

# **Transitions in Engineering Education and Research: An Interim Report from the Field<sup>1</sup>**

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## **Preface**

This article was invited for inclusion in a volume dedicated to Dr. A. J. Carlsson on the occasion of his retirement in 1998 from the position of President of the Royal Institute of Technology in Stockholm, Sweden. The theme of the volume was academic leadership, and perspectives were provided by scientists, engineers, philosophers and economists, among others. Before becoming president of KTH, Dr. Carlsson had a distinguished career as a researcher and scholar in the field of solid mechanics with special interest in fracture mechanics. The pass which follows is a collection of personal impressions on trends in research and education in the field of mechanics.

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<sup>1</sup>From *Science, Technology and Society: University Leadership Today and for the 21st Century*, edited by I. Grenthe et al., Royal Institute of Technology, Stockholm, 1998, 203-223.

## **Introduction**

Ideally, the university is viewed as humankind's repository of accumulated knowledge. Its central roles are to share that knowledge base with those who can benefit from it and perhaps to add to the store of knowledge along the way; these roles are fulfilled through the intertwined activities of teaching and research. It is stable, reflective, circumspect and tolerant. In contrast, a view formed in the course of involvement in the day-to-day work of a research university in the United States over the past few decades is that the university is but one strand, albeit a principal one, in the complex fabric of our society. It interacts with a host of other institutions, it must respond to a variety of outside influences and it must strive to fulfill a diverse range of expectations. In reality, the university is in a state of continuous transition. The task for academic leaders, which can be characterized as making concrete and compelling plans in preparation for an ever less definite future, is becoming more daunting.

This essay includes observations on some issues which have had or are having an impact on the environment for science and engineering within the university in the US, particularly for the field of mechanics of solids. The pressures come from both within and outside the university, and they are having the effect of fundamental changes in the way in which the mission of education and research is being pursued throughout the country.

Various issues along this general theme are discussed in the sections which follow. First, the field of mechanics of solids is characterized, without getting too deeply involved in its details, and the main intellectual influences which have shaped the modern frontiers of the field are identified. This is followed by a discussion of external pressures on many branches of science and engineering, including solid mechanics, arising from both popular crusades and national research strategies. Finally, some thoughts on education, viewed as a means to prepare for events which cannot be foreseen, are included.

## **Mechanics of solids as an engineering science**

Engineering science is the term used for the study of either natural or artificial (man-made) physical systems or processes for which the knowledge gained is of potential use in engineering applications. This is the link between science, the study of natural systems in order to discover new knowledge or to improve fundamental understanding, and engineering, the implementation of accumulated knowledge in altering nature's course for the betterment of humankind. A scientist deals with original ideas without regard for their utility, while an engineer creatively implements established knowledge in designs which incorporate as few original ideas as possible. The role of the engineering scientist is to link these activities by integrating physical postulates, laboratory tests, and techniques provided by science into forms suitable for use in engineering. While the US is widely regarded as preeminent in basic science, it has focused only a relatively small national effort on this intermediate step.

The mechanics of solids is an engineering science which underlies much of the practice of mechanical engineering and civil engineering. It is also an important component in materials science, aerospace engineering, architecture, geophysics and other fields. More generally, mechanics is the science concerned with the response of physical objects to the action of forces impressed upon them. It was among the first of the quantitative sciences to be developed. It is a wonderful combination of extraordinary beauty in its mathematical structure and experi-

mental foundations, and of remarkably broad utility in its application. Discoveries of the 20th century have revealed that the descriptions of physical processes on the atomic and the galactic scales require the new perspectives of quantum mechanics and relativity, respectively, but this has not diminished in the least the status of mechanics as a cornerstone of physical science or its importance to engineering. Over the years, mechanics has become divided into branches concerned with discrete systems (interacting particles and rigid bodies) and with continuous systems (deforming solids and fluids) and, within the past few decades, the latter branch has again bifurcated into the more or less separate engineering sciences of mechanics of fluids and mechanics of solids.

The discipline of Applied Mechanics, including all the principal branches of mechanics, became firmly established in its own right in the US with the founding of the Applied Mechanics Division of the American Society of Mechanical Engineers (ASME) in the late 1920s. The activities in solid mechanics at the time were motivated by needs in mechanical engineering design, but the work already had a strong analytical or engineering science flavor. This point of view was given additional impetus with the creation of the Program of Advanced Instruction and Research in Mechanics under the direction of W. Prager at Brown University in the early 1940s (later to become the Division of Applied Mathematics) and by the publication of the series of advanced mechanics texts by S. Timoshenko. The mechanics of solids was pursued as an area within applied mathematics for several decades to follow, and remarkable results of permanent value were achieved: manageable equations describing the behavior of thin shells, bounding theorems for plastic limit loads, stress wave solutions for plastic solids, wave dispersion characteristics in plates, finite strain elasticity, full field solutions for plastic flow during metal forming, and so on.

This was the background for a typical program of graduate study in the mechanics of solids in 1960. Most programs in the US were strongly inclined toward mathematical studies. A graduate student in such a program would take courses in elasticity, plasticity, viscoelasticity, thermoelasticity, wave propagation, nonlinear continuum mechanics, dynamics and vibrations, each with a strong focus on techniques for solving boundary value problems or initial value problems. In addition, a typical program of graduate study included courses on general methods of mathematical physics, partial differential equations, integral equations, calculus of variations, and possibly on functional analysis, numerical analysis, and theory of functions of real variables. Most boundary value problems dealt with simple geometrical configurations, materials were assumed to follow highly idealized models in their response to stress, and the problems were well-posed, for the most part. Asymptotic methods played a significant role when complete analytical solutions could not be obtained; numerical methods, which were viewed with considerable skepticism, provided the approach of last resort.

It was a heady time. Research funds were plentiful, thanks to the launch of the little satellite Sputnik by the Russians in 1957. The number of journals publishing research papers in solid mechanics was already substantial, but modest enough so that a diligent graduate student could actually follow the literature in his or her field by spending a few hours per week in the library. Universities were expanding their graduate offerings and academic positions were available for advanced degree recipients. The cold war was being waged aggressively, and the national laboratories provided additional opportunities. The US economy with its huge

manufacturing base was booming, offering further professional possibilities. The circumstances were viewed as a stable steady state by those who had experienced them for only a few years of graduate study. In retrospect, however, the signs of further evolution and transition were present.

### **Some changes in focus for the field**

Beginning in the 1960s, the forces which shaped the field in the decades which followed were derived from the potential for the numerical finite element method as a practical tool for stress analysis and the potential for much more effective use of materials in engineering through quantification of the connections between microstructure and engineering properties. This influence was so pervasive that, by 1985, those who had begun graduate study and research more than 20 years prior were justified in claiming that the graduate programs they had followed had vanished, and had been replaced by programs in which numerical methods were as important as analytical and experimental methods, and in which studies of traditional topics were replaced by studies of mechanics of materials at the level of microstructure. Because of the impact of these two changes, it is worthwhile to recall their evolution within the field. In each case, the effort was precipitated by individuals with the perception to see that there was a need to be addressed and that the prevailing circumstances offered an opportunity to make significant progress in meeting the need. This interplay of need and opportunity is one of the key factors in most important advances in the field. Of course, serendipity is usually a factor, as well.

The digital computer revolutionized many areas of engineering and science, and solid mechanics was among the first fields to exploit its potential. Many computational techniques have been useful in the field, but the one having the greatest impact by far is the finite element method. The origins of the method can be traced back to work in the mathematical sciences by R. Courant in the 1940s on finding approximate solutions of elliptic partial differential equations by enforcing them in the weak sense and by a number of people (M. Turner, R. Clough, J. Argyris and others) working on aerospace applications where large networks of beam-columns were analyzed by the matrix methods of structural analysis. The basic idea of the method is to divide the domain of a continuous solid body into a finite number of sub-domains, or elements, and then to adopt an interpolation scheme for representing the particle displacement throughout each element in terms of displacements at a small number of boundary points called nodes. A model of material behavior is adopted, and the approximate deformation fields are required to abide by the principle of virtual work for arbitrary virtual variations of the nodal displacements. This generates as many simultaneous equations for the unknown nodal displacements as there are unknown displacement components in the system. The problem is ideally suited for numerical solution by digital computer. A great deal is understood about accuracy and convergence of numerical solutions for broad problem classes at this juncture, and computational mechanics has assumed its place, along with analytical mechanics and experimental mechanics, among the principal methodologies of the field of solid mechanics. Interestingly, the availability of this tool has enhanced the roles of both analysis and experiment for the field, mainly because the questions which can be posed for investigation have become sharper and better informed as a consequence of numerical stimulation studies.

The introduction of the finite element method has permanently altered the field. While

the method can provide only approximate solutions to boundary value problems, it has greatly diminished the exposure of graduate students to classical analysis and its scientific applications, and it has dramatically reduced the effort devoted toward searching for analytical solutions of difficult boundary value problems. The finite element method has led to a wondrous array of approximate solutions to boundary value problems for nonlinear materials or for large strains that could not be penetrated by classical methods of mathematical analysis, and the field is richer as a result. On the other hand, the method has also led to “solutions” of boundary value problems for which it is known that solutions do not exist. Due to its ease of use, it has also led to publication of large numbers of inconsequential research papers presenting routine numerical results (thus repeating a similar experience with analytical solutions of routine boundary value problems some years earlier). When all the pros and cons are weighed, however, it is clear that the introduction and development of the finite element method is one of the major advances of solid mechanics in the 20th century. The method has been extended to the study of boundary value problems in the traditional areas of electromagnetic fields, heat transfer and mass diffusion, and it is currently being used as a basis for study of atomistic models of material behavior and the coupled influence of strain on quantum mechanical transport in semiconductor materials on the sub-micron scale. It is a powerful tool which can magnify by a hundred fold either the insight or the ignorance of an analyst.

As with the advent of the finite element method in the field, the development of mechanics of materials was also the result of a conjunction of need and opportunity. During the 1950s, those experts advising US government agencies, particularly the Atomic Energy Commission, on matters of science and technology detected a pervasive mismatch between the expectations of engineers for material performance and the ability of suppliers to produce materials for engineering use. A plan was developed whereby the federal government would establish materials research institutes at several leading US universities, providing buildings, equipment and operating budgets. However, there was deep consternation about this degree of involvement by government agencies in academic institutions, a situation which can hardly be imagined today, and the plan was not implemented.

Further study revealed the need for improved materials technology to be even more urgent than first thought, and a broad multi-agency panel was formed to address the matter. Out of this effort, the Advanced Research Projects Agency was assigned the mission of supervising interdisciplinary programs of research in materials science at a cross-section of universities. The main mission was to educate relatively large numbers of graduate students in order to staff the effort to address the perceived national problems in materials technology.

The common viewpoint of the day was that materials science is not a discipline. Instead, it was viewed as an integrative activity whereby a broad range of scientific ideas is brought to bear in manipulating raw materials into engineering materials which can be used in achieving specific goals. Nowadays, the field of materials science has taken such a prominent role in higher education of scientists and engineers that there is little point in debating the question of discipline status any further. In fact, in a recent poll, approximately one half of the university faculty members in all engineering departments in the US judged that they were engaged in materials research. Nonetheless, it is true that a comparison of the activities of the departments of materials science at any two universities will sometimes reveal differences as great as those

between physics and mechanical engineering, for example, at any single institution.

The original group of materials research laboratories focused on electronic and optical properties of materials; within a few years, the scope of the effort was broadened to include structural properties. Some of the leading researchers interested in mechanical and structural properties were formed into a cohesive group which held annual workshops, with the traditional areas of mechanics, metallurgy, ceramics, polymers, physical chemistry and solid state physics represented. The workshops were tremendously productive and the excitement of them quickly spread throughout the graduate programs of the home institutions of the participants. The new field of mechanics of materials had been born.

The nature of curricula in graduate programs of study in mechanics of solids at several leading universities began a gradual transformation. Exposure of students to topics concerned with plates, shells, fluid dynamics, and classical dynamics gave way to the study of crystallography, thermodynamics of solids, dislocation mechanics, and failure mechanisms. The physical character of these topics was accompanied by a resurgence of interest in experimental mechanics applied on a small size scale. The focus in graduate courses changed from techniques to physical phenomena.

Over the past twenty-five years, solid mechanics has contributed much to an ever increasing understanding of the factors which determine the mechanical behavior of engineering materials. The view that solid mechanics can be used systematically to improve the performance of materials or as a basis for the design of material microstructure to fulfill certain technological objectives is now widely accepted. Furthermore, in an area such as microelectronics, mechanics research has been essential to controlling stress driven defects during processing and fabrication even though load carrying capacity is not a design function of the system. The evolution of this view has had a profound impact on the growth of the field of mechanics. At this juncture, the formerly distinct boundaries between the areas of mechanics of solids and materials science have disappeared, and there is currently a continuum of activities connecting the areas. Work at this intersection is broadly based on the traditional quantitative methodologies of solid mechanics and the observational methodologies of materials science. The area of mechanics of materials is unique in its blend of interests, the range of issues covered, and the interaction among experiment, theory and computation that is essential for progress.

## **Popular crusades against higher education**

Turning to external pressures on higher education, one category concerned with the intellectual substance of curricula is common to all disciplines, including English and Sociology and Geology, as well as Engineering. This pressure is most evident in recent books with titles such as *The Closing of the American Mind*, *Profscam: Professors and the Demise of Higher Education*, *The Moral Collapse of the University* and *Killing the Spirit: Higher Education in America*. It would be an oversimplification to claim that the substance of the arguments in these books can be represented with a few summary remarks. Rather, the point is to note that books of this kind exist in surprisingly large numbers. In effect, these books try to make the case that universities actually suppress those characteristics in students that are most admired in graduates, characteristics such as independence, curiosity, analytical problem solving capabilities, rational discrimination, and a willingness to express views for debate.

The kind of criticism contained in these books cannot be attributed to mere nostalgia on the parts of the writers. Admittedly, when someone gets to be a certain age (as many of us have) it is easy to fall into the trap of thinking that civilization reached its peak when we were undergraduates, but has been in a steady decline ever since. This criticism is deeper than such nostalgia.

It is also recognized that this kind of attitude is not new. In an 1842 essay on higher education in America, Francis Wayland, then President of Brown University, expressed the view that the curriculum of the typical university of the time was bloated and confused. He wrote that “We have so crowded it with studies as to render it superficial and probably useless.” This kind of internal critical examination has been going on as long as universities have existed, and it is generally informed, healthy and constructive.

What is different between the remarks of President Wayland and the books mentioned above is the breadth of the current pressure, and the fact that the challenge is being played out in the book review sections of daily newspapers and on TV talk shows, rather than in professional circles. When examined closely, these kinds of books either promote particular political or social agenda, or the authors have a grievance on some point for which retribution is sought, with pious self-assurance, in the form of public embarrassment of universities. The authors are generally eloquent and capable of making a case very forcefully. Furthermore, in any issue under debate, some number of examples can invariably be found which can be used as evidence to support any point of view, and these can be used without counterpoint to great effect in order to lend credibility to the arguments. And finally, when the case is indeed made that something important has been lost in higher education, the question of whether or not something of equal importance has been gained should also be considered, but these authors generally fail to do so. For example, today's students are far more involved in the communities in which they live than were students of a couple of generations ago. They also share a greater responsibility for their own educations than previously. Such additions should be weighed in the balance, but they are more commonly overlooked.

The kinds of views reflected in these books can be regarded as coming from those inherently unfriendly to research universities. These unfriendly voices should be heard, but the temptation to respond in kind to them must be resisted. There is reason to expect that the antagonists will tire of their crusades and will go away after a while without having done much harm.

## **The evolving national research strategy**

A second source of pressure on research universities, which is coming largely from nominally friendly sources and which is particularly acute for engineering and science, is leading to a substantial alteration of the economic foundations of research universities. This effort is being led by the US government, to a large extent. (Admittedly, whether or not this is friendly pressure is open to debate. For many people, the claim that “Your government is here to serve you” ranks along with “The check is in the mail” as among the great lies!) This is an extremely complex issue, but consideration of it can be focused by stating a few questions which seem to be at the heart of the current debate. For example,

- Can US society afford basic research? Is it a necessity or a luxury?

- Is the present size of the national research enterprise too large (or too small) for the post-cold war economy?
- Is the prevailing model for basic research in need of adjustment?

The issues raised are very difficult to define with any precision, and the prospect of finding consensus on unambiguous answers is remote. Nonetheless, it is important for university administrators and faculty members to listen to their critics on these matters, even if what these critics say is appalling to us. There are government officials and business leaders who want to know “what have you done for me lately, how much did it cost, and what was the return on that cost in the last fiscal quarter?” The questions are going to be answered, and it is better if the academic community is involved in formulating the answers. Ultimately, the answers perceived to be true at the policy level will dictate the form and shape of research universities in the years ahead.

Can the US afford basic research? The case for societal payoff from basic research is so overwhelming, in medicine, materials, communications and air transportation, for example, that the value of basic research to society should by now be among those truths held to be self-evident. It simply must be pursued as one source of vitality to society, along with other things important to culture. Indeed, it can be argued that research is the only way that the standard of living can be sustained over the long haul. Some basis for continuing basic research on a broad scale must be protected. Knowledge may be costly but, in the long run, not so costly as ignorance. It also appears likely that an increasing fraction of basic research will necessarily be carried out in universities, as opposed to other possible settings. With the sharpening of competition through globalization of the economy in recent decades and the accompanying decrease in profit margins, most companies seem to have neither the capital resources nor the support of their investors which would be necessary to sustain basic research laboratories. The rapid pace of technological innovation has also become a deterrent to establishment of industrial research laboratories. Once the lifetime of a typical technology becomes significantly less than the length of a career of a scientist or engineer, the effectiveness of a research laboratory with highly trained specialists is lost. This has opened the way for formation of small specialty companies that can sell technological expertise to large companies on an “as needed” basis, obviating the need for the company to assume the high costs of a permanent staff in all areas of specialization of relevance to their products.

¿From another perspective, it can be argued that engineering research is the vehicle by which wealth is added to a society. This point of view is at variance, however, with a prevailing myth of the so-called Age of Information. The tenets of this myth are based on the fact that huge amounts of information in electronic form can be conveyed in an instant to distant points on the Earth and on the assumption that this information has intrinsic value. To the contrary, the fact that information can be created so easily and made available so readily has diluted the value of any individual item of information to the point that the concept has little meaning. It is true that this technology, whereby information can be moved from one place to another very quickly, also makes it possible to move wealth very quickly from one place to another. There are numerous examples showing how fortunes have been made by clever use of information, but typically at the expense of someone else. That produces a *redistribution* of wealth, but that is quite distinct from the creation of wealth at the level of societies or countries.

Real wealth, the characteristic that accounts for the standard of living in industrialized countries, is literally created by exploitation of information. On the basis of knowledge, means are found by which the intrinsic value of real, tangible materials can be increased at a cost which is less than that increase in value. The intrinsic value of goods is what a customer is willing to pay for it in the open market place, and it is through sales of products with high valued-added margins that wealth is created for a society. This is the real essence of the information age. Scientific and engineering research is at the core of this process; it is precisely through these activities that the knowledge to be exploited is made available.

There are several aspects which make this issue more complex than the foregoing simple picture would suggest. For example, industrial competition is essentially an effort to minimize the costs of implementing ideas, through basic research on materials processing, manufacturing methods, assembly strategies and transportation; these matters are typically lumped into some measure of "productivity". Another modern factor in adding value to goods through technology is the cost of assuring that those materials will be returned, after their useful lifetimes, to a state that is not detrimental to the quality of human life. But accomplishing this must also rely on information gained through research which can be implemented while still leaving a net benefit to society.

If the premise that research is an essential part of life is adopted, then what about the size of the current academic enterprise? In the US, it is probably too large and diffuse. This is a consequence of having had most universities create graduate programs in many areas during the time when federal research funding was plentiful, without concern for whether or not there was a need for additional graduates with the qualifications that could be provided by those institutions or if any constituency was being served through creation of the programs. In the traditional areas of mechanical engineering and applied mechanics, the size is surely going to decrease somewhat in the years ahead and the overall effort will become more directed. It seems to be inevitable.

The following experience indicates one manner in which universities are attempting to respond to this situation, which prevails in nearly all areas of study and research. A few years ago, a committee called the Academic Directions Committee was formed at Brown University with faculty representation from each of the major disciplinary areas of the University. The charge given to this group of faculty members and senior administrators was to identify academic areas of the University where resources should be added, and where they should be decreased, all within the constraints of a realistic budget. This is an example of a fundamental change in thinking in academic administration in the US. It is now being widely recognized that it is no longer possible to do all worthwhile things well, or even to do them at all. Universities have to choose those things which it wishes to pursue from the cafeteria of intellectual activity, and it has to be satisfied that its offerings are at least representative of the arts, humanities and sciences, even if the offerings are not comprehensive. Another example is provided by the recent announcement that Yale University is adopting a policy called "selective excellence" whereby its resources are to be focused on its largest and strongest academic departments. The goal is to ensure the health and vitality of those academic programs which are most likely to have a significant impact at the national level on the scholarly disciplines they represent. Presumably, other areas will be supported only to a level necessary to maintain a reasonable

diversity of instructional programs. At Yale, the programs in physical sciences, engineering and management were not included among the select programs.

The reliance of various programs at Brown University, including engineering and applied mathematics, on external research funds came up frequently in the deliberations of the Academic Directions Committee, and a cross-section of views on the matter was actively sought. It was surprising that many faculty members from the sciences and engineering who were interviewed by the Committee expressed the view that the downturn in external funds available for academic research in the US was temporary, probably due to the recession of the early 1990s or some political swing in Washington, and that it was a short term anomaly. It is indeed likely that an anomalous period is coming to an end. However, that anomalous period began with the launch of Sputnik and has continued throughout the intervening 40 years, during which time the research enterprise has grown steadily. On the basis of foreseeable circumstances, it is unlikely that the level of external support for academic research will recover and adjustments are essential to protect against an over commitment of university resources.

A matter of particular concern in considering the overall size of the research effort in the US is the status of those national laboratories whose strategic missions have been diminished significantly with the end of the cold war. These laboratories house superb research facilities and employ highly educated people with unique scientific and technological expertise. In their search for a new role, these institutions have become direct competitors with universities for the limited national pool of funds available for basic research. The research equipment at these labs, acquired with public funds, is superior to that at most universities; furthermore, the permanent support staff is well trained and probably more effective as research assistants than are transient graduate students. However, the research is much more expensive to do than in a university and, more importantly, young people are not educated in the course of carrying it out. This is at odds with one of the main functions of research universities, namely, educating young people who will constitute the next generation of scientists and engineers who have an appreciation of independent, creative research and who are able to contribute to the national research enterprise. The current situation is unacceptable for the US, and it requires a prompt and thoughtful solution.

So, basic research is critical to our society, but the number of researchers will likely decrease and those remaining will have to become more selective about what they do. What about the model for basic research? Over the past few decades the US National Science Foundation and other agencies have lurched from one extreme to another in devising funding strategies; a stable situation to serve as a basis for long term planning has been elusive. For a time, research grants were available to the most accomplished senior scholars who would supervise and support large numbers of younger investigators of their choosing. This arrangement gave way to a system of single-investigator competitive grants; all those with research interests were expected to generate research funds on their own. This shift has placed a burden on young people in particular, a consequence which has discouraged many very capable young people from even considering academic careers. Then, in a dramatic shift, emphasis was placed on block funding of large programs with narrowly defined goals which require a multidisciplinary approach for achievement, including a period when important decisions on research directions were made in Congress. The record on centers created in this way has been mixed. There are

some cases where one could call the effort successful, but these are probably in the minority.

A model or framework for research which can serve the next generation of researchers in engineering and applied science should have the following elements:

- reliance on individual initiative as the prime source for new ideas
- coordination of efforts in the research community toward identifying common goals, accounting for long term industrial needs
- focus on education on disciplinary fundamentals and the preservation of knowledge

There is simply no substitute for individual initiative and the spontaneity of creative thought in advancing any field, and it is essential that it be recognized, sought out and allowed to flourish. Investigation cannot be restricted to only those goals which have obvious and immediate applicability. It must be accepted that many original ideas, indeed, the majority of original ideas, will never be useful in the engineering sense. This is a small price to pay for the few that turn out to be tremendously useful. A few examples of contributions can be cited within solid mechanics which have evolved from abstract mathematical concepts to maturity in engineering practice within the past three or four decades. These include fracture mechanics, plastic limit analysis and the finite element method. In each case, the ideas were discounted at first as being frivolous and impractical but, in the end, each has had a huge influence on a broad technological front, as discussed earlier for the case of the finite element method.

In fracture mechanics, it is accepted as a premise that all engineering materials contain small material defects which arise naturally or which may arise in the course of service. The goal is to provide a quantitative description of the mechanical state of a deformable solid containing one or more cracks, with a view toward characterizing and measuring the resistance of the material to crack growth. The evolution of the area is typical of engineering science. Research results, obtained through motivation based on little more than curiosity, were produced in isolation. For example, in the early years of the 20th century, mathematicians (G. Kolosov in Russia and C. Inglis in England) interested in boundary value problems in the theory of elasticity found the solution for the stress field in an elastic plate under tension containing an elliptic hole. It was seen that, for a fixed applied tension, the stress in the material at the ends of the major axis of the elliptical hole increased as the aspect ratio of the ellipse increased. In the 1920s, A. Griffith (England) used this result as a basis for an energy balance fracture criterion in order to explain observations on the fracture of glass at stress levels far below the ideal strength of that material. But the criterion seemed to be applicable only to very brittle materials and, consequently, of limited practical value for structural materials. However, in the middle decade of the 20th century, G. Irwin (working in the United States) established the universal nature of the crack tip stress field to serve as a key concept for relating engineering designs to laboratory test results, and he showed the way to extend the Griffith-type criterion to engineering materials. Nonetheless, the engineering research community resisted acceptance of fracture mechanics until major successes in the aerospace industry, spearheaded by P. Paris at Boeing, brought broad acceptance. Within 20 years, forty percent of the members of the Applied Mechanics Division of the American Society of Mechanical Engineers regarded fracture mechanics as a principal research activity! During the 1970s, the concepts were extended by J.

Rice, J. Hutchinson, A. Wells and others to include nonlinear elastic-plastic material behavior. These developments were accompanied by important experimental discoveries, including the direct current technique for measurement of crack length in metals by J. Carlsson. The outcome of this research history is that its consequences now provide the basis for industrial material testing standards, for pressure vessel and piping system design codes, and for design of aircraft, power plants and other engineering systems.

The second element of the research model suggested above is identification of common goals. This implies actually agreeing upon common goals, which is not a simple matter. In the present era of specialization, each researcher is focused on work in his or her own area. It is not easy to stand back and to take an objective look at a whole field, such as mechanics of solids, much less to say something about all of mechanical engineering or all of applied science, but this is what must be done. In thinking of organizing a group of individuals with the makeup of researchers toward some common goal, a comment made by the provost of a leading research university in the US comes to mind. In describing his role at the institution, he said that academic administration at a place like his own is akin to herding cats!

The task of working toward common research goals, on a very modest scale, was undertaken by the Applied Mechanics Division of ASME a few years ago. The director of the Division of Mechanical Sciences at the National Science Foundation at that time expressed the view that there was value in getting input from the research community on what directions NSF should be funding. The director approached the leadership of the Division, which was then and still is now the closest thing to a national focal point for US activity in solid mechanics, and asked that the three most pressing research problems in the area be identified. The reflexive response of many people, when confronted with such a question, is to list the three things they worked on yesterday; the more thoughtful types would list the three things they planned to work on tomorrow. But what actually happened is this. The Division Executive Committee went to its thirteen technical committees concerned with various specializations, such as elasticity, wave propagation, computational mechanics, fracture mechanics, and so on, and asked each for the single most important outstanding problem in its area. The executive committee then filtered the input it received, weighed the supporting arguments, and refined the list to three items. An especially interesting outcome was that very specialized topics from seemingly unrelated areas were often different aspects of the same technological problem so that, in the end, each of the three topics selected had more depth and scope than had been originally anticipated. This is the kind of process that is required, first in mechanics, then in mechanical engineering, then in engineering, and finally in all of applied science, in formulating research plans for the future. This end will be achieved whether or not the research community participates in the process.

### **Teaching to prepare for uncertainty**

Finally, the issue of education in this era of transition is considered. Education is the means by which experiences from the past are used to prepare for the future. As someone once quipped, predictions are always difficult to make, but especially so about the future. So how should education be viewed if it is the means of preparing for uncertainty?

To make the point, consider some developments in technology which occurred in 1960, which is about the time that many people in positions of responsibility in major universities

began their own educations. For example, in 1960:

- approximately 2,000 digital computers were delivered to US customers. In the previous year, the first transistorized computer was built by RCA, and the solid state integrated circuit was invented (independently) by J. Kilby and R. Noyce. The latter went on to found the Intel Corporation. Virtually no digital computers were in use in educational settings.
- the first one-piece aluminum can was used commercially for soft drinks and for beer. This use of aluminum quickly expanded to a volume greater than all other uses combined.
- the first functional device for light amplification by stimulated emission of radiation, or LASER, was demonstrated. The device provided a powerful research tool for all experimental fields, and it ultimately led to a range of consumer products, most notably the compact disc player.
- the Bulova Accutron watch was introduced. This was touted as the first commercial electronic watch but the timing element was mechanical - a tuning fork. Quartz watches were still a few years in the future.
- the Datsun (now Nissan) automobile appeared on the streets of the US as the first step in the revolution of the US auto industry.
- the first successful communications satellite, Echo I, was launched. It was essentially a Mylar balloon coated with a thin layer of aluminum. Communications tests were carried out by reflecting radio signals from its surface and successfully receiving them elsewhere on the Earth.
- the first privately financed nuclear power plant was opened at Dresden, Illinois. Although the output from nuclear sources increased to a large proportion of total energy output, no nuclear power plants have been commissioned in the US in recent years and none are under construction.

What is the point of mentioning these things? These are technologies which are now part of everyday life and the consequences of these technologies, both good and bad, are known. But in 1960 the concepts were new and the consequences of their introduction could not be anticipated and, indeed, were not anticipated. How are young people to be educated to contribute effectively in that kind of situation?

Perhaps the main goal of teaching should be to define the boundaries of a discipline, to convey an understanding of the modes of thought which can be brought to bear on questions, or to learn to interpret information that is not put into an artificial framework. A very important purpose of teaching should be to convey methodology, or mode of thought, which is what distinguishes one discipline from another. Methodology is the life-blood of a discipline. Methodology in mechanics commonly refers to the process of (1) examining real things and abstracting them to their essential qualities, (2) invoking laws of nature, (3) applying mathematical or scientific methods to draw inferences, and (4) holding these inferences up against real world behavior for comparison. An approach to any discipline can be represented in much the same way. By

the time a student earns a university degree, he or she should have seen this way of viewing disciplines often enough to recognize that it exists as a fundamental structure. Indeed, this may be closer to the essence of education at a research university than the details of course content in a curriculum. It is the habit of thinking critically or deeply that is important.

These ideas are not original, of course, and they are raised merely as a reminder in the context of this discussion. Indeed, these ideas on learning have been beautifully summarized by one of the greatest writers ever to sit in the White House in Washington DC. In an 1859 speech on Inventions and Discoveries, Abraham Lincoln, in an apparent reference to James Watt, said “But was this first inventor of the application of steam wiser or more ingenious than those who had gone before him? Not at all. Had he not learned much of them, he never would have succeeded – probably, never would have thought of making the attempt. To be fruitful in invention, it is indispensable to have a *habit* of observation and reflection; and this *habit*, our steam friend acquired, no doubt, from those who, to him, were old fogies.”

### Some issues taking shape

When it comes time to update this report in a decade, which issues under debate today will have emerged as those shaping the intervening period? An answer given now can be no more than a guess, but perhaps some possibilities can be identified. A few suggestions follow:

- In mechanics research, development of computational procedures for simulating the response of structures or machines, including essential features over a wide range of size scales, will be a focus for some time. For example, for design purposes, how should a lightweight composite superstructure on a ship be simulated in order to accurately capture the overall structural behavior as well as any degradation in stiffness or strength due to environmental or service damage at the level of material microstructure? Some approaches being pursued are based on automatic adaptivity of finite element meshes or on adaptive multi-grid methods with reliable data transfer between grid levels.
- Another emerging research focus is driven by the fact that material microstructures can be observed and manipulated at ever diminishing size scales, and by the belief that the macroscopic mechanical response of some material systems is controlled by physical processes which are operative on the nanoscale. Complete atomistic models of such systems are too large to be amenable to analysis with computers of the foreseeable future. Mechanics research integrating theory, experiment and computation will likely provide some of the key elements in linking overall continuum level behavior with local phenomena related to the discrete nature of real materials.
- In faculty development at many universities, strategies are being implemented for broadening the basis for decisions on hiring, promotion in rank and academic tenure by incorporating substantial input on teaching performance and service, as well as research qualifications. It is not clear how this is to be done in a consistent way. Progress in this effort may result in a more diverse faculty in which each individual makes contributions of genuine value to the institution, but in which not all are expected to contribute in the same way. On the other hand, it may irreversibly compromise the most essential qualities of a faculty.

- Viable modes of interaction between universities and industry are developing more slowly than had been expected, and the nature of that interaction is not yet clear. Views obtained from industry leaders indicate a need for university graduates who are well grounded in the fundamentals, adaptable and flexible while, on the other hand, the research they would prefer to have done is short term and product oriented. Similarly, universities are trying to address the technical concerns of potential industrial partners, while also maintaining broad academic competence as a basis for their long-term futures and their academic independence. These various issues are often at odds with each other, and workable arrangements have been elusive.
- The National Science Foundation was established as an independent government agency a half century ago with the mission of fostering basic research in all branches of science and technology, and of ensuring the availability of well-educated scientists and engineers. In recent years, this mission has been broadened to include the goals of informing the public on progress in science and technology, and of improving the quality of education in general science of American school children at all grade levels. This is surely a worthwhile goal, and success in achieving it would provide a better informed population and a more capable work force. On the other hand, the new mission has been funded at the expense of the basic research mission, and researchers themselves have been given much of the responsibility for carrying out these efforts under the rubric of “outreach.” Communicating research results to non-specialists may necessarily become a part of research through this process.

## Closure

The foregoing assortment of impressions and opinions represents a personal view formed over a span of many years. Surely this view has been influenced by unique local factors and, just as surely, it is flawed due to limits of experience or perception or wisdom. Nonetheless, the view is shared so readily because the underlying issues are so important. Hopefully, others will be inclined to reflect on their own views on these issues as a consequence of reading these.