Jobs for My Grandchildren

Thoughts about Creating Jobs by Creating New Industries

The key to the future lies not in creating jobs but in creating entirely new industries.

George Whitesides

I would like to talk about the subject of managing research (which the members of IRI are all in the business of doing) but from a somewhat different perspective than usual. I think we would agree that most of the work that brings technology to people is done by companies. But where we may slightly disagree is over the question of whether the *sole* function of companies should be to serve a financial objective, or whether there's also a social objective that ought to be served.

I know, I know. I'm a Cambridge academic. I'm a liberal, pinko, whatever. Actually, I'm a pretty hard-core capitalist. And I'm going to arrive at a conclusion that may be a little bit surprising, which is that combining a financial function with a social function may actually be a good thing for early-stage innovation in the current business environment (though not necessarily in later stages).

Let me start with my motivations and why I've titled this talk "Jobs for My Grandchildren." I have two grandchildren. One of them is a thoughtful, intelligent three-year-old boy. His sister is a one-year-old and is, as her mother describes her, "ferocious." They'll be coming on the job market and looking for jobs in about 25 years. So the question is, "What should we be doing now so that, when they get to be 25, they will have the same opportunities for really interesting jobs that we had?"

I had no trouble getting a job when I left graduate school, but if you talk to students now, the thing they're most concerned with is whether or not they're going to get a job. It really is a serious issue. If you think of the way our society is set up, you can't really become an adult unless you have a job. You have to have one. The problem of job creation in the United States is often framed in terms of what we can do better than India or China. I think there is actually a much more demanding problem: it is not competing for existing jobs, or even creating new jobs, but creating new *categories* of jobs. That is how we prospered in the period between World War II and the present: we created new kinds of jobs, jobs that simply didn't exist before. Bioengineering didn't exist; the pharmaceutical industry as we know it now didn't exist; information technology didn't exist; social engineering didn't exist. The places where we've done only so-so, such as automobiles, were areas where the advances in manufacturing were spectacular, but those kinds of jobs existed before.

The Teakettle Problem: Framing the Issue

Let me start by talking about jobs and what is called in the trade "the tea kettle" problem. This was first posed to me by a theologian, John Polkinghorne, but it's become pretty well known for helping to think through complex issues. Suppose I have a tea kettle, and the tea kettle is hot. Why is the tea kettle hot? One answer is that the kinetic energy of the molecules is high due to thermal agitation; that's a scientific answer. A second answer is that it was on the stove for 15 minutes; that's an historical answer. A third answer is that I wanted tea; that's an intentional answer. And there are other kinds of answers.

The point these "teakettle" answers make is that when you ask a question—"Why is the water hot?" or "How do we create jobs?"—you actually ask many questions all at once. If you only give one box for the answer, you're doomed from the very beginning: you've walked into a semantic trap.

So when we ask, "How do we create jobs?" we need to be open to many answers. There are jobs that are broad employment challenges: "How do we get Detroit back on its feet?" There are political challenges: "How do we help the people who vote?" But the jobs challenge that I'm interested in is: "How will we create entirely new industries, new areas for my grandchildren to work in?" You might say, "Okay, but the

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United States is a very creative society; why is this even a problem? We've done it before."

I think there are a couple of answers to that question. One is that part of our prosperity, in the period following the war, came from the fact that we didn't really have any competition. Europe was severely damaged; Japan was almost out of action until the late 1970s. So it was not that we were terrifically smart; it was that everybody else was getting things back together again. That world has changed. It's gone. The world may not be flat, but it's certainly not the Himalayas that it once was.

A second thing is that we've moved to a world composed of two extremes: there are societies that have wants, and there are societies that have needs. In the United States, we're now very much a "want" society. We don't really need 27 different companies making designer blue jeans. There are countries where there are potential customers who have a single kind of pants and who desperately *need* clean water. How do you put these things together? How do you think about wants and needs?

Every story has to have a villain, and I apologize to my friends at Goldman Sachs, but my idea of the villain in this piece is Goldman Sachs—not the company, but rather the whole investment industry that has imposed on all of us a requirement that we must think in terms just of money, and in terms of 18 months, not 25 years, because the capital markets can't do a time- and risk-adjusted discounted cash-flow analysis that makes any sense over that long period, and they have almost no ability to judge technical risk. In my experience, starting really new things—starting with a new idea and getting it to the point where it's a perceptible reality—takes about 15 years and about $100 million dollars. Wave your hands about these numbers, but they are roughly right. And interestingly, it's the same whether you're in pharmaceuticals or in airframes or whatever. It's probably not the same in social networking software, but it is in many other areas. So how do we put this all together? That's really the subject that I want to talk about: what are the possible new business models that we can use to address the world's challenges now?

Let me tell you just a little bit about my background. I've been very interested in the subject of converting university-based science into technology for about 30 years. In the course of that time, I've been involved in starting perhaps 15 companies. If you look at the aggregate market cap of those 15 companies, it's approaching $30 billion. (I emphasize that this value comes almost entirely from the smart people who built the companies, but a good idea at the beginning helps!) Of the 15 companies, 3 have been multibillion-dollar companies; a couple have been a lot smaller, and some have been failures. But I know that it's possible to create value from university-derived ideas if you go about it the right way. I normally work in the part of the story that goes from annualized cash flow of minus $5 million a year to, let's say, plus $5 million—I usually bail out just about the time things get to be profitable. I can't handle profits; I only deal with impending catastrophe (which is the repeated experience of most start-ups). So that's my base of experience.

Most of my experience is from a time when things really worked well. We had a venture model, we had biotechnology coming along, we had unlimited growth in the healthcare industry. I don't think any of that's true now.

What do we have now? We have a couple of things that are really important not to lose track of. The first is that we still have, without any question, the most creative society around. I have many friends in Europe and many friends in Asia; they have different skills, but we're still the ones who are really good at creating new ideas, products, and companies. So that's one.

We also have a very mobile workforce. If I want to hire somebody in a company and they're in Seattle, I can call them and say, "Will you come?" One time out of three, they'll come. That doesn't happen in France; it doesn't happen in much of Asia. We also have a variety of sources of capital. Whether they're matched to the problems at hand right now is something I want to discuss. We also have a terrific system of research universities. The American research universities—the R1 universities, of which there are maybe 40—are like nothing else in the world.

And, finally, we have large companies that are spectacularly good at manufacturing, distribution, sales, quality control, regulatory clearance—all the rest of the things that you have to do well to create a real business.

Making Science into Products

Now the question that I think all of us are interested in, in one fashion or another, is "How do you put the science that comes out of the research universities together with the muscle that comes with large companies?" because we don't, at the moment, do it very well. We had a model at one time, which was based on venture capital; this model largely doesn't work any more. So we've got to find something to replace it. And the question is, how do we do that?

I think that the problem we're facing in bridging the science/product gap results from failure on both sides, but I put most of the blame on the universities. A good friend of mine, who was the CTO of a German firm, explained it to me. I used to go with him and drink at splendid bars on the banks of the Rhine in the afternoon and talk about the terrific science that was going on. After a couple years of this, he said, "Look, I really like drinking with you, but I have to tell you—none of this conversation is coming to anything, nor is it going to." I asked, "Why?" And he said, "You're talking about

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converting science into products—products that we might imagine—but big, established companies can't do that." Big companies cannot imagine new products. Now some of you will say, "Of course we imagine new products." But most of what businesses are doing is not radically new; it's a variation on what they already do well. They are not creating the new job categories of 25 years from now.

Making things better is unarguably a very good thing to do—I have no problems with it—but I'm concerned with creating new job categories. What my friend meant was that companies can't innovate really new stuff without a sense of the whole concept. If you have an idea and you bring it to a company in fully developed prototype form, with a device made out of cardboard, for example, but with all of the attributes there, that is something an established company can work with. Once you've done that, they will make that thing so beautifully engineered that you can't imagine it. They'll get a million-fold increase in performance and a million-fold decrease in price, and it will be fantastic. But first you have to invent the concept of the new product for them.

Terrific. But how well do universities do that? They do it incredibly badly. Universities don't really even know what a product is. So there's a big gap between university science, which is extraordinarily good, and the ability of large companies to build on it by actually imagining something that wasn't there before.

Herb Kroemer, who won a Nobel Prize in physics some years ago for his part in inventing the transistor, made, I thought, a very interesting remark. He said that "The principal applications of any sufficiently new and innovative technology always have been—and will continue to be—applications created by that technology." I saw some very interesting numbers recently for a technology which is, I think, relevant to the conversation, which is the commercialization of MEMS or micro-electromechanical systems—the micro mirrors and the accelerometers in your cell phones and in air bags and things of that kind. Someone has tracked the time from the demonstration of a new technology in a university laboratory to its full-scale commercialization. The mean time was 28 years. The shortest time was something like 18 years, and the longest time was about 50 years. I point out to the graduate students in my lab that if they're lucky with some of this stuff, and they invent something on the day they start graduate school, then they might get it out in full-scale commercial production by the time they retire.

Now, can we make this work faster? The answer, I think, is that we can, but the reward systems don't encourage it. Think about the three partners who are ostensibly tasked with putting this all together: the universities that do science and grow smart people and come up with new ideas; the big companies, which have the muscle to actually make things work and have the capital required to do full-scale commercialization; and the venture groups or angels in the center, who are supposed to be the matchmakers.

One trouble is the reward structure. In the United States now, the product of the university is papers. We write papers. The market for that paper is the peer-review system. What we get from the peer review system is money to do research to write papers that go into the peer-review system. And you might say that it would be nice if that cycle spun off technology, but it doesn't, necessarily. So what universities have is lots of good ideas, lots of smart people, but they don't know what a product is; they just have no idea how difficult and expensive it is to solve a real, practical problem. The notion of creating a solution to a problem, a solution that is so valuable that some other person who doesn't even know you will write you a check in order to have access to your idea—*that* we don't do very well. And it's not because we're opposed to the process; it's just because we don't know how to do it.

This leads to one of my suggestions. Others have said that universities are starved for money. I am actually not sure that we're starved for money. I think we're actually starved for *directions* that are important enough to warrant longer-term, more fundamental research. One of the things that I would suggest is that we not think about sponsored industrial research, which I think largely works pretty badly, but rather think about something else: a sort of trade that will be good for both parties.

This is the trade I'm proposing: businesses give universities information about what actually needs to be done, and universities give businesses prototypes of products—that is, plausible solutions for those problems industries need solved. What universities get out of the exchange is ideas for new directions—ideas about where to look, areas that need to be understood. One of the things that has always been impressive to me has been the depth and the detail with which companies understand needs and markets. In universities, we just have no idea.

What companies would get is an option on real product concepts. What universities would get would be options on important research directions. And it wouldn't cost anybody anything. I think it's a pretty interesting thing to think about. We're beginning to practice this in my research group, and I think it works pretty well. That's one of my suggestions: an exchange of options on new problems and directions for research, for options on really new kinds of products.

Financing Long-Term Solutions

Here's a second idea. One of the trickiest parts in the conversion of a technology or science from a university into a beginning-stage product technology is early-stage financing. How do you do it? My idea is the following: there are a lot of

people and institutions out there who have, as objectives, using technology to make something in the world better—that is, to fix a hard social problem, rather than to make a profit. The area that I happen to work with is medical technology for the developing world, but there are many areas of interest: the environment, dealing with the CO_2 problem, whatever it might be.

Now foundations, or sometimes the government, will not give companies money for such problems because the companies are going to take the money and get richer from it, but probably not solve the problem. But these same organizations will give people in universities—who are in principle interested in returns other than profitability—money to solve important social problems. So for example, in our program on developing-world medicine, the not-for-profit organization we've put together will probably raise something like $20 to $30 million to make its initial product. It will, however, get this money at zero cost of capital. Foundations and government can provide money, and a lot of help, and a lot of connections, with no expectation of return on that investment. And you can go a long way in developing a technology with that amount of money.

This kind of funding doesn't handle the really big-time stuff—making the first billion chips, for example—but it handles the funding to make the first 10,000 chips. What do we have to do to leverage this altruism, or societal objective, to create new products? We have to think about using the technology to address needs rather than wants. We have to ask what society actually needs rather than how to sell more units of something by painting it a different color. But the trouble for companies in problems like these is that that they are often long term: you don't know what the outcome is going to be, and they can't be managed with an NPV calculation. On the other hand, the universities are potential partners in this sort of work, and they are very interested in solving problems that are long term.

Let me give you an example. The way I think about this—and I suggest this as one component of a shared approach to thinking—is in terms of what I call "inevitabilities." None of us can predict what technology is going to be successful in 25 years. On the other hand, there are areas where we are absolutely certain there are going to be enormous problems 25 (or 100) years from now, things like the CO_2 problem and climate change, or cost of health care, or population control; those are problems that will be there for the foreseeable future. So how do you put those two together?

You can look at these as partnerships in which universities look for radical solutions to these long-term problems, not with the expectation that they're going to make the products for you, but with the expectation that they will provide the framing, the background, the science, the general understanding of the area. As the field matures, then, companies are in a better position to make decisions as to what to bring to market.

I'll continue with my example. As I said, one of our major efforts right now is in the area of developing-world health. We've developed a little piece of paper on which we print hydrophobic lines and some dyes; you put a drop of serum on it, or a drop of blood, the blood makes its way through channels into test zones, and you get color changes or you get a signal that you can upload to the web. It's designed to provide a test for hepatitis, for example, for a cost on the order of a cent. When you go to the doctor for a hepatitis test today, it's $50 to $100. So it's a radical change in price structure. Why are we doing this?

It starts with an ethical reason: we think that our society should share its benefits with those who don't have them. There's also a national security reason—though people argue with this—but we believe that if the disparity between our standard of living and the standard of living in other countries is too large, you can expect to find resentment that can lead to hostility. That's when the mischief is planned in places like the east slope of the Andes or the horn of Africa, places no one wants to be.

There is a third reason. This *kind* of very low-cost, or even no-cost, technology is exactly what you need for agriculture, for the military, for homeland security, anywhere you want tests done quickly and easily. The early applications provide a test bed.

And there's a fourth reason, which is actually pretty interesting: To change radically the structure of the US healthcare system, from one that's focused on capitalist-driven motives—one in many ways almost indifferent to patients, and focused on treating the symptoms of established disease—to a system of health care focused on quality of life, on public health, requires thinking about this kind of science. At my current age of 73, I would have been dead for 30 or 40 years if I had been born 100 years ago. And my longevity is not because of high-technology medicine; it's because of public health initiatives: vaccinations, clean water, safe food, air bags, and other things of the same type. If your objective is not to run a biopharmaceutical company profitably, but actually to contribute to health care, you have to think about the world's problems in a different way.

But how do you pay for something whose price you intend to take almost to zero, that will take money out of the system? Explaining *why* you'd do this is easier: we know that this is inevitable; the fraction of the US GDP that goes into health care right now is 17 or 18 percent and rising. And if you look at all the measurements of the quality of health care in the United States, it's awful: we're in 19th or 20th place among developing countries in most of the metrics. We really

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do have to do something different, and it's inevitable that we *will* do it. But our current system is not set up to push it forward rapidly.

There is a potential business opportunity in this inevitable shift to lower healthcare costs, which is to bring technology to public health. That's what my research group (and, of course, many others) is beginning to do. One of the characteristics of the business model is you have to switch from paying $50 for every bit of information used in diagnosis to one in which information becomes free and the value is added by storing it, archiving it, checking it against your genome, looking at your past occupational history, all the rest of that. Medicine must become an information-rich area as opposed to an information-starved area. We know that can be done, but it's a totally different business than the pharmaceutical and clinical diagnostic industry now uses.

So the argument that I make to corporations is that if they think about areas that are not immediately profitable, but for which you can start with some relatively small effort that produces a product or a solution that solves an important human problem, there's much more capital available for that than you ever would have guessed—free capital. Those social initiatives can do the early-stage work almost for free, and with lots of help from people who really want to help; students love working with this kind of thing, for example. This initial work opens the option for commercialization of these technologies on a large scale.

So those are my two major suggestions: First, fit together the large company with the interested university or research group around solving large problems that have social benefit. Second, arrange that work in such a fashion that the company gets a forward-looking option on products, and the university gets a forward-looking option on possible directions for research. Universities get information that only companies have about what needs really are, even if they're not profitable; companies get real solutions to problems that they can engineer into new products. The United States gets new kinds of jobs. My grandchildren have something to work on! I think it could make a very good partnership for all of us.

Facing Inevitabilities

Now let me just close by saying a few things about inevitabilities. What are some examples of things that are inevitable? I've mentioned reducing the cost of health care in the United States—no doubt about the urgency of that problem. The economic importance of the developing world is another obvious inevitability. The United States is only 300 million people; there are another couple hundred million in Europe. But the remaining billions of people are elsewhere. If you talk to business people in India, their strategic objective is to bring the poor into the middle class. Why? Because the creation of a middle class creates a market. That's the way the world is going to go. We can participate in this movement, or we can choose not to, but it's surely something we should think about.

There are also a host of very interesting inevitabilities around population shifts. The one that intrigues me right now is the management of megacities. The megacity is a city with a population of 50 million people, something like that. Such cities are a recent phenomenon. We have no idea how to manage these things, just zero idea how to do it. But if you think about it, it's not that different in some ways than Ludwigshafen (the central manufacturing site of the chemical company BASF, and an enormously complex enterprise that is part plant and part city). Megacities present many interesting problems: a mass transport problem for food, water, and energy coming in; an internal transportation problem to move people and things around inside the city; problems in managing the flows of information, and disease, and ethnic groups. In many ways, it's a logistics and queuing problem, with sensors everywhere. It needs to be designed to be lean: it's "waste not," for sustainability and stability. The people who probably know the most about this kind of problem in management are companies that do systems integration and large-scale systems analysis. There will be enormous, enormous businesses in the systems integration of megacities. That's an example of an exceptionally interesting problem, and it's an inevitable one.

Climate instability and the management of CO_2 is another. It's absolutely astonishing to me that we know so little about this thing called climate instability, given its importance. We know that we have CO_2 and we know that warming and CO_2 levels are correlated. Since CO_2 production is linked to energy production, what do we do? If you think that somewhere in university laboratories there is a solution for what to do about increases in CO_2, for example the CO_2 coming out of oxygen-fired Chinese coal plants, you're wrong; there is none. Or at least, not yet.

There are many good, hard, meaningful problems: water purity, education, dealing with conflict. Much of the technology that was developed in the United States in the period between 1945 and today was developed as part of our defense industry. That's really pretty astonishing. This isn't happening now because the nature of conflict is different. Understanding exactly how one deals with the issues of modern conflict is really, really engaging and, I think, will also lead to new (and, one hopes, beneficial) technology.

I'll mention one last area of inevitable interest, which is jobs, not jobs *per se*, but rather robotics. The basic problem kids see as they're progressing with their education is a lack of jobs! They don't see that society needs them. People from many corporations have looked at the HR issue from a financial perspective and said, "Our pipeline is good; we don't have to worry about that. We're finding the people we want." That may sound great from the company's point of view, but it's horrible from the point of view of the young people who

are leaving school. What has happened, and one reason the United States has had a recent burst in productivity, is that we have increasingly substituted information systems, and in the future will substitute robots, for people in jobs.

This substitution is, of course, going to make the jobs problem worse. I don't know how to fix that problem. But I think that robots are inevitable, and it's everybody's job to understand how to make robots that can become assistants that amplify the capability of people, as opposed to robots that displace people from meaningful work.

Conclusion

Let me just close by telling you about the companies that I work with now. I have a major effort in diagnostics for the developing world that is intended, ultimately, to provide the lowest cost information possible for use in the developed world. How do we change the information and cost structure of health care in the United Sates? Very low-cost diagnostics is also a small step toward a partial fix of that problem. We are clearly only a tiny part of the changes in global and US health care, but the cost of diagnosis is the problem we're trying to deal with.

We have a company that is trying to do surface chemistry and nanotechnology, basically trying to manufacture very small-scale objects at very low cost on a small scale. And that company reflects many of the interesting problems that come up outside of the medical area. Trying to understand the business model there is a challenge.

We also have a company working on soft robots, which are robots made purely out of elastomers. They don't look like animals; they look like starfish. Their major applications, at least initially, will be as assistants to surgeons. Future applications might include search and rescue, where the robots can crawl over rubble; if the roof falls on them, they're cheap enough that you don't care.

What I'd like to leave you with is the idea that I have never seen a time when there were more really interesting problems to work on—things that society really needs. That's terrific for all of us. We have wonderful research universities and a system of strong companies, a strong financial system, and an integrated market. We just need to find ways of putting the long-term, important problems together with this collection of characters in a way that leads to something that's good for everybody. My personal view is that the thing that will make it more clearly focused and less vague is to focus on bridging that gap between science and products in innovative ways.

And we *can* create jobs for our grandchildren. There is absolutely no shortage of vitally important problems, both local and global; there is no shortage of smart people; there is really no shortage of capital. And we have 25 years to do it. But we do have to find a way around the timidity of the capital markets, the solipsism of the universities, and the inertia of large companies, and we have to remember that there is more to life than next quarter's earnings.