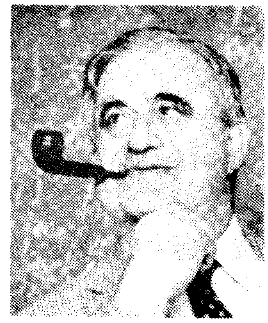

The 1980s is not an era for standing still in science

A speech by fusion scientist Professor Marshall N. Rosenbluth upon accepting the Fermi Award.

Professor Marshall N. Rosenbluth, leading U.S. fusion theoretical scientist, received the Department of Energy's prestigious Fermi Award on Feb. 6, 1986. What follows are excerpts from the speech he made on that occasion. Rosenbluth has directed the Institute for Fusion Studies at the University of Texas at Austin since 1980. Prior to that, from 1967 to 1980, he was at the Institute for Advanced Study at Princeton.

Science today is no longer primarily a product of individual brilliance. To paraphrase Thomas Edison, it is 10% inspiration and 90% cooperation. While I am delighted to receive this award, I am happier still with the real purpose of the occasion: to recognize the efforts and contributions of many, many people in fusion research, some of whom I am pleased to see here today.

I am a physicist, like most of us, for two reasons. First, for the intellectual thrill of understanding the universe a little better. To me that understanding has a deep spiritual meaning. A ceremony such as this conveys that our citizens, through our government, share to some extent in the feeling that an increase in scientific knowledge adds a little bit to all of us.

My second reason for becoming a physicist was a deep conviction that technology could make life easier and better for the human race, indeed was essential for its continued existence. I have been extremely fortunate in being able to combine these two motivations into a career primarily devoted to the quest for controlled thermonuclear fusion. Precisely because of these dual satisfactions, the worldwide fusion community is a close-knit and happy one. Being associated with it has brought me great joy through the friends I have made.

President Reagan at the summit demonstrated keen insight by recognizing that fusion research is an area where the world has worked in harmony and can continue to do so. I trust that concrete details of this great cooperation can soon be finalized.

Why, after 30 years of effort, am I so optimistic about fusion's prospects for eventual success? Not only because of the increasingly better plasma conditions we have achieved, but more basically for a reason which may sound a bit strange to a nonscientist—that I have felt, especially within the past few years, that understanding is maturing, that theory and experiment are converging, and that within the parameters of our understanding lie the parameters for success.

Let me perhaps run quickly with you through those 30 years of magnetic fusion research we have shared. The 1950s were a decade of innocence when we hoped to succeed without really understanding what we were doing. Early research, in particular my own at Los Alamos, centered on the simple Z-pinch. We could properly estimate the dynamical time scales, but immediately uncovered the insidious kinks and sausages, the first visible head of the instability hydra. Early work with stellarators and magnetic mirrors also quickly ran into trouble. Confinement in all these early devices was many orders of magnitude below what we hoped for and have now attained. I might remark parenthetically that one of the real triumphs of understanding of recent years has been the final elimination, in accordance with the very complex theory, of the so-called losscone modes which had plagued mirrors since those early days.

The 1960s were the decade in which a fundamental framework of theory was laid down—a solid, if incomplete, framework which serves us to this day. From the raw intractable equations for the behavior of innumerable individual particles we could derive the appropriate tractable equations for the basic types of plasma dynamics—the ideal magnetohydrodynamics, resistive MHD, small-scale drift waves, and the anisotropy driven modes referred to before. These modes are known not only in laboratory plasmas, but throughout the universe. The 1960s were perhaps the era of my own maximum productivity, the Golden Years of the TAERF program at General Atomic, which incidentally marked the beginning of my "Texas Connection" which continues happily today at the Institute for Fusion Studies in Austin.

While the 1960s were a decade of progress in theory, experiments continued to be disappointing—due in large part to small scale and to inadequate technology. This situation was changed towards the end of the decade with the dramatic successes of the Soviet Tokamaks.

The 1970s saw a great increase in support for fusion research, due in part to renewed hopes raised by Tokamak results and in part to the oil crisis of those days. With these new funds, the needed technology, for example, heating and diagnostic techniques, could be developed and smarter, often costlier, but more successful experiments deployed. I was fortunate enough to be in Princeton during this era of progress and participated as theoretical work of the 1970s evolved towards nonlinear theory and large-scale computation.

Now in the 1980s, the world's break-even size tokamaks are on line: The Princeton TFTR, the Euratom JET, and the Japanese JT-60. It is striking that, despite the more than a decade that has passed since their planning, and despite many fascinating details we have not yet digested, their overall behavior is very close to original predictions. They will almost certainly fully meet their objectives.

To illustrate why I say understanding is maturing, let us look at the three main features of tokamak behavior. When enough heating power was applied to test predicted pressure confinement limits, these limits exactly matched those of the MHD theory. Magnetic islands grew and saturated or disrupted, and sawteeth reconnected just as *a priori* nonlinear resistive theory had postulated. And while a theory precise enough for engineering calculations does not yet exist for anomalous heat loss, this loss has been shown to derive in about the right magnitude from drift-wave microturbulence. Much of its scaling is understood.

In short, the theoretical framework of the 1960s, after 20 years of disagreement due to experimental or theoretical inadequacies, now explains in broad outline the experimental results of the 1980s. This understanding, which gives grounds for optimism, has a very important corollary. There are new avenues which theory begs us to explore—feedback control of magnetic islands has long been my favorite, shaping and profile control are others. These new avenues will surely convert presently projected parameters, marginal for a reactor, into satisfactory ones.

I am well aware that scientific success is only a beginning for reaching the Fusion Era. Many extremely difficult engineering problems must be solved before the test of the marketplace can be applied. My crystal ball is necessarily cloudy, but there is one safe prediction for the future: computer-controlled manufacture and quality control and robotic operation will resolve many of what appear today as very high-technology issues.

The budgetary outlook

There is one problem about which it is difficult to be optimistic in the near term—the budgetary support outlook. The nation faces very difficult choices. Many urgent short-term needs must be met. Much human distress must be alleviated. What is necessary for national defense is not completely under our control.

In the face of those demands, do we have the national resolve, and the appropriate mechanisms for pursuing the long-term adventures on which 21st-century greatness will rest? Basic physics and biology, energy research, and space exploration come to mind.

Progress in such adventures is often slow; sometimes we suffer failures as Three Mile Island and last week's terrible Challenger tragedy illustrate too well. The temptation to drop these challenges is great. No national disasters will ensue in the near term and few votes will be lost. But, can we be a proud and successful nation 20 years from now if we abandon the struggle? I doubt it. This is not an era for standing still.

I would happily exchange the proceeds of this marvelous day for the assurance that I will be able to take part in this great human triumph. I want to see controlled fusion in my lifetime, and I am sure I will, just as I am confident that taming our most abundant energy source will ultimately lead us to new heights.

I am not pessimistic. I have a great faith in the wisdom of America's people, and in the workings of the American system of government. In often mysterious and sometimes tortuous ways, the right decisions are eventually made, the path to greatness is followed.

What I have experienced and seen of history in my lifetime assures me that the exploration of our frontiers will continue. We now stand in fusion research on the threshold of scientific success. One final step is urgently needed: to produce an ignited plasma, to actually see the effects of fusion products, so long dreamed of and studied on paper, to give the proof test to the existence of a burning fusion-reactor fuel. Studies of the last year have convinced us we can provide such a test at moderate cost.

I would happily exchange the proceeds of this marvelous day for the assurance that I will be able to take part in this great human triumph. I want to see controlled fusion in my lifetime, and I am sure I will, just as I am confident that taming our most abundant energy source will ultimately lead us to new heights.