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NOTES: FOR SPEECH GIVEN UPON RECEIPT OF THE TIMOSHENKO  
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First of all, I am flattered and pleased to have the association with the proud name of Timoshenko. Over the years, my respect for him has grown, as I have gained appreciation of how much he did to upgrade the education of mechanics, in engineering. The trend which he helped initiate has continued so that, today, his works seem rather naive and unsophisticated. In part, this is naturally associated with growth on the subject. It might also reflect some good judgment on his part. There are limits to kinds of changes which rather conservative professions will accept. Such need some prodding, if they are to avoid becoming obsolete. Collectively, those of us here represent a kind of activity which we call Applied Mechanics. Like social clubs, it has a kind of formal structure of organization, or we would not be here tonight. At least some like to think of it as more than a club, a profession or something like it. As a profession, it has some responsibility to pinpoint or generate the interesting and important problems, and to find ways to bring to bear the best talent to solve these. Where it fails, the profession will lose ground to another which is more effective. Times have become harder, so we can ill-afford to lose ground, and we should be more aggressive in finding new turf. I would like to reminisce about personal experiences, to illustrate these points.

Particularly, I remember two previous occasions when Ronald Rivlin served to introduce me. One was for a lecture which I gave at an ASME meeting in Atlanta. Afterward, he opened that it would be a good idea if my next performance took

the form of mime. Tonight, I shall ignore this sage advice to keep my mouth shut. The other goes back many years, when I was a graduate student in mathematics, at Indiana University. Then, it was not him personally, but his research which was involved. I was not so interested in mathematics, per se, but in what could be done with it. Available courses in continuum mechanics interested me more, but didn't quite fit the bill. During my last year, Clifford Truesdell joined the faculty, and gave a course on general continuum mechanics, covering about what was later published in his now well known article, "The Mechanical Foundations of Elasticity and Fluid Dynamics". I was excited by Rivlin's imaginative work on rubber elasticity and non-Newtonian fluids, becoming hooked on the subject. I then moved to the Naval Research Laboratory which was, for a time, a center of the activity. It is still pleasant to remember the heated but friendly arguments concerning the foundations of continuum physics, with Rivlin and Richard Toupin, particularly. Imaginative work can induce young people to enter into a field, if they learn of it. Rivlin's work got me involved but, except for Truesdell, I might not have encountered it.

At that time, the Applied Mechanics community was too conservative to be much interested in things so esoteric as the three-dimensional nonlinear theories of elasticity and viscoelasticity, so we made contacts elsewhere.

At the time, practical interest in these subjects was associated mainly with problems arising in the production and use

of high polymers. Classical theories of fluids fail to describe the behavior observed in the melts or solutions used to spin synthetic fibers. In the rubbery solids, the deformations of interest are too large to be described by the theories of infinitesimal deformation which were the stock in trade, in Applied Mechanics. In earlier times, the well-known slogan "Better Living Through Chemistry" applied rather well. Most of the research had been done by chemists or chemical engineers, with little training in mechanics. They had real problems, of a complicated nature in mechanics, applied mechanics, in the literal sense. Those in Applied Mechanics might have created a new set of jobs for this establishment; had they been more aggressive. Instead, those primarily involved worked with us. I helped some of them learn about mechanics, and they shared their knowledge with me. As a visiting professor of chemical engineering at Delaware, I gave one of the first courses on general continuum mechanics, for this profession. However, most of the mutual education was accomplished by much less formal means. There were groups outside universities which played an important role in fostering such cooperation, like the Mellon Institute and the Rheology Section at the National Bureau of Standards. Gradually, various universities evolved more formal training programs, helping to bridge the gaps. It has been years since I worked on viscoelasticity theory, but I still communicate with those in polymer rheology, for example. Gradually, such theory has become more accepted in Applied Mechanics, but it would not be fair to say that the center of activity is now here.

I have not lost my love for the nonlinear theories of elasticity and thermoelasticity. I do enjoy the kinds of mathematical problems which they suggest, so it pleases me to see new uses for them. For a long time, it has been recognized that they are useful for the rubbers, and for analyses of typical buckling phenomena. I have become more excited about a different area of application, which is serving to illuminate some misconceptions about such theory. I refer to phase transitions in solids. One type of phenomenon is encountered in a material which you might have used, or seen advertised, the heat-shrink insulation material. Raise the temperature enough, and it shrinks. Similar kinds of things happens in various polymers, also in the so-called memory alloys, like Nitinol. Rather commonly, crystals undergo changes of symmetry, as temperatures change. Presently, we do not know how to properly analyze such effects. When I first began to think about them, I thought that, surely, the answers were to be found in works on classical physics. I soon found that they are not, and that parts of the theory are in a controversial state. Somehow, existing concepts are not quite right, and we need to find proper modifications. I rather like to try my hand at this kind of activity.

Nowadays, we are all familiar with some of the devices employing liquid crystals. There are the digital displays found in watches or small calculators. There are the <sup>co</sup>ntainers which evolve pretty color patterns, when a cold drink is set upon them, similar things being encountered in devices sometimes used for

non-destructive testing of people, as well as inanimate objects. At first glance, it might seem that it is optics or electronics which is involved, rather than mechanics. Actually, the changes in optical behavior, etc. are associated with changes in orientation. In turn, these are produced by couples. Very roughly, it is similar to bending a wire, by applying couples.

At least in part, I am being honored for my contributions to the theory of these. Thus I will ask you to indulge me, while I recount some more personal history. Shortly after coming to Hopkins, I began to do research on liquid crystals, before I knew that they existed. Studies of the foundations had made clear that remarks about anisotropic fluids in the fluid mechanics literature were based on erroneous concepts. I was teaching a course on continuum mechanics, and thought it would be <sup>in</sup>constructive to present a simple theory of anisotropic fluids, consistent with basic principles, so I did this. It was a theory of structured continua, reminiscent of old theories I had read about in the work of the Cosserats and others. I thought it would be worthwhile in developing a theory of this kind which really worked, for some real material. Chemists are likely to know of good prospects, so I sought advice at an ACS meeting. I was told that my ideas suggested liquid crystals; I had now learned of their existence. I soon became fascinated with their beauty and unusual behavior, soon realized that familiar theories of fluids would not work. Gradually, I plugged away at the theory, to understand how it operated and to make the modifications which seemed to be needed. Almost a decade later, Frank Leslie came to work with me, putting together the final version. Almost immediately, the theory

passed its first real test, correctly describing the unusual scaling behavior in Poiseuille flow observed by Fisher and Frederickson<sup>o</sup>. For a long time, this has been an activity in pure science, largely chemical<sup>stry</sup>, but the scene was beginning to change. I had encountered and had fruitful exchanges of ideas with one physicist interested in developing practical devices, James Ferguson, then at Westinghouse. Rather suddenly, interest in possible applications perked up, and we began to see more such people. An influential theoretical physicist, Pierre de Gennes, became interested, and formed a very active group at Orsay. We corresponded a bit, about adaptation of theory to cover light scattering. Soon, they were using this to measure viscosities, branching out to cover the other aspects of basic physics, and interesting other physicists. From a selfish point of view, such efforts did much to speed up testing of the continuum theory, which is now well accepted. My original goal was attained, so my interest shifted to other questions. When I began, the basic concept of structured continua, Cosserat continua, or whatever you like to call it, was essentially unused, in Applied Mechanics. In the meantime, a number of others have tried out variations of the idea, on different areas of application, some with success. In liquid crystal research, there is room for those who like the kinds of problems which are encountered in ordinary viscous fluids, to analyze hydrodynamic instabilities, for example. Some such work has been done, mainly by physicists. There are numerous instabilities which occur at very low Reynold numbers, having no counterpart in ordinary

fluids. Some have been analyzed fairly well. There are very detailed measurements, motion pictures, etc. of others, not yet analyzed. Liquid crystal research is beginning to overlap polymer research, in an important way. Highly oriented polymer fibres, of exceptionally high strength, are now being made and used commercially. These are spun from a liquid, a polymer solution. The trick is to prepare this so it is already well oriented. Qualitatively, this is, in effect, a liquid crystal. \*Some of the available data indicates that the theory will have to be modified somewhat, to cover these newer fluids, but we can make good use of some of the experience acquired, and the polymer workers are ~~passing~~<sup>pursuing</sup> this.

My experiences suggest that Applied Mechanics has had two quite different meanings. First, it has served as a name for a kind of profession having rather narrow and fixed limits. Visco-elastic fluids do not belong in Fluid Mechanics, you know. As a young man, I found this kind of attitude too stodgy for me, and I still do. Second, we encounter numerous applications of mechanics to real problems. If the profession is too reluctant to provide expert help with these, it does not deserve the name; it is in fact a private club. In easy times, the profession might survive, but the last several years have not been so easy, and territory has been lost to other professions. Call the predators vultures, if you will, but ask whether a vulture prefers to attack healthy, vigorous prey.

In Mechanics, the scene is changing in other ways, at least partly because of economic pressures. Some see the whole subject



shifting, to become much more concerned with immediate applications. I think that it may become both more and less applied at the same time, which is what I would prefer. As the mathematician or other pure scientist shifts, he may come in on the <sup>e</sup>last applied end, and I see this happening. My experience in liquid crystal research, as well as polymer mechanics indicates that more real progress is made when people with diverse backgrounds mingle, stimulated by mutual interest in some problem. Nearest neighbors tend to think alike, to get stuck in a rut which might now be comfortable, but is headed into quicksand. As a group, we in Mechanics seem to have a reputation for being intolerant of those who do not see eye-to-eye with us. He who is stung by this will be glad to see us disappear into the mire. Perhaps it is wishful thinking, but I believe that we are changing for the better, on the whole.

I see more professionals venturing outside the stockage<sup>d</sup>, finding the experience rewarding, and making contributions, in area quite different from those which I have mentioned. We still need to do more to help the best minds tackle the best problems. Fields start to move because someone gets a novel idea or technique, and others see ramifications which appear to be attractive. It is my own view that our own interests are best served by doing whatever we can to promote the best research. I do not know what can be done to create more of the spark plugs, but we can be alert for ways to help those which are firing. It is quite feasible to assess the talents of some bright young man, helping them to find the right plugs. It requires that we have some contact with them, and I would

be happier if there were more such contacts. Like some of you, I have been thinking about and experimenting with ways to improve these situations. Perhaps, this is the time, but not the place, to assess the value of the efforts.

There comes a time when a speakers enthusiasm for talk must give way to concern for the listener's weary bottoms. Thank you for listening to my ramblings.