DR. FERNANDO (FRANK) L. FERNANDEZ received his B.S. in mechanical engineering and M.S. in applied mechanics, both from Stevens Institute of Technology, and earned his Ph.D. in aeronautics from the California Institute of Technology in 1969. He was appointed Director of DARPA in May 1998, where he is responsible for the agency's high-payoff innovative research and development.

In 1994 Dr. Fernandez founded AETC, Inc., which specializes in the use of advanced processing technologies to improve the detection and identification of underwater and underground hidden objects. In 1976 he founded Areté Associates, an applied research firm. From 1975 to 1976 Dr. Fernandez was a Vice President at Physical Dynamics, Inc.; from 1972 to 1975 he served as a Program Manager for R&D Associates; and from 1963 to 1972 he was with the Aerospace Corporation.

Until his appointment at DARPA, Dr. Fernandez was a member of the Chief of Naval Operations Executive Panel, where he provided advice to the CNO on a variety of issues. He is a former Director of the Green Foundation, a nonprofit endowment for earth sciences, and is listed in *Who's Who in Science*. He is also a member of the New York Academy of Sciences.

Dr. Fernandez spoke at the Millennial Challenges Colloquium series on 28 April 2000. The text of "DARPA: Into the Future" follows.

DARPA: Into the Future

Frank L. Fernandez

I am very happy to be here. I want to focus, after giving a little background on DARPA's mission today, on some areas where I'd like to continue to solicit help from places like APL, which has the intellectual horsepower that is going to be needed in the 21st century.

Some exciting and challenging things are happening at DARPA, and some of the changes that are affecting the DoD are extremely new. First, the threat that we once knew and understood from the standpoint of national security is shifting faster than most of our standard techniques are capable of defining. This is the kind of problem that will be routine in the future. The second factor is the globalization of critical technology. We don't own in the Defense Department much of the technology that is critical for national security. That's a big difference. When I was doing research many years ago, we could prioritize: if we didn't get funded to work on five efforts, we worked on the top four and did the fifth a year or two later. Today we have to respond to challenges from the technology standpoint that other people are driving, a very different model than the one many leaders have been accustomed to. Third, the rules of engagement are changing. Biological warfare allows an *individual* to wage war, a distinct scenario shift from a nuclear war model where sophisticated engineering skills and machinery are needed. Also, with the emphasis on peacekeeping and coalition warfare, we have partners to deal with constantly. Our Navy can't just fight another navy. They have to work with other people in a coalition environment. And finally, we have taken technology to a point where we can bankrupt ourselves. So the affordability of technology becomes as important as performance. We can't just ask for the best because we can't afford the best that technology has to offer.

One point made by Peter Drucker[1] in a recent book is that change today is the norm, not the exception. Most of us would rather live in a time where predictability and control are the norm. Well if change is the norm—and I think this will be the case for the next 20 years—we have to accept that fact. We really can't manage change because most of what's going on is being affected by outside forces that we can't control. We have to adopt a strategy that lets us stay ahead of change. We have to run faster. Words like speed, flexibility, and adaptability apply. We need to plan in such a way that we can adapt and react to a future we can't predict.

Some wise people over 40 years ago created DARPA, which has really been structured to be the change leader for the Department of Defense—from the space days, to the Internet, to stealth. Some of those changes have become standard and have had incredible effects on the DoD. Looking back to 13 years ago, for example, DARPA started funding work at universities on devices called microelectromechanical systems. They were an oddity. Now, work we are doing in some of our most advanced systems with respect to radars and transmitter/receiver modules, etc., is totally dependent on this technology, which is having a significant impact from the standpoint of weight, cost, and all the things that count when you're building systems. Significant gains are constantly being made by looking for change for defense.

MISSION AND APPROACH

I have broken down DARPA's mission statement—Technical Innovation in Support of National Security—into three general areas:

1. Solve national-level problems
2. Enable operation dominance
3. Develop and exploit high-risk, high-payoff technology

National-level problems are those that threaten our national survival. Our responsibilities in this regard within the national security environment are not well established. The second area, operational dominance, is where we're looking for ways to solve some of the problems that will help realize the vision of the CINCs and Joint Staff, i.e., we'd like not just to win wars but to dominate military situations. Finally, our core focus—high-risk, high-payoff technology development and exploitation—is really over the long term what has brought DARPA continued success. I will be talking about these three areas later in more detail.

DARPA is DoD's enabler for innovation. Innovation is much more than invention. It is taking an invention and using it to change the way business is done in a large organization. Innovation can be sparked by technical, financial, or operational inventions, but it goes far beyond the initial idea. It means taking the idea into the operation and using that idea to change the operation itself. People like Dr. Clayton Christensen[2] of Harvard have argued that there are really two kinds of innovation (Fig. 1). There's the well-planned, evolutionary, sustaining innovation, which is where you have a road map and, based on this road map, you change the way you do business in a fairly predictable manner. Most good organizations have such road maps. The second kind of innovation is radical, disruptive innovation, where something new happens that initially doesn't fit into and probably doesn't improve upon the current way of doing business. But if this radical innovation can be nurtured, it will eventually far exceed the performance of the standard business model.

How do you look for and nurture these radical innovations? First you need patience, because changing the way an organization does business takes time, no matter how much you try to accelerate the innovation's integration into the total business operation. And because it takes patience, the innovation will need champions who are willing to go into it for the long haul. Nuclear submarines were not built in a day. It took a long time to learn how to operate a nuclear submarine force. And if it had not been for certain people in the Navy like Admiral Rickover who had champions in the Congress, the whole concept would have been killed several times over—by the Navy itself!

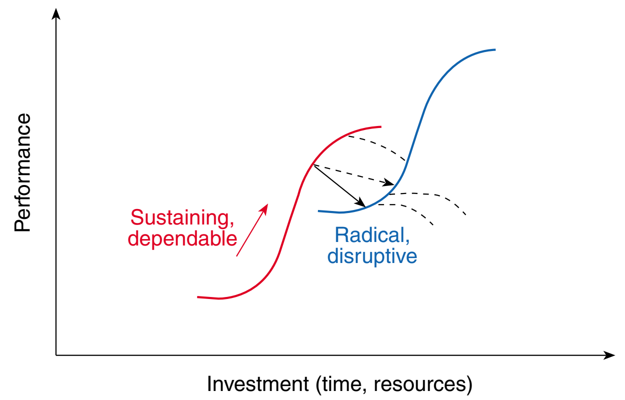

Figure 1. The two kinds of innovation are contrasted in terms of performance and investment.

Innovation is risky. The first thing that usually happens with a radical innovation is that it challenges an ongoing business area. Let's say you're the CEO of a pharmaceutical company whose job is pain relief and your research lab comes up with a brand new way of providing it. As soon as you start to promote this new way of doing business, the vice president in charge of aspirin sales says, "What are you doing? You'll be competing with your own cash cow." And the answers is yes, but soon we won't be making any money from aspirin sales and we have to go for a new product. That requires leadership. If the leader doesn't handle these problems and lets the system take care of itself, the odds are that the innovation will be killed.

Innovation also requires dedication, usually from a zealot, somebody who will bank their career on it. And so in almost every case where there has been some radical innovation that has persevered, you find two people: the zealot—somebody who wants to get it done—and the champion—somebody in a powerful position who protects the zealot until the innovation starts to mature. Again, this new way of doing business may not perform as well as the standard ways that have been honed down to the last degree. And it's risky. It's risky because it challenges the same organization. It's risky because it depends on people. If something happens to some of the people who are involved, there's a good chance that you'll be back to square one because innovation is extremely people dependent.

For example, you all know about the Arsenal Ship. At the time, I was serving on the CNO Executive Panel. We were the ones who recommended to Admiral

Borda that he cut a deal with DARPA to see if there was a new way to build ships. Why did Admiral Borda want to do that? Having been a surface ship driver before becoming CNO, he looked at the numbers and said, "If I continue to build ships as usual, the only way I can reach a force level of 120 warfighting ships is if they are all frigates." There was *no way*, with the current model for building ships, that he saw having enough capital for a real Navy. So he and Larry Lynn, my predecessor at DARPA, cut a deal. Where did Borda get the money? He took it by slowing down the programming for an aircraft carrier because the Navy's contribution to this deal had to come from somewhere. Admiral Borda died, and the deal went south. Why? Because when one of the champions who was critical to the deal died, so did the Arsenal Ship. Now a lot of the technology that the Arsenal Ship pushed is being used on the DD 21, so good came of this also. But it just goes to show how risky innovation of this kind can be.

I want to stress that DARPA is not in competition with the Service R&D infrastructure; we complement it. The DoD requires both the radical kinds of innovations that DARPA is chartered to do and requirements-based R&D. DARPA is "bottoms-up" driven. Program managers come to the DARPA director and sell a program. If the program managers don't sell the program, the program doesn't exist. We have never had successful programs at DARPA where the director has said, "I have an idea for a program. You go run it." It just doesn't work that way.

DARPA is also allowed a great amount of process flexibility. We don't have a formalized process. We integrate our research. We perform 6.1–6.3 work together in many of our programs. In the DoD, under Service R&D, work is segregated and maintained under different control. We're chartered to effect radical change, which means that we're allowed to fail. Much of the Service R&D has to be reliable. That piece of R&D and science and technology has to be available because an acquisition timeline depends on it. DARPA, on the other hand, is not in anybody's acquisition schedule.

DARPA is also unique. We're a central DoD agency. We do not have to support any one service mission. So if, over certain periods of time, DARPA can help one service verses another, the agency will emphasize work in that area. For example, in the 1970s, we were very involved with the Navy in surveillance. DARPA started work involving nonacoustic anti-submarine warfare, and the Navy took it over after a while. This was a case in which an activity began at DARPA and then was transitioned to the Navy, where it became a real program as compared to just a concept. Finally, our job is to do away with what we are doing, i.e., planned product obsolescence, whereas one of the most important jobs in the Service R&D structure is to ensure planned product improvement.

DARPA is a small organization. About 120 technical people, including the Director's Office, run the agency. Again, flexibility is our most important approach. We're more like an investment firm than an R&D lab. The program managers have ideas, but all the work is done through places like Hopkins, other universities, other government laboratories, and industries—wherever the good ideas and smart people are. DARPA's job is to find those ideas and people and invest in them. We try to maintain a minimum established constituency. We don't *own* wind tunnels, we *rent* them. Without a constituency, however, we have to worry about protecting our budget in Washington. One of the best ways to protect your budget is to have a constituency, a representative, to take your case to. So the good news is that we have very low overhead; the bad news is that we have very low overhead.

We constantly rotate our programs and program managers. People spend about four years on average at DARPA. Why? Since we're probably not going to grow (because the government and research don't grow very much), the only way to make room for new people is to rotate personnel. It's not the best situation, but it is the lesser of the two evils when your job is to fund contractors. If you keep investing in the same people, it's very hard to get new players. The key is to go outside of the established group of people for new ideas. A lot of the most important new ideas are being generated outside the DoD control spectrum. Since we don't have much continuity, we depend on institutions like APL and industry to provide it. Our program managers will come and go, but the smart people in places like APL will continue on to provide the understanding it takes to do the things that DARPA paid for them to learn how to do. That's really where the continuity is.

DARPA is DoD's enabler for radical innovation because of three unique attributes. We have a much broader horizon than venture capital firms and commercial ventures in that we can work with a professor at a university and at the same time build prototypes with the National Reconnaissance Office. We can be much more focused than university researchers because we don't have to fund areas, but rather possible outcomes. Much of our work is at the universities, but it's not 6.1. They're working on areas that are more applied. And DARPA combines 6.1 and 6.2 to get things done.

Finally, we are not bound by Requirements, with a capital "R," where requirements exist once a decision has been made to buy and there's an acquisition plan. That is, we can work on hard problems without worrying about arriving at a solution by a given date. Somebody long ago understood that if you tell smart people that they must solve a very difficult problem and they have to do it by Thursday, there's a good chance that they'll dumb the problem down. They'll get you the solution you need by Thursday, but it may not be the solution that would have gotten to the heart of the problem. That's what people tend to do if they are held to impossible schedules. DARPA is not bound by those schedules.

CURRENT FOCUS AREAS

DARPA, as I noted earlier, is focused on three strategic areas. In national-level problems right now, we're concentrating on "protection from biological attack" and "protection from information attack." On the question of operational dominance, we're looking into "affordable precision moving target kill." We are very good at still targets, but because our enemies know this, they're moving and hiding. So the issue is how can we counter this in an affordable way? The real question for "dynamic command and control" is how can we go from mobile command and control to planning and replanning in a hurry? Our military is good at planning. They are *constantly* planning. So the technical problem is not with the planning. The technical problem is how do you replan rapidly and adapt to what's going on in near–real time when your plan is being attacked. Now that's a very difficult problem. If you want to try to automate planning and replanning to reduce large numbers of staff, it becomes a very challenging technical problem.

"Mobile networks" is one of the most challenging technical problems I've seen. Everybody talks about the digital battlefield and how we are all going to be talking to each other and how the commercial world will give us a solution to this problem. That's hogwash. They're not working on the problem of security (and I don't mean encryption) or networking in a constantly changing environment. They're not working on mobile links, as the military does, where you have to deal with legacy systems, where your networks are being jammed, or where propagation and routing tables have to be adapted and constantly switched. The commercial world has no reason to work on these problems. There's no market yet. One may evolve, but right now it's an area where the DoD has to step up and take point.

An example that is critical to the military is logistics. Trying to develop logistics for "planning and replanning in near–real time" when the logistic train is attacked is extremely important. What do you do when the ship sinks or when you suffer more losses than anticipated? How do you divert the resources? In terms of military spending, most of the money directed to warfighting is in logistics. The heart of the matter is whether we can modernize logistics using advanced information technology.

"Future warfare concepts," another facet of operational dominance, includes several major areas. For "hard and deeply buried target classification," DARPA isn't working on any weapons. Instead we're trying to figure out how to tell the person who's got the weapon where to put it. How can special forces use robots to get into places where they can't fit or survive? How can we improve reconnaissance? How do we conduct "combined manned and unmanned operations"? We already have unmanned warfare and unmanned platforms. We have satellites and unmanned surveillance vehicles. The next question is whether we can carry out combat. I think it's too hard to do *only* unmanned combat. Are there situations where teams of unmanned and manned vehicles can work together—the unmanned vehicle doing the more dangerous things mechanically that people can't do while control remains in the hands of the human being? Finally, "asymmetric threats" have emerged in the last few years where terrorist tactics are being used to inflict serious damage against us, our embassies, our bases, etc. This is another area where DARPA is asking whether we can do some smart things to counter the threat.

The third strategic area is high-risk, high-payoff technologies, where we're looking at materials, electronics, and microelectronics. We're also interested in something called "beyond silicon CMOS," which I will discuss later, and the "intersection of biology, information sciences, and microsystems."

KEY INITIATIVES FOR THE 21ST CENTURY

Here I'd like to give you some specific examples of exciting initiatives in support of DARPA's mission. One example is the time-of-flight (ToF) mass spectrometer (Fig. 2) being built by APL. If it works, it could really make a difference in biological detection. The idea is very simple but intriguing. You take a piece of videotape coated with a laser-absorbing matrix and deposit the particles from an air collector onto it. The resulting

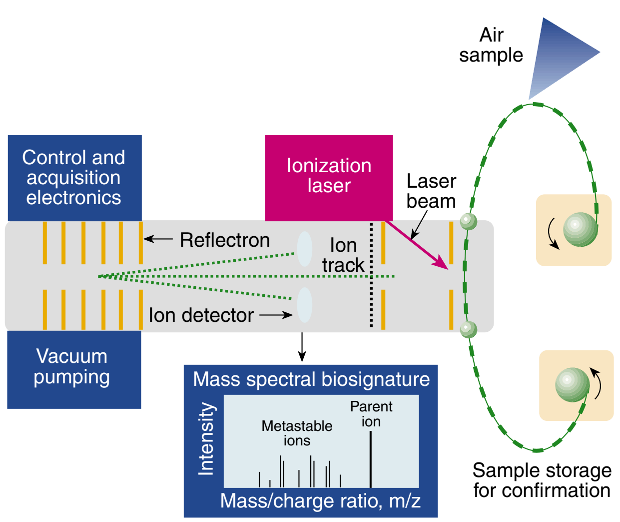

Figure 2. Schematic of a miniature time-of-flight biodetection system developed by APL.

sample is transported through a vacuum interlock into the source region. You zap the sample with a laser, and some fragments get ionized. Then, by doing ToF measurements, you obtain the mass spectrum of the fragments.

This technology has yielded some incredible results; for instance, whole proteins can be desorbed and identified in the spectrum for the precise identification of agents. It can work rather rapidly and may allow us to carry out some very interesting detection and classification, although some issues are still unclear. Nobody quite understands why, for example, zapping the sample with a laser allows you to pull out a whole protein but doesn't cause everything to disintegrate. DARPA is working with APL to understand the entire desorption, ionization, and detection process. How does it work as a function of the laser parameters? How do you optimize the sample preparation? The goal is to achieve complete understanding through modeling of the MALDI/TOF process toward building a reliable detection scheme.

Another technology is the Affordable Moving Surface Target Engagement (AMSTE) Program (Fig. 3). In the near-term, can we design and demonstrate the capability to effect networked targeting, tracking, and classification of moving vehicles on the ground using available platforms? The key is *not* to build a whole new set of platforms but to use available GMTI (ground moving target indicator) radars on platforms like F/A-18s, F-16s, JSTARS, and Global Hawks. Can we network them to solve difficult obscuration and accuracy problems encountered with the normal GMTI radar? Can we get a track that's sufficiently accurate to enable putting a unitary warhead on the target? Can we provide a link to the warhead to give it updates in flight that would tell it to go to a slightly different point than the one initially designated? The challenge is to make this happen with little enough latency that you can predict the target's position at the point where you have to put the weapon on the target. Can we accomplish this over existing links, or do we need to design a whole new set of communications links for our aircraft? We are trying to demonstrate that this capability is possible using available systems but with smarter algorithms.

The near-term plan is to see if we can guide a Joint Direct Attack Munition to within 10 meters of a single moving target in an uncluttered environment using available resources (F/A-18s, F-16s). Then, by 2002, the plan is do the same thing in a cluttered environment, where we can hold a target for 30 minutes in track and then put a weapon on it while it's still moving. We may get the required accuracy if we have a minimum of three radars on the target most of the time, or perhaps eventually two. But three radars might be needed to resolve the ambiguities. We are working this issue with the Air Force. They will be supplying some of the experimental setup and platforms, and we will be providing the algorithms and some of the links. This also needs to be wired into the current GPS position fixes that the missile can get right now.

If this works, we take it to space. With Discoverer II (Fig. 4) we use the same idea but move it to a network of satellites. Moving target indication from space has never been done before. Networking satellites is a much more difficult problem. If this concept works, can we build the satellites at an affordable price? We're going to do an in-space experiment, I believe in 2004, with two satellites to see if it works. After that the military can decide if they want to adopt it.[3]

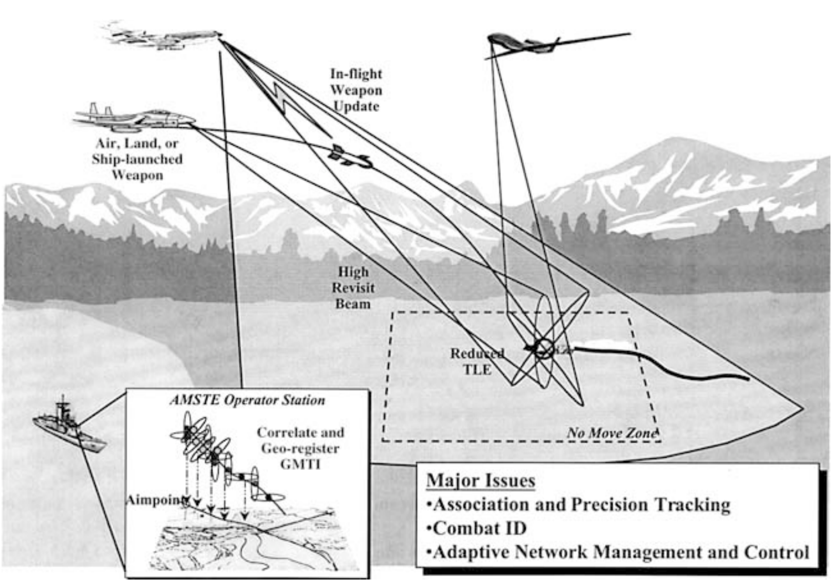

Figure 3. The Affordable Moving Surface Target Engagement (AMSTE) System concept (GMTI = ground moving target indicator, TLE = target localization envelope).

The advantage of this technology is that nobody can hide behind mountains anymore. The minute you get up into space and can network, everything becomes visible and the obscuration problem goes away. But there are difficulties, particularly because of scope. When a network of satellites approaches from different angles, it's hard to do deception. Tasking these satellites tactically in real time also is a concern. You have to be able to handle requests from many users, appropriate and apportion the satellite resources in near–real time, and enable the satellites to adaptively use databases to obtain information on local terrain, etc., so they can track accurately.

The Army will also be looking into the use of the Cooperative

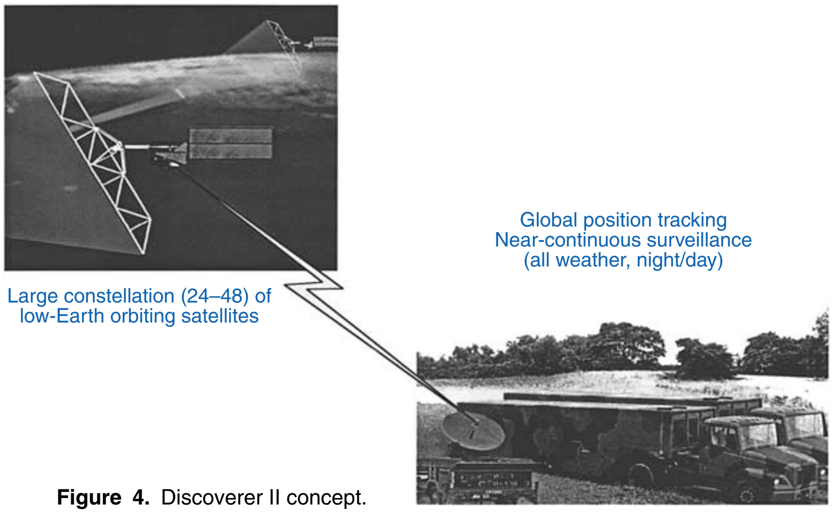

Figure 4. Discoverer II concept.

Engagement Capability (CEC) concept to help them solve a problem with the M1 tank. The M1 is extremely lethal, very survivable, and virtually nontransportable by anything that can go fast. It weighs 75 tons and requires an enormous amount of logistics support in operation. It was designed with the Cold War in mind, when we would preposition and predeploy our forces in a particular area and then fight there. Now the Army has to be able to go everywhere rapidly. The only airplane that can carry one M1 is a C-5A (I don't think a 747 has a ramp that will take it). The Army needs to fit M1 capabilities on something like a C-130, which can land almost anywhere (and we have a lot of them). When they asked DARPA if we had any ideas, we told them they'd have to change something basic.

The first step is to look at the functionality that goes on inside the tank. On my own visit to Fort Hood I saw four crewmen: a driver, a commander, a person shooting the gun, and another loading the gun. These people work as a very tight network inside the tank, and they all have to be protected. Can we take these functions (Fig. 5)—observe and communicate, shoot, and move—and separate them physically so that the human being is only in *one* tank? As it turns out, the weight of the M1/A1 tank is determined primarily by the volume of space that has people in it. You have to have a certain armor around a certain volume that dictates 70% of the weight of that tank. Why is the tank so big? Could we separate these functions and then have them work functionally as if they were doing the same job with very tight, very high-bandwidth local area networks? That's really what it comes down to.

Next, can we place human beings in only one of those tanks and make the others robotic and smart enough to tell the operator at their instruction base what they're doing? Then can we change the rules by giving the tank the line-of-sight capability to look and set fires? The tank has always been limited by its direct fire capability. Can we give it a beyond-line-of-sight surveillance capability so that it would not be limited to the 4 km that it is limited to now by its direct fire weapon? Can the tank also have, as part of its fire control system, indirect fire capability? Can we position the people further back and the robotics systems up front so that they take the first shot? Finally, can we make the tank light? The numbers show that if the people are in the back, we don't need as much armor. For a gun without a face, we're talking about much lighter systems; substantially less than 20 tons (as opposed to 75) might work as far as the platforms are concerned.

Can we solve the robotics problem, that is, the instruction problem? Can we solve the networking problem? Can we learn how to do network fires and network command and control? We're looking for good ideas. We have four or five major technical thrusts that are running in parallel involving mobile networking, network fires, robotics, command and control vehicles, and beyond-line-of-sight capability.

I'll give you an example of nonlinearity in the battlespace (Fig. 5). For a vehicle up to 20 tons, you use

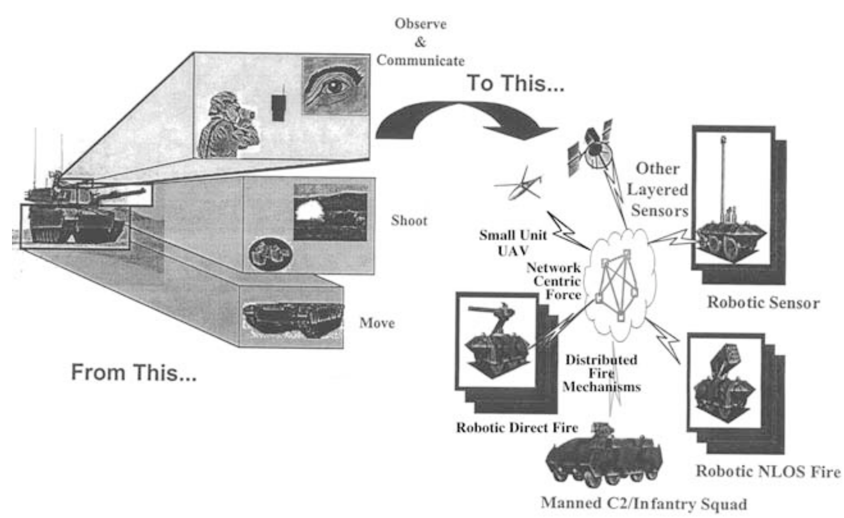

Figure 5. Future combat systems: network-centric distributed platforms.

wheels. Once a vehicle exceeds 20 to 25 tons you *cannot* keep the wheels from sinking into the earth, so you have to use treads. But the overhead cost of the treads, cogs, etc., becomes astronomical. And that's why most construction vehicles built today are lighter than around 20 tons. So there are all kinds of nonlinearities. The scaling of the weight of armor with human volume is a very nonlinear effect. The networking question is how will the units come together and become new networks, i.e., how will these networks re-form? DARPA has 3 years to come up with and demonstrate this concept for the Army.

We're doing the same thing for the Air Force with the Unmanned Combat Air Vehicle (UCAV) System concept, i.e., looking at getting groups of manned and unmanned combat vehicles to work together. Will we give tasks like enemy defense destruction to an unmanned vehicle? That's a very dangerous undertaking because those air defenses tend to shoot back. The manned vehicle can take on tasks like strike, where a human eye on the target is needed and we want to get away from collateral issues. We do this now with cruise missiles, but the problem with cruise missiles is that they don't come home. Can we perhaps make three vehicles smart enough to work together to rapidly triangulate on a radar, even if it only comes up for less than a second? If so, these vehicles could be lethal. They can be small, which makes them harder to see. They are cheap. When you go "small" and don't have people, you can make everything smaller for a decent payload. You can use all-electric drive for flaps and everything else, and no hydraulics are required. It's another case of a very nonlinear effect. If we can learn how to go small, the proficiency problem goes away. This concept would have an incredible impact on the way we approach warfare and on some of the difficulties with all the proficiency questions. The Air Force is now into the demonstrator building phase of this effort.

The Navy is starting up a study to see what they can do with this idea as well. The difficulty with the Navy version is that, unlike the Air Force, they have an airport problem. DARPA and Admiral Johnson have agreed to go 50/50 on the study phase. UCAVs could perform close-in jamming. They won't need a human being. We could put them into places where we would never put a person with a jammer, and then send them home to change payload later.

Other areas of interest in terms of asymmetric threat include human identification and wargaming the asymmetric environment. Are there ways that we could automatically achieve force protection? Can we model human behavior to extract information from open source literature rather than relying on intelligence? Can we use the technology to predict human behavior and thus prevent an attack (Fig. 6)?

We are also looking at "BioFutures," a term that means the use of biology's understanding of complexity to develop new approaches to algorithms, software, components, and systems. We're interested here in modeling the dynamic behavior of complex biological systems and using information technology to better understand them. Can we simulate the dynamic behavior of complex biological systems? Biological systems can effect incredible tracking and homing with very small processes. The goal, then, is to understand how they work.

In addition we are exploring "beyond silicon CMOS," that is, demonstrating the feasibility and design of powerful information technology devices and systems using approaches that extend beyond traditional CMOS scaling (Fig. 7). DARPA did a lot of pioneering work in advanced integrated circuits. Now we want to know what happens after silicon. We're really talking here about nano-sized features. This is a long-range area, and right now there is no military funding for it.

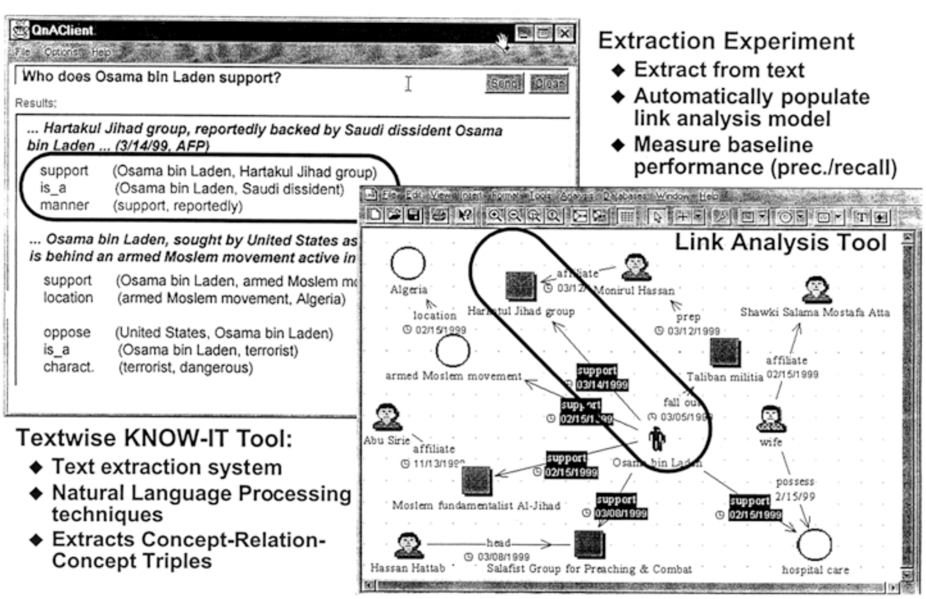

Figure 6. Evidence extraction and link discovery.

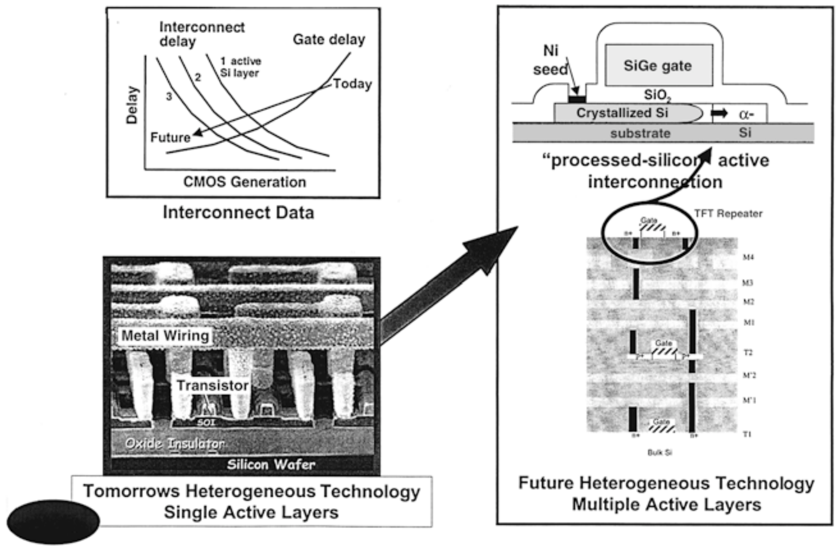

Figure 7. Going "beyond silicon CMOS" is expected to yield powerful information technology devices and systems.

CONCLUSION

In closing, I want to stress that DARPA needs smart people, people like you at APL, who understand areas like tracking, hitting, and shooting. We're looking for people with the experience and expertise in networked warfare who can help with DARPA activities like the AMSTE Program, the Navy UCAV, and the Army Future Combat Systems. We're interested in other technical areas in which the agency can partner with the Navy. Some very exciting things are happening at DARPA, and we encourage your participation.

REFERENCES AND NOTES

1. Drucker, P., *Management Challenges for the 21st Century*, Harper Business, New York (May 1999).
2. Christensen, C. M., *The Innovator's Dilemma: When New Technologies Cause Great Firms To Fail*, Harvard Business School Press, Boston, MA (1997).
3. In July 2000, the FY01 Defense Appropriations Conference Report terminated the Discoverer II Program.