems

The simplicity of the few-nucleon systems (^2H , ^3H , ^3He , ^4He) gives them a special status in nuclear science, as their description at the "conventional" level has been refined enough to expose physics sometimes obscured in the more complex nuclei. For example, the first convincing evidence for the effects of exchange currents came from the deuteron and from the $A = 3$ nuclei. Recent advances in theoretical and computational methods make it possible to calculate the properties of the three-body (and perhaps the four-body) bound states with great precision, at least within the framework of nonrelativistic quantum mechanics and potential theory. Such calculations are therefore important baselines from which to gauge departures from that framework and to test subtle effects, such as violations of charge symmetry or those due to relativity. Further improvements in our descriptions of few-body systems, including solving for the continuum wave functions and developing relativistic treatments, will be essential in interpreting new experiments that probe for the effects of sub-nucleon degrees of freedom, such as those to be carried out at CEBAF.

III.3 Nuclear structure

Nuclear structure (the study of energy levels, transition rates, form factors, and such) has been at the heart of nuclear science since its inception. As a finite, strongly interacting many-body system, the nucleus poses a formidable challenge for theorists. In fact, modern formal many-body theory began as an attempt to understand nuclear matter. Efforts to describe important nuclear degrees of freedom in a simple way have produced a series of seemingly disparate models, ranging from the compound nucleus model to the shell model to the collective model. New experiments, such as measurements of form factors at high momentum transfer, are a necessary adjunct to the continuing efforts to improve and reconcile these models, a task that is a principal goal of modern nuclear structure studies.

The mean field or independent particle model has helped us understand many phenomena. For example, in a Hartree-Fock approach, or when combined with a macroscopic model via the Strutinsky procedure, the independent particle model allows an understanding at the 1 MeV level of both fission barriers and nuclear ground state masses, along with associated deformations. Recent experiments have dramatically shown the validity, and failure, of the independent particle picture in heavy nuclei. Understanding the limits of the mean field, the role of multi-nucleon correlations, and the importance of relativity are important, but unrealized, goals.

The successes of the Interacting Boson Model, which has provided an elegant, powerful, and unifying description of low-lying states of complex nuclei, have led to a resurgence of interest in algebraic models of nuclear structure. We have made great progress in understanding the quantitative relationships between these collective models and the underlying microscopic Hamiltonian. The challenge that remains is to develop the technology to predict collective or single-particle parameters quantitatively from first principles.

Despite its sophistication, nuclear structure theory still lacks the precision necessary for many applications. For example, attempts to exploit the nucleus as a laboratory for studying fundamental processes such as parity violation or double beta decay require detailed and accurate nuclear wave functions that, in many cases, are not available. Nuclear astrophysics offers similar examples, where there is need to know the properties of specific nuclear levels to determine reaction rates. It is important that nuclear structure theorists

continue to develop and refine their tools if these important applications are to advance. Truncations of the shell model basis, such as fermion dynamical symmetries, look particularly promising in this regard.

"Quantum chaos", or the manifestation of chaotic classical behavior in quantum systems, has recently become of great interest in a number of fields, including atomic, molecular, and mathematical physics. Central questions are the universality of spectral fluctuations, sufficient conditions on the many-body Hamiltonian to guarantee such universality, and quantal signatures of the transition from integrable to chaotic classical motion. The highly excited states of nuclei are the prototypical examples of quantum chaos, and, in fact, the commonly used Random Matrix Theory was developed for their description. Further theoretical work along these lines (and experiments stimulated by it) may become prominent in coming years, and would be an exciting interface between nuclear theory and other areas of physics.

The supercomputer will likely change our approach to nuclear structure. In some cases, such as the shell model, traditional calculations can now be performed for a wider range of nuclei. In addition, entirely new techniques are being developed that rely directly on numerical simulation. Although it is still too early to assess the impact of this work, the intense interest in numerical techniques has focused much attention on the many-fermion problem.

III.4 Relativistic theory

High-energy electrons and hadronic probes provide experimental information about the structure of nuclei. Pions, whose motion in nuclei is deeply related to nuclear forces, are so light that they are relativistic even with modest kinetic energy; the relativistic motion of nucleons tests the spin-isospin structure of nuclear forces; and relativistic heavy ions are our best tool for making high-density nuclear matter. A full understanding of these rich phenomena will likely require a relativistic theory of nuclear structure. And, of course, the description of nuclei in terms of quarks and gluons is inherently relativistic.

Relativistic theory is in many ways more difficult than its non-relativistic counterpart: it has to deal with relativistic kinematics, the non-instantaneous propagation of forces, the presence of real and virtual antiparticles, and the creation and annihilation of the relativistic quanta. The Paris and Bonn effective two-body interactions provide a good starting point for incorporating relativity, but many-body methods to deal with relativistic systems are in their infancy. Relativistic Hartree and Brueckner-Hartree-Fock computations reflect the state of the art for perturbations about the nuclear ground state.

To fully exploit the results of experiments with pions and high-energy electrons and nucleons, relativistic methods must be extended to include collective correlations, at least at the level of RPA. Relativistic heavy-ion collisions promise to yield quantitative information about hot, dense nuclear matter, but require more sophisticated tools to treat transport phenomena in a time-dependent picture.

There are difficulties even in the formulation of relativistic quantum equations with the requisite features. As these are resolved, the techniques of relativistic theory must be developed into a computational methodology to give quantitative understanding of measurements with electrons and relativistic hadronic probes. We note that some of the problems to be addressed are common with relativistic descriptions of atomic structure.

III.5 Heavy ion collisions

Heavy ion collisions at low energies present a great variety of theoretical challenges. One must deal with a large system with many intrinsic degrees of freedom, displaying both classical and quantal behavior. Productive interactions between experiment and theory have elucidated many of the gross phenomena. Outstanding theoretical issues include a theory (as opposed to a phenomenology) of tunneling in dissipative systems, as occurs in both fission and heavy ion reactions. To fully exploit heavy ion beams for spectroscopic studies, we must develop tractable methods for describing direct reactions involving many coupled inelastic and transfer channels.

Current experiments with newly-available beams of heavy nuclei at 10–100 MeV per nucleon raise an urgent challenge to nuclear theory. Nuclear matter is severely perturbed in collisions induced by these beams and phenomena ranging from mean-field to collision-dominated and from dynamical to statistical are evident. Early theoretical work has stimulated elaborate experiments measuring many reaction products simultaneously, a necessity for determining the collision dynamics. Now, interpretation of these results requires improved dynamical models, as well as increased communication between theorists and experimenters. One goal here is to excite nuclear matter to a regime where it must have a phase transition to a mixed liquid-gas phase never before observed.

Heavy ion collisions at energies of a few GeV per nucleon present theorists with the challenge of determining the nuclear equation of state and dynamical properties of nuclear matter from the remnants of a transient thermalization; there are implications for both nuclear matter theory and astrophysics. There has been much recent progress from the interplay between theory and experiment. Current issues of high interest include determining the influence of velocity dependent forces on the scattering and interpreting the observed pion multiplicities.

At the higher energies of the current CERN and BNL experiments with ^{16}O and heavier beams, the principal challenges include understanding the particle formation mechanisms and subsequent rescattering. These experiments and their interpretation are in many ways a warmup for theorists (as well as experimentalists) anticipating the RHIC experiments.

Ultrarelativistic heavy ion collisions are uncharted territory, and the theoretical opportunities will develop in close conjunction with experiments at the SPS, AGS, and eventually RHIC. The study of nuclear matter in new domains of energy density and baryon density will of necessity require strong theoretical guidance. Of particular importance is the interface with QCD. Theory must develop precise answers to the questions of how to probe the existence of the quark-gluon plasma that could be made in a heavy ion collision, how to locate the transition to the plasma state, and how to study the plasma, including the long range properties of QCD within it.

III.6 Fundamental symmetries and weak interactions

The nucleus has a capacity to filter and amplify interactions, which makes it a marvelous laboratory in which to test fundamental symmetries and conservation laws. Twenty-five years ago, elegant nuclear studies laid the ground work for the standard model by determining the form of the weak interaction. Today's experiments are driven by the hope that a subtle violation of low-energy symmetries will be the first glimpse of physics beyond the standard model.

Theoretical and experimental work in this subfield are intimately intertwined. Theoretical questions usually motivate experiments and theorists often work closely with experimentalists in exploring the significance of new measurements or in suggesting new possibilities. These activities place significant demands on the theorist, as a detailed knowledge of elementary particle, nuclear, and atomic physics, as well as experimental technique, is often required. The following are among the outstanding theoretical challenges.

- **Parity nonconservation:** Tests of the low-energy NN weak interaction are possible in the scattering of polarized nucleons and in parity-violating nuclear decays. Convincing theoretical arguments indicate that the isospin dependence of this interaction differs from the naive expectation based on the form of the current at the quark level: the isovector NN interaction, which should be greatly enhanced by the neutral current, is in fact quite weak. The result is at least superficially similar to the $\Delta I = 1/2$ rule in $\Delta S = 1$ decays. Both of these puzzles indicate that our understanding of the structure of the nucleon is far from complete.

- **CP/T nonconservation:** The origin of the CP/T nonconservation observed in the neutral kaon system is not understood. Theorists have proposed powerful new experiments on atomic dipole moments of atoms testing CP- and P-nonconserving nuclear moments at a level that, in some models, competes with limits on the neutron electric dipole moment. The rapid improvement of these experiments is very promising. Classic nuclear tests of CP violation include detailed balance experiments, complex phases in electromagnetic mixing ratios, and triple correlations in beta decay. The last have provided new constraints on CP violation in left-right symmetric models. Analyses of the statistics of nuclear energy levels have constrained CP-odd P-even internucleon interactions. There is also intense theoretical and experimental interest in probing T-violating nuclear forces by the coherent interactions of low-energy neutrons with polarized materials.

- **Lepton-number nonconservation:** Neutrinoless double beta decay provides our best test of lepton number conservation and of the masses and right-handed couplings of Majorana neutrinos. Theorists have made progress on both the nuclear physics and particle physics of this process, and the significance of double beta decay as a constraint on modern gauge theories is now well appreciated. Double beta decay neutrino mass limits are approaching one eV.

- **Charge Symmetry and Charge Independence:** The TRIUMF n-p elastic scattering experiment and the 80 keV non-electromagnetic binding energy difference between ${}^3\text{H}$ and ${}^3\text{He}$ provide two of the best measures of charge symmetry breaking. Much theoretical effort is focused on understanding its origin.

- **Exotic interactions and particles:** The GSI e^+e^- coincidences remain a puzzling phenomenon. Constraints on axion masses and couplings have been derived from various theoretical analyses of nuclear decays. The importance of the nucleus as a source of varying baryon number/mass and isospin/mass ratios has been exploited in ongoing searches for new long-range interactions.

III.7 Nuclear astrophysics

Nuclear properties play a key role in many astrophysical situations. Theoretical work motivates, and is often a necessary adjunct to, experiments in this field. Indeed, in some cases, large theoretical extrapolations are our only understanding of these situations.

The hydrogen burning phase of the the main line of nucleosynthesis is now well understood after several decades of study. However, the rates of key reactions in the subsequent Helium, Carbon, and Oxygen burning phases are known only very poorly. These rates are crucial in determining the fate of a star (e.g., white dwarf or supernova), as well as the abundances of the various elements involved. Their present imprecision is due to theoretical uncertainties in the extrapolation of laboratory measurements at high energies to stellar conditions. Problems associated with nuclear structure are present also in the hot CNO and NaMgAl cycles, and there are significant uncertainties in our understanding of the synthesis of heavier elements, where the *r*- (and to a lesser extent, the *s*-) processes require theoretical descriptions of nuclei far from the beta-stability. In primordial nucleosynthesis, we need to explore the implications of the hadronization transition in the Big Bang, which could have induced fluctuations in baryon densities and relative *n/p* ratios. Basic input to the latter question can be gained from ultrarelativistic heavy ion experiments, given adequate theoretical effort.

The deficit of high-energy neutrinos from the sun remains a mystery. Much activity has been devoted to verifying the nuclear physics in the solar model and to identifying experiments beyond the present one involving ^{37}Cl . Recent excitement has been caused by the realization that neutrino oscillations might be enhanced by matter in the solar interior. Experiments in progress or planned, involving Gallium, heavy water, and proton decay detectors, will most likely aid in resolving this vexing flaw in our understanding.

Study of compact objects in astrophysics—neutron stars and supernova—raise fundamental problems in nuclear theory; nuclear theorists have played a leading role in the progress of recent years. With the discovery of the new supernova, Shelton 1987a, theorists have a remarkable opportunity now to understand the evolution and cooling of a neutron star from its birth. It is noteworthy that the observed neutrinos from SN1987a are consistent with theoretical expectations based on both the effects of electron capture on core structure and of neutrino degeneracy during the later stages of infall.

The theory of supernovae, in all its aspects, involves a subtle interplay of basic nuclear theory (e.g., electron capture rates, equations of state) with other areas of physics. The properties of nuclei and nuclear matter at high temperature and densities are central to a correct description here. It is exciting that we have begun to see comparisons between the data from heavy ion collisions and the equations of state required to explode a star, a connection that would have been unthinkable several years ago. It is a tribute to our theoretical understanding and model-building ability that we have come this far, but it is clear that more work will be required to proceed with confidence.

Fundamental problems in neutron stars are to understand the earliest moments of formation and the subsequent cooling, as well as their structure. These are issues in nuclear theory, involving achieving better knowledge of the nuclear equation of state under extreme conditions. The existence of possible unusual states of matter in neutron star cores can in the future be pinned down from cooling studies interpreted with the reliable theory. The cooling process can also constrain the couplings of exotic particles, such as the axion

and Majoron. Problems of sudden speedups of neutron stars likely involve interactions of neutron superfluids with normal excitations and vortices, and draw closely on related work in condensed matter physics.

III.8 Relation to other fields

It is clear from a number of topics discussed above that the intersection of theoretical nuclear and particle physics is a fertile one that will become increasingly important during the next decade. Indeed, aspects of the two fields are becoming indistinguishable, merging into the common disciplines of "hadronic physics" and "low energy tests". Conferences and workshops involving the two communities are becoming more common and are to be encouraged. Some collaborations between members of the two communities exist, but they are not as widespread as might be hoped. This should change as nuclear and particle theorists realize that they have something to teach one another.

The theory of the nucleus, a rich structure involving an extensive array of degrees of freedom at different scales, shares a common intellectual heritage with the theory of condensed matter systems, and has benefited enormously from interaction with this field, as well as contributing to it. For example, diagrammatic many-body theory originated in nuclear physics, while ideas of pairing and phase transitions in high spin states—the analog of superconductivity in high magnetic fields—were drawn from condensed matter physics. Notions of screening clouds and backflow surrounding quasiparticle excitations, with accompanying mass and wave function renormalization and their shedding at higher frequency, are shared in common with quantum condensed matter systems, as is the notion of effective degrees of freedom. The Landau Fermi liquid theory underlies the understanding of the shell model. Many-body methods (like the hypernetted-chain and coupled-cluster expansions and Monte Carlo calculations) find common application in nuclear and condensed matter systems.

Among ongoing interactions with condensed matter theory are the studies of chaos and multidimensional tunneling that we have mentioned above, Landau Fermi liquid theory as applied to relativistic descriptions of nuclei, and understanding of the role of the nuclear matter liquid-gas phase transition in nuclear fragmentation experiments. In the latter problem we must describe how nucleation occurs in a many-body system where Fermi statistics and strong correlations play significant roles. The physics of finite metal-atom clusters also has much in common with nuclear phenomena, as shell structure and collective deformation have been observed in these systems.

Nuclear and atomic/molecular theory share many concepts and techniques; both fields are concerned with finite quantal systems. Among the commonalities are structure techniques like the shell model, RPA, Hartree-Fock, coupled-cluster, and Monte Carlo methods and reaction techniques like few-body methods, coupled channels, TDHF, and semiclassical pictures. Relativistic models are under development in both fields. The algebraic techniques of the IBM have been useful in describing molecular vibrations. Although there has been little recent interaction between nuclear and atomic/molecular theory, we suspect that a renewed dialogue would be quite fruitful.

IV. People and institutions

The level of nuclear theory activity, the ability to play the role outlined in Section II and the fruition of the scientific opportunities of Section III are all ultimately dependent upon the people now doing nuclear theory, the institutional settings in which they work, the funds they have available, and a sufficient flow of talented graduate students into the field.

Accurate demographic and funding data are notoriously difficult to come by and time limited our ability to gather on our own. We have therefore relied on the data of others, which we use below with appropriate caution. In particular, we have the data supplied to us informally by the NSF and DOE nuclear theory program officers. We also have the results of the recent DOE manpower survey requested by NSAC, which include both supported and unsupported personnel in the DOE program. The 1986 survey shows 212 Ph.D.-level theorists involved in DOE programs, of which 175 were directly funded. Of these, the DOE theory program supported about 133 FTEs in that same year. The differences can be attributed to theorists incorporated within the experimental program, as well as unsupported and partially supported personnel, and to personnel receiving their funds from other sources (e.g., domestic and foreign fellowships). In addition, there were 12 theorists supported by the Nuclear Data program, which was incorporated into nuclear physics in FY 1988.

In our funding analysis below, we consider only DOE theorists supported by the DOE nuclear theory program. This is because we have difficulties in determining how much theory support does not flow through these channels. We note, however, that the key ratio of theorists to experimentalists is roughly invariant to whether all or only the directly supported DOE scientists are included. Of course, these data still allow an accurate analysis of the funding per funded theorist.

IV.1 How many theorists are there?

Virtually all of the funding for nuclear theory in the U.S. comes from either the NSF or the DOE. NSF support is almost exclusively for individual or small-group university-based researchers, while the DOE supports large efforts at five national laboratories (Argonne, Brookhaven, Lawrence Berkeley, Los Alamos, and Oak Ridge), six large university groups with four or more permanent faculty members, and a number of smaller university-based groups.

In Table 1, we show FTE theory personnel supported by the DOE and NSF nuclear physics programs (both theory and experiment).

Table 1: Personnel directly supported from Federal nuclear physics funds (FY86)

	Faculty/Staff	Post-doctoral	Total Ph.D.	Graduate Students
NSF U.	62	21.5	83.5	32
DOE (Lg. U. Grps.)	33	22	55	31
DOE (Sm. U. Grps.)	47	21	68	41
Total University	142	64.5	206.5	104
National Lab	46	18	64	7
Total	188	82.5	270.5	111

There is some internal consistency in these numbers. For example, assuming that a graduate student is supported for three years, there is an annual production of 37 Ph.Ds per year. If the mean post-doctoral tenure is four years (two two-year positions), then these 37 fill 20 post-doc positions, which is roughly in accord with the committee's perception of the true situation. We therefore conclude that there is no imbalance in the relative numbers of students and postdoctoral fellows supported.

The absolute level of personnel support is surprisingly meager. There are 0.7 graduate students for each supported faculty member in the universities, despite the fact that theoretical faculty typically carry two or more students simultaneously and there is high student demand for work in nuclear theory (see Section IV.4 below). Similarly, there are less than 0.5 postdoctoral fellows per senior Ph.D. throughout the entire field.

The disparities between the DOE and NSF are also interesting. Note that there is roughly one graduate student per faculty member in DOE supported groups, while only 0.5 in NSF supported groups. Similarly, at the larger DOE university groups, there is roughly 0.7 postdoctoral fellows per faculty member (0.5 at all DOE universities), while only 0.3 postdoctoral fellows in the NSF grants. This clearly reflects a difference in funding philosophy between the two agencies (the NSF intentionally provides partial support to many on the funding "margin"), but we believe that the DOE level of support comes closer to optimizing scientific output and might be a reasonable goal for NSF funding.

IV.2 What are they doing?

The activities of these theorists are diverse and consequently difficult to quantify. However, one measure is an analysis of the FY86 DOE effort supplied to us informally by the nuclear theory program office for that year, which is shown in Table 2. It must be borne in mind, though, that individual theorists usually work on several of these problems simultaneously.

Table 2: DOE effort in various subfields (FY86, %)

Few-body	12
Relativistic nuclear physics	11
Nuclear structure/reactions	11
Meson-nucleus interactions	11
Quarks in nuclei	11
Heavy ion dynamics	9
Collective models/giant resonances	7
Quark-gluon plasma	6
Symmetries and weak interactions	6
Nuclear matter	6
Hypernuclei	5
Nuclear astrophysics	4

We see no significant mismatches between the present distribution of effort (as opposed to the overall *level* of effort) and the opportunities for progress in the field. This we attribute to the fact that quality people will naturally focus their efforts on the interesting problems. It also suggests to us that efforts to “target” theorists programmatically will be counter productive.

IV.3 Is there enough theoretical activity?

It is our unanimous perception that there are not enough theorists working in nuclear science. Interesting ideas are not pursued, important calculations are not being done, and all of us have heard more than occasionally the complaints of our experimental colleagues about the lack of theoretical support for their activities. The primary reason for all of this is lack of people in the field. This shortfall is likely to be exacerbated when the major new experimental initiatives of CEBAF and RHIC are realized.

A more objective judgment of “quantity” can be had by a comparison between subfields. Table 3 presents AIP and unpublished NAS data from 1985. The fraction of scientists in each subfield who are theorists is listed; data are given for two age groups, as well as the total population.

Table 3: Theorist fraction in various subfields (% , 1985)*

Field	Age \leq 39	Age \geq 40	All ages
Nuclear	16	19	18
Elementary particle	47	30	37
Atomic/Molecular	37	30	32
Solid State	20	23	22
Plasma	43	44	44
Astrophysics	39	34	37
Optics	11	19	17
Total Physics	29	29	29

* Includes full and part-time employed Ph.D.'s only.

Other data support the nuclear physics number. The NSF programs and the DOE census results in FY86 involved 1050 Ph.D. level experimentalists and 307 theorists, giving a theory fraction of 22%, approximately consistent with the value of 18% in Table 3.

Nuclear science appears significantly deficient in theory. Perhaps even more disturbing is the age trend shown by these figures. In many of the subfields, the theory fraction is higher among younger scientists than it is in nuclear theory. This is consistent with our perception that fewer junior university positions are going to nuclear theorists.

IV.4 The universities

The universities are the keystone of the U.S. effort in theoretical nuclear physics. Like their counterparts in the national laboratories, university theorists are actively engaged in forefront research, generating the ideas and providing the insights that further the field and inspire new experiments. It is also their responsibility to recruit and train the students who will make up the next generation. No task is more important to the long-term health of nuclear science. If our field is to remain vital, we must continue to attract our share of the bright young people coming into physics.

Professorial faculty are, of course, central to nuclear theory activity at the universities. Approximately 10 junior faculty positions in the U.S. have been filled by nuclear theorists over the past two years, an encouraging increase over the previous numbers. Nevertheless, there remains a pressing need to increase the nuclear theory presence at first-rate universities and colleges. Faculty at these institutions play a special role, both in representing our field to the physics community at large and in attracting the very best graduate students. An overwhelming fraction of currently practicing nuclear theorists were trained in these "top" departments. There are many strong programs at such institutions (examples are faculty groups with three or more members at MIT, Illinois, Stony Brook, Texas, Washington, Maryland, Pennsylvania, Ohio State, Indiana, . . .; in addition, there are some very strong programs at universities with one or two faculty). However, a peculiar but serious problem has evolved at some of the most prominent universities. Choosing for purposes of discussion one of the standard rankings of "top-10" physics departments (note that this

is an overall ranking of departments across all fields of physics), we see from Table 4 that nuclear physics is adequately represented at prestigious universities. This attests to the vitality of the field and to the importance of nuclear physics in the educational process. The problem lies in the lack of theoretical faculty at some institutions, including some that have strong experimental programs. This deprives nuclear theory of access to an important pool of graduate students and weakens the experimental research and training program, both locally and nationally. It is difficult to build programs without a base. Hopefully, the vital experimental programs at many institutions coupled with opportunities provided by major new accelerator and detector initiatives (e.g., CEBAF and RHIC) will provide the leverage needed to achieve balanced programs.

Table 4: Professorial nuclear scientists at top-rated physics departments*

University	Theorists	Experimentalists	Total
U.C. Berkeley	0	3	3
Caltech	1	5	6
Chicago	0	2	2
Columbia	0	3	3
Cornell	0	0	0
Harvard	0	2	2
Illinois	5	10	15
MIT	7	8	15
Princeton	0	8	8
Stanford	0	3	3

* Alphabetical list of the top ten physics departments according to *An Assessment of Research-Doctorate Programs in the United States: Mathematical and Physical Sciences*, L. V. Jones *et al.*, National Academy Press, 1982. The selection is by their measure 8, "Mean rating of the scholarly quality of program faculty".

The long-term solution is to recruit and support the best students available and train them well enough and broadly enough so that they can compete favorably for junior faculty slots. Indeed, nuclear science is becoming increasingly attractive to many students. At a time when particle theory has considerably narrowed its immediate contact with experiment, nuclear science offers the many scientific opportunities discussed in Section III. There is near unanimous agreement among the university-based theorists we have talked with informally that the number of supported graduate students is now limited by funds, not the availability of qualified students, and that increased investment in students will yield large dividends in the near future.

For the present, strong support for theorists in the universities is clearly called for. As noted above, there is experimental nuclear science at many institutions where there are no professorial theory faculty. Experimentalists must continue to press their departments for theoretical appointments. We would also encourage the funding of theoretical

research faculty positions, in conjunction with these experimental nuclear research programs. While such arrangements are clearly less preferable than tenure-track positions, they would provide a theoretical "presence" and enhance the interaction between theory and experiment. They could also be used to broaden existing theoretical efforts to better overlap with experimental programs, and might also lead to professorial appointments for the individuals involved.

Theorists in small university-based groups (one or two people) are especially vulnerable in times of severe underfunding. Without adequate travel or visitor funds, they can be cut off from the rest of nuclear science and so become discouraged and unproductive. This seems a particular waste in view of our need to maintain a "critical mass" of talented theorists to attack many of the important problems posed by experiment. If a lone investigator becomes inactive, we lose his expertise and the graduate students he might train, and also have the possibility that nuclear theory will cease at his university.

We perceive a need to better integrate theorists from small university groups into the national program. This might be achieved by coupling regional investigators to some larger, central group, or to a national laboratory. A nuclear theory center, as described in V.2, could also fill part of this need. Such an integration would provide isolated theorists with the stimulation of a "critical mass" of other people in the field and keep them better informed of developments. Of course, the necessary travel funds must be available so that a lone investigator can attend major conferences, visit collaborators, and invite colleagues to his home institution.

Opportunities exist to strengthen our field in the universities. If we fail to exploit them, the weak representation of nuclear theory within physics will likely continue.

IV.5 The national laboratories

The role of nuclear theory groups at national laboratories is multifaceted. These groups have the critical size required to undertake large-scale and long-term programs, and have often done so successfully. They provide considerable theoretical support for experimental programs at lab-based facilities of national scope and the summer programs they run provide a stimulating atmosphere for visiting university theorists. The corresponding national lab efforts in elementary particle and condensed matter theory have created a truly multi-disciplinary physics environment in some cases. The national labs also play an important part in training postdoctoral fellows, by providing a coherent research-oriented environment in which new ideas can be created and nurtured.

In an era of tight funding, national lab groups have been forced to reduce the "discretionary" parts of their programs. This is because of the large fixed costs of permanent staff salaries. Incremental funding would be particularly effective here in maintaining the quality of the summer programs and bringing the number of post-doctoral and junior staff positions up to an acceptable level. The essential flow of young people into staff positions in national lab groups, with very few exceptions, has virtually ceased in the last few years. This trend threatens to undermine the intellectual vitality of the national lab theory effort. Closer collaboration with neighboring university groups should be encouraged, as a partial means of coping with this problem. For example, some level of funding should be provided to lab groups to support graduate students who elect to carry out their research in collaboration with lab staff members. This implies a corresponding commitment of

the lab staff to get involved more directly in the educational process, perhaps through the teaching of research-oriented minicourses.

V. Opportunities

V.1 Sources of new talent

The consensus of our Subcommittee is that the group of postdoctoral research associate applicants in FY87 was easily the strongest in at least a decade. Table 5 shows the numbers of postdoctoral applicants in nuclear theory at selected institutions during the 1986-7 academic year. Of these, approximately one third of the pool of applicants at each institution were judged to be serious candidates.

Table 5: Applicants for Ph.D.-level theory positions (1987)

Institution	Number
Caltech	75
CEBAF	54
Maryland	85
MIT	72
Ohio State*	57
Penn	23
Seattle	76

*Asst. Prof. Position

Graduate students trained abroad, particularly in Germany and Japan, make up a significant fraction of the nuclear theory postdoctoral pool. Of the applicants listed above, the fraction of those trained abroad averages about 35%. We also note that there are postdoctoral-level scientists working in the U.S. program who are supported by foreign funds.

Graduate students and postdoctoral fellows nominally trained in elementary particle theory are another element of the postdoctoral pool. With the confluence of certain aspects of nuclear and elementary particle physics, such people have naturally become attractive candidates for nuclear theory positions. We expect the number of such "cross overs" will continue to grow in the future.

Finally, we note the importance of undergraduate education in attracting new students into the field. Undergraduate experiences shape a student's choices in graduate school and an exposure to nuclear theory at this stage of his or her education can have significant consequences. University-based nuclear theorists must creatively exploit classroom lectures, supervised reading programs, and undergraduate theses to bring new talent into nuclear theory.

V.2 Nuclear theory centers

It is often said that incremental funding is most easily secured for new projects. While experimental programs can always point to new hardware or facility upgrades as the next step requiring additional funds, theory requirements, by their very nature, evolve more gradually. Indeed, the distribution of funds supporting theoretical physics 50 years ago would look very much the same as it does today, apart from computing. Centers are an exception to this generalization, and, with the recent NSF STC program, the idea of establishing one or more centers for nuclear theory has been widely discussed in the community.

The broad intellectual opportunities of nuclear theory and the need to focus efforts to meet these challenges make the creation of one or more centers for nuclear theory timely and potentially effective. There are many advantages of such an arrangement. By concentrating personnel and activities, centers would provide the critical masses of permanent and long-term people and the critical flow of visitors that can be so conducive to breakthroughs in the field. They could promote interactions between the various subfields of nuclear theory and between nuclear science and related fields. They would also attract and stimulate graduate students and young scientists.

Any center for theoretical nuclear physics must be created to serve the whole community and to act as a center for the broadest range of problems in the field. Although contemporary problems of nuclear theory must be the focus of a center, one of the larger benefits of a center for theory is its ability to promote the interaction of nuclear physics with other fields, such as high energy, condensed matter, atomic and molecular physics, and astrophysics. A center with national visibility will help to broaden the intellectual base of the field. It is also important that any center have sufficient contact with experimental physics, for example by having a number of short and long term experimental visitors, having experimentalists on its advisory board, and directing a fraction of its programs towards topics with immediate experimental contact.

Our recommendation that the funding agencies create one or more centers for nuclear theory is therefore contingent upon the centers' satisfying the following criteria:

- Centers should not be created at the expense of an adequately funded base program. Diversion of funds or personnel from the current program will do little to improve the field. At the very least, support for centers must come from increases in nuclear theory funding earmarked for that purpose.
- As in the creation of the NSF Institute for Theoretical Physics in Santa Barbara, centers should be chosen on the basis of an open national competition. Direct input from the nuclear science community must be sought in deciding the outcome of the competition, for example through the creation of a committee drawn from the community at large to advise the agencies on the selection.
- To ensure that a center have a broad national base, it should have a national advisory body that would shape its general activities and direction by overseeing the choice of programs and personnel. The character of the advisory body should reflect the aims of the center to encourage contact with experiment and other fields.
- Any center created must have a significant interdisciplinary component and an adequate contact with experimental nuclear science.

- Any new center should serve an important educational role, contributing to the training of students and postdoctoral fellows drawn from the general nuclear science community.

V.3 Summer schools

Summer schools can (and should) play an important role in the education of young nuclear scientists. As in all fields of science, they can provide an in-depth, yet pedagogical, survey of the field that typically cannot be gained from courses offered at the vast majority of graduate schools. Furthermore, the possibility of interacting with a sizeable number of peers with similar interests can lead to a sense of excitement, commitment and community. In the long run, these can be invaluable, both to the student, as well as to the health of the field. Ideally, at the end of a good summer school, a student should be aware of what the field actually is and the important problems facing it, why these problems are considered important, what experimental data exist, what experiments are being done and are planned and, of course, what techniques, both experimental and theoretical, are available for attacking such problems. In the best case, these ideas will be carried back to the students' home institutions for further dissemination.

While the above considerations apply to almost any field of science, there are special circumstances that make summer schools particularly important for nuclear theory. There is a great diversity of nuclear phenomena and in the models these phenomena have inspired. Because of the "sub-critical" number of senior nuclear scientists found at all but the largest universities, the typical graduate student is exposed to only limited aspects of this diversity. As a consequence, summer schools are almost essential in producing "complete" nuclear scientists.

Until recently, there has been a paucity of such summer schools in the U.S. But in the past few years, two schools have been organized on an annual basis: one at Hampton University, Virginia and the other at Georgetown University, Washington, D.C. The first of these is primarily inspired by CEBAF and is therefore more limited in scope. The latter, the "National Summer School in Nuclear Physics", has just completed its third and final year of support by the NSF. Specific topics are presented in depth by approximately four lecturers, other areas being covered by "regular" seminars. Both of these schools have, within their purview, been successful. Their organizers and lecturers are to be applauded for their efforts to fill a crucial need.

With the rapid changes in both the content and definition of nuclear physics it seems that the time is ripe for a continuing and comprehensive U.S. summer school. There are many questions and details that must be addressed to optimize such an activity. This is the proper task for the organizing committee. However, among the more outstanding issues are the following:

- Should the school be held in a single location or should its site change? A single location, especially a national lab, has certain logistical advantages, whereas a moving school assures geographical distribution and the possible avoidance of staleness. (The high energy community has opted for the latter approach.) Whatever the location of the school, it is of some importance that it be in pleasant surroundings and provide a physical ambiance conducive to interaction. Among the latter qualities, an isolated setting and common dining facilities are most desirable.

- What should be the school's scope? Should it be confined to theory or should it include an equal measure of experimental material? Even in the former case, it is most important to expose students to the physics of detectors and the anatomy of an experiment. This, of course, is in addition to acquainting students with the present status of experimental data and new facilities.

- Should each school cover all of nuclear physics, from "classical" nuclear structure to the standard model? Should the scope be an annual or local option? To what extent should general theoretical or experimental material not currently specific to nuclear physics be included (e.g., topology)?

- How long and how large should it be? The present schools last for two weeks and enroll approximately 40-50 students; this seems about right for allowing the desired interactions to develop. Alternatively, a larger, more ambitious school might last three or four weeks with twice as many lecturers covering a broader range of topics.

- At whom should the school be aimed? Ideally, it would be a group of graduate students and post-doctorals with a common knowledge of basic nuclear physics. The lecturers might be exhorted to tell the students what they would tell their own graduate students just starting out on a thesis problem. In any case, the school must not degenerate into a workshop or conference. It might also encourage student seminars in a supportive atmosphere engendered by the faculty, as has been done commendably at Georgetown.

- How should the school be funded? This is an issue that clearly must be considered by the funding agencies in the context of specific proposals. However, we would be disappointed if funding (rather than facility) limits prevented any qualified graduate student from attending at least one such school before receiving his or her Ph.D. In view of the importance of such a summer school, we strongly support adequate financing for such an endeavor.*

V.4 Computing

Two current developments in computing present major opportunities for nuclear theory. The availability of time at national Class 6 and 7 computing centers allows theorists to approach exciting computational projects that used to be impractical: lattice-gauge computations and quantum transport theory in three dimensions are among the areas where major progress can be expected. Drastic price reductions in increasingly powerful workstations can lead to increased application of proven, sophisticated methods such as self-consistent mean-field theory and the integral equations (Brueckner, hypernetted chain) of quantum many-body theory to a much broader range of phenomena. These two pieces of hardware, the supercomputer and the workstation, are not independent. Ideally, they are integrated so that the workstation (particularly its graphics capability) allows analysis of the supercomputer results.

To exploit these opportunities, it is necessary to make these tools more widely available. Access to the NSF-funded supercomputer centers has done much along this line in

* We note with satisfaction that the NSF has recently funded a three-year series of annual schools to begin in 1988 with a rotating site. This should address many of the concerns noted above.

recent years and we estimate that NSF-supported nuclear theorists used time on these machines worth about \$1 M during the previous year. The DOE nuclear theory program last year used supercomputer time worth \$3.6 M (\$4.8 M was used for all of nuclear science), but even this did not satisfy all of the requests. We also note that both the NSF and DOE fund supercomputer usage through a "tax", leaving little freedom for program officers to shift money into, or out of, computing in the course of balancing the overall needs of the field.

Most nuclear theory grants are too small to pay telephone and network connection charges for the long times needed to operate major programs at remote centers, or to reimburse universities for the expense of setting up network ties into their offices. Mechanisms for funding these costs need to be established. Moreover, while high-powered workstations are a great bargain, few grantees can choose to forego a year's post-doc or two years' summer salary to purchase such a system from their operating grants. A modest infusion of capital for these tools will pay great dividends in the quality of future theoretical research.

V.5 Agency strategies

In reviewing the Federal funding of nuclear theory, we noted two strategies that the agencies might employ to positive effect. The first is to include appropriate theoretical funding as an integral part of the cost of large experimental projects. As we have emphasized above, theoretical activity is an essential complement to experiment. In particular, it is necessary for the successful exploitation of the large facilities now at the forefront of nuclear science and is thus an appropriate charge in the total facility operations budget. This need has been recognized in a *de facto* way by many laboratory directors through their support of selected theorists with operating funds. Requesting appropriate funds for theoretical activity at the time the facility is proposed would legitimize and stabilize such relationships. More importantly, it could lead to increments in the total theory program as new facilities are constructed.

Our second strategy concerns the need for permanent program officers with a nuclear theory background. For many years, both the NSF and DOE have had a series of dedicated "rotators" who manage their respective theory programs for terms of one or two years. This rapid turnover has helped ensure a contemporary perspective in the management of nuclear theory funding, but it is not efficient, as by all accounts it typically takes almost a year to learn the job. Moreover, without faulting the performance of any specific program officer, we note that these circumstances make it difficult to formulate a coherent policy of funding for theory and to implement it consistently. Of greater importance, perhaps, the agencies lack an informed, credible, and consistent inside voice speaking for nuclear theory, or even advising them on any nuclear science matter from a theory perspective.

In our judgement, the advantages of permanent nuclear theorists as program officers at NSF and DOE outweigh those of the current rotator system. Indeed, there are trained elementary particle theorists at both the NSF and DOE permanently assigned as program officers in that field. Some at the agencies argue that the nuclear theory program is too small to warrant the full-time attention of one program officer; it is difficult for us to make a judgement on this. However, if so, giving a theorist responsibility for nuclear theory *and* some part of the experimental program should be considered. Such an arrangement might provide the much-needed inside theorist's perspective on experimental matters and is not unlike the role played by theorists on program advisory committees.

VI. Historical and present funding

Significant changes in the current pattern of funding theoretical nuclear physics are essential if we are to achieve the research goals described in Section III, to tap the pool of promising students described in Section IV, and to realize the opportunities of Section V. In this section, we review the present and historical pattern of funding by NSF and DOE, and propose a five-year plan for strengthening nuclear theory.

VI.1 Why theorists need funding

Direct salary support is the most obvious (and largest) expenditure of funds for nuclear theory. In short, the numbers of graduate students, postdoctoral fellows, and national laboratory staff are almost directly proportional to the support for these categories from NSF and DOE.* Summer salary support for university faculty is also essential. Faculty are free from teaching and other university responsibilities during the summers, so that this is usually their most productive time. In the absence of summer support to do nuclear theory, senior theorists often seek support from other sources, leaving their students unsupervised during months when they should be accomplishing the most.

Apart from personnel, what money buys a theorist is interaction, both with the rest of the theoretical community and with experimentalists. These interactions take a variety of forms, including brief visits to give a seminar, multi-week collaborative efforts by individuals, extended visits to larger groups, labs, or institutes, and attendance at conferences and workshops. A free-ranging and sustained sharing of ideas with others is the lifeblood of the working theorist and an essential element in the education of junior scientists. Out of these interactions, new science is generated and then refined under the scrutiny of the community. Interactions with others also educate theorists about new techniques and the latest work of their colleagues and give rise to collaborative attacks on difficult problems.

Interaction is particularly important for nuclear theory, for as we have shown in Section IV, there is rarely a critical mass of nuclear theorists anywhere. Junior theorists need these interactions to broaden their perspective and exposure. Senior theorists without them can go "stale" and continue a line of research that is no longer worthwhile. Theorists who cannot afford to attend important meetings, or who cannot visit with their collaborators, will seldom remain productive.

VI.2 The present situation

Table 6 shows the most recent complete data set available to us on the breakdown of NSF and DOE funding for nuclear theory. We consider only funding that flows through the nuclear theory program in each agency, ignoring theory funded from the experimental programs. It is difficult for us to know this latter amount quantitatively. DOE funding, which accounts for about 75% of the total, is split roughly equally between the universities and the national labs. NSF accounts for about one-third of the university funding.

* We ignore the few students who are never supported as research assistants during their graduate career. It hardly need be said that this possibility weighs very negatively in a student's choice of field.

Table 6: Annual funding for nuclear theory (FY 87 in \$M)

	NSF	DOE	Total
Universities	2.6	5.5	8.1
National Labs	—	4.5	4.5
Total	2.6	10.0	12.6

Of the \$5.5 M in DOE university funding, \$3.1 M went to the large groups and \$2.4 M went to the small university contracts.

It is interesting to ask what this funding buys, apart from the personnel discussed in Section IV. In Table 7, we list the current operating funds available to each university-based Ph.D. nuclear theorist (university faculty and post-doctoral fellows).

Table 7: Annual operating funds for each university Ph.D. nuclear theorist (\$k)

	NSF	DOE
Travel	1.5	2.2
Computing	0.5	1.8
Publications	0.7	0.6

Note that the figures are roughly comparable between the two agencies and are astonishingly low. Particularly disconcerting is the level of travel support, given the importance of interaction with others and the current cost of both domestic and foreign travel.

The inadequacy of support for each nuclear theorist can be judged from the data in Table 8, which lists the total support per senior Ph.D. in various theoretical activities.

Table 8: FY86 support per senior Ph.D. (\$k)

NSF Nuclear Theory	42
DOE Nuclear Theory (Lg. University Groups)	85
DOE Nuclear Theory (Sm. University Groups)	57
DOE Nuclear Theory (All universities)	71
NSF Elementary Particle Theory	56
DOE Elementary Particle Theory (Universities)	56
NSF Atomic Theory	33
DOE Atomic Theory	77
NSF Theoretical Physics (All fields)	51
NSF Theoretical Chemistry	65
NSF Theoretical Condensed Matter	62

The evident disparity between DOE and NSF nuclear theory funding reflects differences between the two agencies in the per capita support of post-doctorals and graduate students, as the per capita operating funds are nearly equal (see Table 7). While the DOE support level per nuclear theorist is comparable to that in other fields, the differences between support levels among the various NSF subfields are marked.

VI.3 Funding history

It has long been widely recognized that the activity in theoretical nuclear science is insufficient relative to experiment and that an increase in the fraction of nuclear science funding supporting theory was required. For example, in 1977, Section 4.6.7 of the report of the *Ad Hoc* Panel on the Future of Nuclear Science (published by the National Academy of Sciences) contains the following:

We further advocate as a longer-term goal, say within ten years, an increase in the fraction of nuclear scientists who are theorists from the present (~ 20 per cent) to about 30 per cent. This increase is equivalent to ~ 10 per cent of the nuclear science operating funds in fiscal year 1987 allocated to support nuclear theory, compared with the present 6.2 per cent.

The 1979 Long-Range Plan (LRP), prepared by NSAC, endorsed this recommendation and contained a budget projection that implemented it. A growth of theory funding from 6.9% of operating funds in 1979 to 10.4% in 1986 was envisioned. It is apparent from Table 9 that this recommendation has not been followed. Indeed, it is the only major recommendation of that document that has not been implemented. It is evident that theory funding has not kept par with projections and that a 75% increase of the FY87 level (\$9.4M to a total of \$22.0M), would be required to meet the LRP recommendation of 10.4%.

Table 9: Federal funding for nuclear science (\$M)

	FY81	FY82	FY83	FY84	FY85	FY86	FY87	FY88*
DOE Theory	7.0	7.7	8.2	9.0	9.3	9.0	10.0	10.5
NSF Theory	1.8	2.3	2.2	2.3	2.7	2.5	2.6	2.8
Total Theory	8.8	10.0	10.4	11.6	12.0	11.5	12.6	13
DOE Experiment†	96	107	112	124	136	134	154	162
NSF Experiment‡	23.7	24.0	25.3	30.3	34.3	34.6	35.8	38
Total Experiment	119.7	131	136.3	154.3	170.3	168.6	189.8	200
Theory/Total (%)	6.8	7.1	7.1	7.0	6.6	6.4	6.4	6.2
NSAC LRP (%)	7.2	7.4	9.8	9.8	10.2	10.4	—	—

*Approximate

†Operating only including facility operations and CEBAF/RHIC R & D; SBIR and nuclear data funds subtracted

‡Operating only

In preparing this table, we have not considered funds allocated for supercomputer use. We regard supercomputer funding as somewhat analogous to the allocation of capital equipment funding for accelerator development in the experimental program. The problems we have been addressing in the theory program are largely caused by inadequate funds for normal operating budgets, particularly salaries for young theorists. As discussed in Section V.3, we estimate that about \$4.8M of supercomputing funds were used by nuclear theorists in FY 87. This is an extremely important resource for the field, and must be maintained. However, our assessment is that theory suffers from a shortage of manpower, and indeed, the 1979 Long Range Plan Recommendation is phrased in this context.

The 1986 report *Physics Through the 1990's—Nuclear Physics* perhaps best summed up the situation:

Although the NSAC 1979 Long Range Plan stresses the need for increased support of nuclear theory, a comparison of the current FY 1984 budget for nuclear physics with the FY 1979 budget shows that during the intervening years, funding for nuclear theory has remained essentially constant as a percentage of the whole (5.8% in FY 1984 versus 6.0% in FY 1979). We believe that there is still a clear need for a substantial relative increase in the support of nuclear theory, especially in light of the new and challenging frontiers that are opening up in nuclear physics.

Little has changed in subsequent years, and, if anything, it has gotten worse. Some further measure of the slippage can be noted in the fact that the number of dollars supporting each NSF nuclear theory PI (\$42 K) has remained constant from 1983 to 1986. Because of the high fraction of theoretical support devoted to salaries, static budgets in the face of increased costs necessarily translate immediately into lost positions for students, postdoctoral fellows, and sabbatical visitors, curtailed travel, and loss of university summer salary. Some of the major DOE university groups have lost one or more postdoctoral/student positions in recent years. In the national laboratories, a similar effect has taken place, as funds for visitors' programs, travel, and post-doctorals are squeezed out between constant funding and inflation-driven salaries for senior staff. For example, the very productive and important summer visitor programs at several national laboratories have been severely reduced or terminated entirely in recent years.

Theory support relative to experiment now stands at an historically low level. The erosion in support for theory has continued despite broad support for increased theory budgets by the entire nuclear science community, including specific recommendations by NSAC. With several major experimental facilities on the horizon, the necessity to provide for the present and future theorists who can guide and interpret experiments seems particularly urgent.

VI.4 A five-year plan for nuclear theory

It is not difficult to define what is a prudent level of theoretical activity and support, to devise a plan for reaching this level over the next several years, and to calculate what the whole will cost. We found it useful to engage in such an exercise. Perhaps not surprisingly, we find that an appropriate level of nuclear theory funding is significantly greater than the

present one; the required increment is close to the \$9.4 M (FY88) of the 1979 Long Range Plan.

Our assumptions are the following (all in constant FY88 \$).

- "Catch-up" funding for DOE groups is required just to preserve the level of effort that existed two years ago. We've assumed \$1.2 M, which could represent 12 sabbatical visitors and 12 postdoctoral fellows (alternatively, part could be in travel funds). We've also assumed an NSF increment of \$1.5 M, which would bring the funding for each faculty member to \$63 k, roughly in line with the other fields shown in Table 8. Thus, the total "catch-up" increment is \$2.7 M.
- Strong groups now in existence can absorb approximately 24 additional postdoctoral fellows. This would increase existing staff levels to two postdoctoral fellows for every three senior Ph.D.s in the top ten research groups and to approximately one fellow per two senior Ph.D.s in the next ten.
- Six new research assistant professors and ten new tenure-track faculty will be appointed at universities by 1993. This is roughly one research faculty and two professorial faculty per year, not unreasonable in view of current trends.
- Eight new national laboratory staff are added in the five-year FY 89-93 period, as the CEBAF and RHIC theory groups are formed.
- The student pool expands to feed the enhanced postdoctoral program. This occurs about 2.1 times faster than the postdoctoral expansion so as to maintain the current postdoctoral/student balance and leads the postdoc expansion by two years.
- Our assumed costs (including overhead and fringe benefits) are:

- Grad student: 17 k
- Post doc: 56 k (includes 1.5 k travel, 1 k publications)
- Research faculty: 95 k (includes 3 k travel, 1 k publications, 3 k visitors, 8 k secretary)
- Professorial faculty: 75 k (includes summer salary, 3 k travel, 3 k visitors, 8 k secretary, 28 k for 0.5 postdoc, and 17 k for student)
- National lab staff: 160 k (includes 0.5 postdoc)

- A theory institute will be created with start-up funding of 0.5 M in FY89 and annual operating costs of 1.5 M thereafter.

These assumptions lead to the cumulative personnel schedule shown in Table 9.

Table 10: Cumulative personnel increments (FY89-93)

	Postdoctoral	Student	Research faculty	Prof. faculty	National Lab
FY89	6 + (1.5)	25 + (3)	3	2	1
FY90	6 + (3)	38 + (6)	6	4	3
FY91	12 + (5)	50 + (11)	6	6	5
FY92	18 + (7)	50 + (15)	6	8	6
FY93	24 + (9)	50 + (19)	6	10	8

The numbers in brackets are the personnel increases that accommodate the increases in faculty and national lab staff. In Table 10, funds for these are included in the figures for the faculty/staff. The other numbers are the ramp-up to satisfy the needs of existing faculty (and their replacements) on retirement. By assumption, the entries for postdoctoral fellows and graduate students outside the parentheses saturate in FY93. However, Table 9 can be extended into FY94 and beyond by incrementing the last two columns. Then the figures in brackets in the first two columns would continue to increase. We view the research faculty positions as a wedge for creating new positions in some universities, not an end in themselves, so that they are not ramped up. The Ph.D.-level positions associated with the theory institute must be added to these totals.

These personnel then imply the cumulative funding increments shown in Table 10, expressed in FY88 dollars.

Table 11: Cumulative theory funding increments (\$M in FY88 dollars)

	FY89	FY90	FY91	FY92	FY93
"Catch-up"	2.70	2.70	2.70	2.70	2.70
Post-doctorals	0.34	0.34	0.67	1.01	1.34
Students	0.43	0.65	0.85	0.85	0.85
Research faculty	0.29	0.57	0.57	0.57	0.57
Prof. faculty	0.16	0.31	0.47	0.62	0.78
National lab staff	0.16	0.48	0.80	0.96	1.28
Theory center	0.50	1.50	1.50	1.50	1.50
Total	4.58	6.55	7.56	8.21	9.02

Significant as these increases might seem, they still fall somewhat short of what would be required to meet the recommendations of the *Long Range Plan* of 1979. Our plan leads to a net increase of some 60 Ph.D. level personnel after five years, which, if the number of experimentalists didn't change, would bring the fraction of all Ph.D.-level nuclear scientists who are theorists to about 26%. The "steady state" funding reached in FY93 (\$13.3M, the projected FY88 budget, plus \$9.0M, the recommended increment in FY88 dollars) would be \$22.3M. This is approximately 10% of the total FY88 nuclear physics budget excluding facility construction. More specifically, we are suggesting that an immediate FY89 increment of about \$4.5M (about 2% of the nuclear physics budget), ramping over five years to an asymptotic increment of about \$9M, be invested in a balanced way between a strengthening of existing groups and new initiatives. This will raise the national nuclear theory effort to a level commensurate with the intellectual challenges and experimental opportunities awaiting us and with the obligation to provide highly trained scientific manpower for the nation.

APPENDIX A

UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

*College of Arts and Sciences
Office of the Dean*

December 11, 1986

Professor Stephen Koonin, California Institute of Technology (Chairperson)
Professor Ralph Amado, University of Pennsylvania
Professor Gordon Baym, University of Illinois
Professor Carl Dover, Brookhaven National Laboratory
Professor Wick Haxton, University of Washington
Professor Franco Iachello, Yale University
Professor Art McDonald, Princeton University
Professor Ernest Moniz, Massachusetts Institute of Technology
Professor Philip Siemens, University of Tennessee
Professor Jeffrey West, Los Alamos National Laboratory

Dear Colleagues:

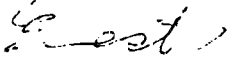
Thank you one and all for agreeing to serve on a Nuclear Science Advisory Theory Subcommittee for Nuclear Theory. This Subcommittee reports to the parent committee and through it to the Department of Energy and the National Science Foundation. The charge to the Subcommittee is as follows:

1. By considering the likely key research areas over the next five to ten years, the Committee shall determine the needs and opportunities which exist to strengthen the vitality of nuclear theory, and outline resources (e.g., special computing needs) required to support theoretical research in key areas.
2. The Committee shall make suggestions for ways to strengthen the nuclear theory presence at universities, and determine if any special efforts are required to attract outstanding students to the field.
3. The Committee shall determine the advantages and disadvantages of developing special mechanisms to strengthen nuclear-theory such as one or more national nuclear theory centers, summer schools, summer institutes, national laboratory workshops, or institutes, etc.
4. The Committee shall advise on ways to optimize the synergism between theory and experiment.
5. For areas of research which overlap other fields (e.g., particle physics), the Committee shall determine in what manner the research quality can be optimized.

We request that the Committee file a report with the parent committee prior to the Autumn 1987 NSAC meeting, which is likely to be sometime in October of 1987.

Again, I thank you for your willingness to serve on this important committee.

Sincerely yours,



Ernest M. Henley, Chairman
Nuclear Science Advisory Committee

EMH/vp

cc: D. Hendrie
H. Willard
Members of Nuclear Science Advisory Committee

**NUSAC Theory Sub-Committee
Agenda
(Caltech, 2/23-24/86)**

Monday 2/23

- 9:00 Opening remark (Henley?/Koonin)
- 9:15 Funding/Demographics (B. Barrett, W. Thompson)
- 10:15 Coffee Break
- 10:30 (What are the field's problems? Opportunities? What resources are needed?)
- 12:00 Lunch (Atheneum Card Room)
- 1:00 Research Areas

(Nuclear Structure/Many-body, Few-body, Light ions, Electromagnetic probes, Hadronic probes, QCD, Relativistic HIs, Low-energy HIs, Symmetries/Weak interactions, Nuclear Astrophysics)
- 3:00 Coffee Break
- 3:30 Discussion (Where is the overlap with other fields? With experiment? Should this be improved? How?)
- 4:30 Discussion — (computing)
- 5:00 Adjourn
- 6:30 Dinner

Tuesday 2/24

- 8:30 Discussion (Special problems at universities, funding mechanisms/strategies)
- 10:30 Coffee break
- 10:45 Outline of report/writing assignments/schedule next meeting
- 12:00 Adjourn (Lunch in Athenaeum)

Agenda for the Meeting of the
NSAC Subcommittee on Nuclear Theory
Forrestal Building, Rm. GE-036
May 7, 1987

- 8:45 Opening remarks (S. E. Koonin)
- 9:00 Comments from the "public" and general discussion
(S. Pieper, S. Wallace, G. Brown, D.-H. Feng, ...)
- 11:00 What are the 3-star problems in nuclear theory?
- 11:30 The wisdom of institutes, centers, temporary
concentrations
- 12:30 Lunch break
- 1:30 Human resources
(What are appropriate levels for students, post-docs,
faculty, national lab staff?; how to achieve them)
- 2:00 University issues
(Wisdom and level of subsistence grants; what is a
critical mass?; support of research faculty; DOE/NSF
funding disparity)
- 2:45 National lab issues
("New" groups at CEBAF/RHIC; need for post-docs,
visitors; balance with universities; improving contacts
with universities)
- 3:30 Other issues
(Balance and contact with experiment; summer schools; a
permanent nuclear theory presence at the agencies;
computing)
- 4:15 Review of existing draft and future writing
assignments; schedule of subsequent meetings
- 5:00 Adjourn