Yes, I want to save lives. Yes, I want to be a great surgeon. Yes, I want to deliver state of the art care in a cost efficient manner. Yes, I want to teach medical students, residents, nurses, patients and everyone else everything I know. But that's not why I come to work every day before the sun rises.

What I really want to do is improve the care that is delivered is the year 2025. I want to look back twenty years from now and marvel at how ignorant we all were in the year 2005, and I want to do it all in a very real, tangible, and quantifiable fashion. I want to document the fact that because of changes I have made, more people are alive and healthy. It is a tough yardstick, but this isn't a fashion show. I need objective results.

Research is the answer in traditional and non-traditional forms. In the traditional manner, my laboratory works to develop promising immunomodulatory approaches to the treatment of infection, yet always with an eye toward clinical application. Clinically, we take the tried and true routes of randomized controlled trials and observational studies attempting to tie biological phenomena (the immune response) to relevant outcomes. But none of these studies really stretch the imagination. Non-traditional research, where my ingenuity can be used to explain patient-centered observations that are not predicted by current dogma are what fires my creative spirit. Why is the crude hospital mortality associated with a urinary tract infection in my intensive care unit 31%, while it is only 24% for a ventilator-associated pneumonia, the exact opposite of expectations? Is it the infection or the immune response? If the intensive care unit protocol in use for one year dictates a serum glucose of 80-110 mg/dl (shown to improve outcomes), why is the mean morning glucose still 123 mg/dl? How can we modify healthcare provider behavior to improve compliance with interventions that are known to be effective? Complex and challenging questions like these need attention if we are to impact patient survival appreciably.

But the big quest, the windmill, is different and may be impossible to achieve. I want to find ways to correct the astronomical imbalance between the beauty, precision, and sheer quantity of basic science knowledge and the embarrassing pathetic state of clinical research in medicine. How can a society understand the most intimate relationships between nuclear transcription factors yet not know the optimum duration of antibiotic therapy for a patient with a perforated appendix, a condition seen literally hundreds of times per day in this country? It is my belief that clinical research is so confused and poorly organized that it cannot keep pace with technological development. For example, the "definitive" study of sentinel lymph node biopsy for suspected breast cancer is just now finishing, despite the fact that this procedure has been an accepted technique for almost a decade. A fundamental change in the way we perform and interpret clinical research is desperately needed. We may well have to begin by staying the sacred cow known as the randomized controlled trial except for testing new drugs or devices, and develop equally effective methods based on very large numbers of patients, information technology, and correction for the inevitable heterogeneity that must be considered but is so frequently excluded from randomized studies. My approach will be to start with projects to demonstrate the feasibility of valid inductive reasoning based on the thousands of clinical data points that already exist for every single one of our patients. Small studies will be followed by larger studies. Research fellows will be trained to ask relevant questions and answer them in a patient-oriented, and if necessary, non-traditional manner. Clinical trial networks will be formed, and eventually I will build a whole department from the bottom up, where clinical and research data are so integrated into practice that they are indistinguishable. Every patient will be a research patient for either observational or interventional purposes. It is only by new methods focusing on a patient-oriented increase in research data throughput that we as clinicians will ever be able to fulfill the promise given to us by the brilliant basic science advances of the last century. I will do all I can to lead that charge.