The Future Research University

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At the outset, I should announce that this article will be based on predictions. Perhaps I should say "confess," rather than "announce," because predicting does not engender great respect among intellectuals. Scientists and engineers would tell us that the only things worth predicting are discoveries and breakthroughs, which by definition are unpredictable. Economists hate people who prophesy, especially other economists, who constantly do so. The CEO's of the largest corporations sincerely rate themselves as visionary, but often show little respect for their own planning departments' chartings of the future. Only politicians consider predicting to be a truly valid activity. To them predicting means poll-taking, critical to learning in advance what stands they should take to garner the most votes in the next election. With politicians being for predicting and all others against it, it ends up with a bad image.

Yet, if you are competent in some field, you are bound to see important trends and sense many consequential events before they occur. You should expect to be right in calling out future happenings 50 percent of the time or so. You ought to steadily update a list of the big changes you think might occur, guessing the probabilities of their actually happening and their importance to you if they do. Then you should ask yourself what actions you might take early to maximize the potential benefits and minimize the negatives of coming changes.

Of course, you must avoid falling in love with your predictions and foolishly committing all your resources on the assumption that all your predictions will turn out correct. You are certain to discover later your failure to include key future characteristics, and you will be badly off in the timing and details of some occurrences you did foresee. Maybe, as a precaution, you should always place at the head of your predictions the possibility that every prophecy that follows will turn out wrong. On the whole, however, I submit you will be better off than if you had totally disdained predicting. That certainly has been my experience.

As I have gotten older, my predicting has become bolder, and I now concentrate on long-range forecasts. I prophesy with great confidence at least 20 years ahead, often much further, this for several reasons. First, I am now no longer concerned that the unfolding of the future will prove me wrong. If any of you were to walk up to me a quarter of a century from now and say, "Si Ramo — boy were you wrong in that speech I heard you give at UCLA!," I wouldn't mind at all. I'd be delighted! Another important reason for looking forward at least 20 years is that it by-passes the confusions and frustrations caused by existing crises and dilemmas. This relief is important. While critical problems exist that urgently need solving, the strongly entrenched culture and operating patterns of academe, along with the stubborn resistance to change exhibited by all the influential establishments outside the universities, guarantee that no great changes will occur soon.

So we can be forgiven for assuming that if we are going to engage brazenly in predicting, we should favor a time period of around 25 years from now. Simply, it will take that long before significant "re-inventing" of the university will have been accomplished.

Take as an example the universities' performance in updating the teaching of engineering. Fifty years ago, World War II disclosed that American engineering education was highly inadequate. The typical engineering graduate of the '30s had only a bachelor's degree, one earned without any courses in science after the sophomore year. To create the military engineering advances needed to win the war — such things as undersea warfare technology, radar, and nuclear weapons — Ph.D.'s in science had to be drafted. They dominated the developments, so when the war ended all universities were determined to alter their engineering schools. The courses, research projects, and professorial interests were shifted to emphasize the science underlying engineering. It took until about 1970, 25 years, to accomplish this transformation.

But in the early '70s America began to be seen as falling behind Japan and other nations in manufacturing and more generally in relating science and technology advances to the market and economic growth. Unfortunately, to apply science and technology to design a product — which will satisfy a public need, can be manufactured reliably and at a price justifying its being purchased, and will generate a satisfactory return on the risk investment required — this was a pursuit far from the interests and experiences of most engineering faculties of the 1970s, so largely science oriented had they become. Most engineering students in American universities then were being graduated with only a faint impression of what constitutes the profession they were about to enter. By 1980 this mismatch of engineering programs to national requirements had become a commonly expressed concern of university, governmental, and industrial leaders. The perceived urgency to "re-invent" engineering edu-

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cation increased every year. Yet, by 1994 we are only a little way along on this changeover.

Suppose now that we try to picture the typical research university of 2020. We squint purposely so as to ignore fine details and spot only the striking changes. What differences will we see? The most pronounced, I predict, will result from the influence of *interdisciplinary* learning on courses, degrees, and research, on the organization of the university and its perceived role in society, and even on the definition of scholarship. When I say "interdisciplinary," I mean to include everything from the single hyphen category (like "bio-technology") through the multi-disciplinary class (like "environmental sciences") all the way to the complex socio-politico-techno-econo mega-issues (like "the health care system" or "the information super-highway"). By 2020 the universities will have come to understand in depth how best to undertake interdisciplinary work and will have created from such understanding intellectual disciplines deserving high standing as academic pursuits and professional career activities. (For brevity, I shall henceforth call these "inter**d**isciplinary **d**isciplines" simply D^2 and will refer to the present conventional or established university endeavors, the "**f**ocused **f**ields," as F^2).

Today, although there is much discussion about D^2, and here and there on a few campuses and a few D^2 topics some serious and excellent academic activity going on, the overwhelming emphasis of universities remains F^2. The ratio is at most five percent D^2, 95 percent F^2. In 2020 it will be more like 50-50. F^2 is here to stay, and it must be. A strong F^2 component at the university is obviously a necessary condition for D^2 to be strong. F^2 will be influenced substantially, however, as to course content and research priorities by the rising of D^2.

Why might it be forgivable to predict that D^2 will play so forceful a role in changing the university? It is because the society's most important problems, needs, opportunities, and challenges will be recognized as D^2 in basic character. D^2 is the underpinning of real life in a world whose complexity and pressures will grow. As gifted university researchers seek new territories to explore, they will find the D^2 issues the most fascinating and most likely to be influential. It is in the D^2 area that the funding and other support for universities will be most generous because the return on investment there will be perceived as maximum. It will be in D^2 where jobs for graduates will be most certain to become available in an increasingly internationally demanding and competitive world, be those jobs in industry, government, education, or professional services.

Industry and government, motivated by necessity, will move D^2 forward through "school of hard knocks" efforts, and these entities often will lead in resolving D^2 issues. It will become accepted by 2020, however, that wherever such tasks might be undertaken, they are likely to be accomplished most effectively if the team involved includes those who have enjoyed university education in D^2. The university will be seen as unique for creating intellectual bases for D^2. It will be the academics who will have the greatest of interest in comprehending how D^2 matters should be fundamentally approached. Of course, in the D^2 area practical factors always will be present whose complexity will defy reducing the approaches to a formula-like methodology. Recognizing this, a typical D^2 issue will be viewed in the universities as an array, a meshed network, of interactive components that can best be integrated by the use of logic and analytical tools, as well as creativity and imagination. The D^2 professors will be interested in the process, not merely the results, when D^2 research is carried out. They will seek to identify the mental steps a good D^2 researcher or problem-solver employs so that they will be able to teach such procedures.

In predicting that the universities by 2020 will have succeeded in creating intellectual orderliness for dealing with the D^2 area, this is not to suggest that there will emerge a single D^2 solving pattern. D^2 areas are much too diverse for that. Many D^2 approaches will be developed. For example, the period ahead will see enormous advances in disclosing the working of the human brain, and by 2020 a number of universities will have become stand-out contributors in this engrossing field of research. They will have done so by organizing teams of biologists, physicists, engineers, chemists, computer scientists, physiologists, and psychologists, who will have learned how to merge their skills to achieve a higher plateau of competence for attacking this unique D^2 challenge. The intermingling of ideas and knowledge they will have come to develop will constitute an intellectual level vastly above that of any specialized researcher on the subject of the brain, no matter how individually brilliant. At the same time, the make-up and practices of this brain research team will differ enormously from the D^2 approaches of a quite different university team, one dealing, say, with urban problems.

In predicting the ascending of D^2 to a firm place in university research and education, I most certainly mean to include that D^2 will come to play a conspicuous part in undergraduate education. Will D^2 be introduced in 2020 even in the freshman year? Definitely, and a D^2 area will be selected as a major by as significant a number of undergraduates as those who will choose F^2 areas for their majors. How is this conceivable or sensible, even after a quarter of a century will have passed? After all, already today we know that some people can be capable of contributing substantially and comfortably only when engaged in specialized fields, focusing their minds ever more deeply on their chosen subjects. Others can be successful only when immersed in projects characterized by a maze of overlapping, diverse specialties, as is typical in the D^2 field. Is it credible anyway to expect anyone, students or graduates, to deal with D^2 before accumulating years of education and experience with F^2?

The universities, I forecast, will learn how to handle all this in 25 years. The new academic skills of that era will include being able to select early the students whom it is right to expose to D^2 issues. All the while, F^2 will not be neglected. The time now devoted to F^2 education alone will be shared in the future between it and D^2.

What if it turns out wrong to bring D^2 into early-year college education? Suppose this is actually done but then regretted? How serious will have been the harm? Very little, I suggest. Introducing D^2 thinking to 18-20-year-olds is surely a broadening of their education because their lives after graduation will be heavily D^2 affected. Surely some D^2 exposure, some introduction to the art of practical problem solving, if done well, should even increase the young students' interest in and appreciation of the value of the F^2 courses they also will take.

A chance encounter I had with some teenagers is pertinent to report at this point. I returned home from the office to find a granddaughter and friends around the swimming pool celebrating the completion of year-end exams. On the way home I had been mulling over D^2 thoughts in preparation for this talk. Thus I was immediately interested when I heard these youngsters, some still having a last year of high school and others having finished a year of college, discussing the news reports that day about the puzzling so-called "Gulf War syndrome." Two pre-med students were arguing loudly that this was a matter for physicians alone to settle. "Why are the media and politicians and the military all in the act? It's purely a medical question — whether there is a disease or not," they insisted. I started to explain that they were discussing an issue whose study and solution needed not only medical expertise but considerations of military, political, legal, economic and sociological matters as well, all inextricably involved with each other.

At first our talk was chaotic. Is the U.S. Defense Department hiding that the Iraqis used poison gas? Will the victims of the disease, if one exists, be entitled to special damage money from the government? Who really has the responsibility of getting the facts and make them known? What fraction of the troops are suffering from this undiagnosed "disease?" Any more than people the same age who were never in the Gulf War but who also have unexplained pain in their joints and are strangely always tired?

I raised the question of just what is the question, the issue before us, something I suggested it is important to try to define early in an effective attack on a D^2 problem. What, I also asked, would constitute a solution? They were intrigued with the fact that in D^2 problems there is no single solution — rather an infinite number of them, none perfect. How then can we know when to stop further searching, when we've picked the best? I told them that this question again was characteristic of D^2 issues, that we deliberately had to invent ways to rate alternatives, and that in working D^2 problems we had to keep going back to reshape both the definition of the problem and the criteria for judging candidate solutions.

Continuing our conversations, we quickly found ourselves needing to separate pertinent quantitative factors from qualitative ones. What might the cost be if there is a need to compensate the ill? How does one do estimates? This was fascinating to them. One student had just finished a course in probability theory, but said he had not the slightest idea how to estimate how many war veterans and/or members of the general public one would have to examine to attain reasonable accuracy in learning whether a real difference in health had resulted specifically from the Gulf War experience.

I could not resist telling them that if they now appreciated the D^2 base to the issue we were discussing, they should recognize that the Gulf War "disease" is a relatively minor matter. What about national health care as a whole? Now, there we have a *super*D^2 issue! Health care comprises a huge seventh of the nation's economy, including the medical practitioners, hospitals, drug companies, makers of instruments and equipment, workers in government associated with health care information handling and regulation, insurance companies, and the growing staffs in every business organization made necessary because a major cost now is health care for employees. There is one chance in seven, I opined, that after graduation they will be employed in health care. At least they will be indirectly so employed even if they work for an auto manufacturer, computer company, motion picture producer, newspaper, bank, or a law firm.

They were much taken by the concept that all of the world's operations involve the linking together of people and equipment. All system components are arranged in some clear and deliberate (and probably some inadvertent) enmeshing. Information, energy, and materials travel through the numerous interconnections. If the desired functions are to be accomplished, the whole of any system must constitute a reasonably harmonious, compatible ensemble. To design such a system, they could readily see, would be challenging. It most certainly would require both a knowledge of the workings of the many elements and an understanding of the interactive combinations. Most importantly, they recognized that they had never been exposed before to this kind of "systems" thinking.

As I left them, the subject became what they should study in college to prepare for what they began to call "the D^2 world ahead." They wondered whether D^2 is taught in college. What would be the names of the new courses? By then, I had begun to feel it would be O.K. for me to predict the coming of D^2 to undergraduate education by 2020.

Continuing with forecasting, let us move to a fantasy I have concocted to make some further points. Imagine that in the year 2020, the U.S. government announces a "New City" program. It is to be a study effort, one presumably to be followed by implementation should the study uncover sufficient evidence of potential success. The program derives, we imagine, from the nation's several decades of frustration with extremely unsatisfactory experiences in urban redevelopment. Existing cities will be increasingly deemed in the next century to be horrible messes as to housing, transportation, pollution, crime, education, energy and water usage, waste removal, basic infrastructure, layout, and more — all faults hurting employment, life-style, and health. Why not, political leaders and others will ask, create a new city? Take some presently unpopulated stretch of land and there do everything right, producing a model city as an experiment. Maybe the new city would work out well enough to set an example for building more new cities or for improving existing ones.

The winning response to this request for proposal, we next imagine, is the one presented by a great research university of 2020 (one I will name with a fictitious acronym, a few letters of the alphabet taken at random: UA). UA, we suppose, wins the contract in part because of the stature it has attained in urban systems design and management, in which subjects it has offered degrees for years from the bachelor's to the doctorate. Its graduates are sought avidly by companies providing equipment, systems designs and services for urban needs (housing, health care, transportation, crime detection, communication systems, education, energy, water, etc.), as well as by governmental agencies concerned with urban operations and regulation. UA's center for urban research is staffed with tenured faculty in architecture, economics, political science, business, medicine, sociology, and engineering, all with years of highly co-operative, successful D^2 experiences.

The backbone of the proposal has a number of strong vertebrae. One is a plan to act on the fundamental idea that success in creating a city requires ensuring jobs for the city's wage earners. To this end, and demonstrating UA's skill and imagination in urban systems, the proposal team offers a novel plan. Selecting a specific 100 square-mile parcel of federal land in a huge virgin area in the Sierra Nevada foothills, UA obtains support from senior legislative leaders for this block to be purchased by a proposed new city land corporation at its nearly zero initial market value. UA explains in its proposal that if a city with a population of 20,000 headed for 100,000 is established — equipped with all necessary infrastructure — then the market value of the land will rise to the billions of dollars. UA plans to use this capital gain to reward all those who will have engaged in risk-taking to ensure the development of an economically and socially sound new community. The capital gainers would include companies agreeing to place an operation in the new city with a guaranteed minimum employee level.

The new entity is named Science City. Included in the proposal are letters of agreement (all contingent on the project's going ahead later) with five large technological corporations. (Again I choose names for these companies by simply randomly writing down segments of the alphabet: IBM, GE, GM, ATT, and TRW.) All of these companies in 2020 have the usual problem of deciding where to locate their growing R&D, and they favor sites where they can most surely attract outstanding brain-power. They commit to locating at least a 500-person portion of their expanding R&D staffs in Science City, especially happy to do so since this is accompanied by a generous stock option in the Science City Land Corporation that will own all the land initially and will sell it gradually to the private sector. (For its part, UA proposes to use its own proper portion of the gain in land value to endow permanently its contingent in Science City as the "UA New City Laboratory," with some 1,000 professionals and supporting staff completely devoted to new-city research and development.)

UA and the five corporations will thus provide an employee base of several thousand wage earners. Multiplying by a factor of three to account for the average family size, then that number by a factor of two to include those employed in supporting services (teachers, barbers, pharmacists, market employees, hospital staffs, police, and families), Science City would have a beginning realistic base population of over 20,000 with reason and room to expand readily to 100,000 or more.

UA's proposal describes the huge number of interacting parameters and alternatives needing study and planning to effect the eventual full founding of Science City. It lists an array of industry contractors that have agreed to handle the many specialized aspects of the overall design (energy, water, transport, housing, communications, etc.), as well as some large systems companies that are prepared to assist in the overall integration and coordination. For the proposed phase I contract, UA plans to chair the study, carried out with the aid of these industrial firms. The study will answer such questions as: Should Science City's electric power requirements be met by a single advanced nuclear power plant (fail-safe, as available in 2020)? Should the layout of the city encourage walking, a minimum of dependence on public transit, and still less on individual vehicles? Should the city incorporate wide-band information highway access virtually everywhere? Should water waste be treated and recirculated for further use? How best can the capital be raised to finance the building of infrastructure before the city's operations will grow large enough to provide the cash flow base to fund such activities? What is the proper approach to allocating funds to various aspects of planning and implementation and to provide motivation for the coming into the city of needed elements?

So much for this scenario, which depicts some of the kinds of contributions universities will be making in 2020 and some of the new ways universities will team with the free enterprise sector and government.

Finally, a few words to connect the foregoing prophesying to the present. It must indeed be frustrating to those of you who lead universities that you cannot move faster and cannot suddenly become more influential in obtaining support everywhere for what should be done, especially when you think you see what changes to make. Perhaps, however, you will agree that there is something worse than being held back. It is to move quickly in the wrong direction. If you have no choice but to accept that alterations will be slow, then you should try hard to increase the chances that you will make the best changes when finally the privilege and power are granted you. Good planning requires doing good long-range predicting. Possessed of advance knowledge of the future, you can plan ahead and be ahead of where you otherwise would be — if your prophesying turns out to be right.

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Dr. Ramo earned his B.S.E.E. in 1933 at the University of Utah and the doctorate in 1936 at California Institute of Technology. As a General Electric scientist he attained world recognition as a pioneer in microwaves, developed GE's electron microscope and accumulated 25 patents before age 30. He is the author or co-author of 14 books.

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