Dr. Craig Fields
1974-1990

Interview: March 15, 2007

Interviewer: Can you tell us your name and describe your tenure at DARPA?
Fields: Well, Craig Fields. I arrived at DARPA in March of 1974, and left in early 1990—so that's a pretty long tenure. One of the few Directors who came in as a program manager and then stayed and became Director after a long time.

I: How did you happen to come to DARPA?
Fields: Well, it was an accident. A man named J.C.R. Licklider—and that's a person whose name I hope arose earlier in your investigation—is, to my mind, the father of computer technology in this country, although he gets little credit for that. And he was a friend of mine. He was a professor at MIT. I graduated with my Ph.D. from Rockefeller University and went up to Cambridge a few months before starting the job on the faculty of Harvard, and we became acquainted and friendly. And then a few years later, he said, "How would you like to come to work at DARPA? I'm going down there for my second stint as Director of IPTO (Information Processing Techniques Office), which was the computer office, and, whimsically, I said, "Sure." My wife was getting her Ph.D. at the same moment from Harvard, and so it seemed, since she was going to look for a job, that if I was going to accept the offer, it was good timing.

So, we said, "Let's go down and try it for a two-year experiment and just see if we like it, and then we can go off and do something else." And two years became three, and four, and five, and there you are.

I: Why did it become such a long stint?
Fields: Well, I took my own advice, which is not common. I always tell people that you should not go from a job to another job just because there's something particularly unattractive about the job you have, but that there has to be something really attractive about the job you're going to get. And every

opportunity that arose was just not nearly as good as DARPA. DARPA's a terrific place.

I don't know one Director—although the other Directors presumably spoke of themselves, or will—who ever left DARPA to a better job, a more enjoyable job. Jobs with higher ranks, or with more compensation, certainly, but if you look at the entire picture of enjoyment, there's nothing better. And I still look back on it and think that way. So, every year, every day, I would think, "There's just nothing better," so I just stayed.

I: Dr. Ruina said, "DARPA's a fun place to be."
Fields: It really is. For technologists, scientists, it's the best place to be.

I: Who was Director when you came in?
Fields: Well, it was Steve Lukasik. He's a friend who lives here in Washington now.

I: And what was DARPA like when you came in? Did it meet your impressions?
Fields: It was about what I thought, and, interestingly, it's about what it still is. As you know, it's common for old timers to look back and say, "Oh, the good, ol' days. Things aren't as good as they used to be," and so on, but, actually, I think they're as good as they used to be. You know, things change, some things get better, and some things get worse; but, on average, I think it's still the same wonderful place now that it was then—namely a place for entrepreneurs to be able to pursue an idea, and it'll succeed or fail, and there's no great stigma if it fails. And with enough resources, you can actually get something done, and you can get the satisfaction of some accomplishment. So, I think it exceeded my expectations then, and it's the same now.

I: What makes it survivable like that? It's unique in government.
Fields: Well, there are a few unique aspects. It's actually unique on Earth. When I was there, oftentimes I would visit other countries, or the representatives of other countries would visit me wanting to know how they could reproduce DARPA, and it's never happened. I think it actually won't happen, because you need a combination of factors that are hard to achieve elsewhere. There are some cultural issues. You need a country with a culture where there's tremendous entrepreneurship, and a willingness to fail—failure isn't the end of your life or the end of your career and people with enough self-confidence that they're not holding onto a job forever. The word "career" has an old-fashioned meaning. People are willing to take something for a year, or two, or three, and accomplish something. A country that's large enough so that a small percent of the people are still a large enough number of people to actually populate an agency. I mean, there are smart people everywhere, but if you want critical mass, and it's a tiny percent of folks who are good enough to do the job, you'd better start with a big number in order to find enough.

And so that kind of size and culture is not so easy to get elsewhere, and then you need it to be housed in some kind of organization. I think DARPA

benefits tremendously by having a customer such as the Defense Department. A lot of what DARPA does helps people who aren't in the Defense Department, but it *always* helps people in the Defense Department, and that keeps it grounded, which I think is very helpful.

No organization is willing to take huge risks with all of its budget, or most of its budget, or even a lot of its budget; but organizations are willing to take risks with a *little* of its budget. So, imagine you're willing to take a risk with a third of a percent of your budget. Well, you'd better start with a big budget for a third of a percent to be big enough to make a difference.

The Defense Department has a big budget. So, try to find another country with an organization as big as DOD that will take the same kind of risk with a tiny percent and, yet, end up with a large enough number to actually make a difference. That's hard.

So, I think it's sort of unique in the way it runs, and we should be glad we have it.

I: The ability to accept failure because it operates on the edges of technology?
Fields: Um-hum.

I: So, if you assume a large risk, you assume a risk of payoff and a large risk of failure?
Fields: Well, I think you're getting into an important part of the calculus, which some people misunderstand. A common statement is that we seek high-risk projects. Well, no one seeks high risk. You seek high-payoff projects, and you know inevitably high-payoff projects are high-risk. And, inevitably they're long-term. Oftentimes, they're costly, but the goal is high payoff.

I used to love projects that were high-payoff, but also low-risk—low-cost and short-term—but there were hardly any of those. So, you know: what's the cart and what's the horse? You want projects that are very high-impact, and that's the focus, and that's usually accompanied by high risk.

I: Where do these projects come from?
Fields: Everywhere. The most important source, from my perspective, is program managers in DARPA who have ideas about which they are passionate. It's hard to find any substitute for passion, and when you have passionate program managers who will do absolutely anything to get their project to succeed, and then you've got really something golden.

Oftentimes, they come from the contractor community—universities, nonprofits, companies—and that's great, too, because then there are passionate people ready to actually do the work; because if no one's interested in doing it, it doesn't matter. An idea is only as good as, you know, the paper it's written on until someone acts and really executes.

Sometimes ideas have come—but not all that often—from what I'll call "customer demands." You'd like to think, in a sort of abstract sense, that a customer would say, "I need X," and then DARPA would go off and figure out a way to provide X. Realistically, that rarely happens.

I: A lot of times it comes from the customer.

Fields: You know, the ideal that people talk about is a customer who says, "I need something," and then DARPA takes that information, that instruction, and then goes off and figures out how to do it. And that just rarely happens. It sometimes happens, but it's rare.

Much more often, DARPA—either the program managers, office directors, or sometimes even the Director—has an idea. Contractors, the private sector has an idea, and then the idea is brought to the customer and they say, "Look at what you could do," or, "Look at the problem that could be solved." So, it's the opposite of what you might think, but it works really well.

There's a lot of debate about which comes first, the push or the pull. Actually, you just need both at the end. If you don't, if you have push without pull, or pull without push, it's a failure.

I: You were there at DARPA a long time, so you got to see a lot of the evolution of the relationships. Can you talk about how those dynamics shifted and changed?

Fields: Sure. Well, I don't think it shifted and changed. If you take a long view, I think they exhibited the symptoms you'd expect of human behavior constantly, and that's not a bad thing. Customers aren't quite sure what they want. After they get it, they're not quite sure they want it. After they use it, they can't live without it. That's normal behavior. There are any number of R&D organizations with very high levels of excellence throughout DOD, not just DARPA, although I have a special affection for DARPA. Sometimes there's competition among them. Sometimes there's cooperation among them. It's just human behavior, so I don't think there's been a very obvious, long-term trend.

In the short run, there are always changes. At the moment, for example, DARPA's doing a slightly higher fraction of work that's highly classified than in the past, but it has been that way in the past, too. I mean it goes back and forth. So, I can't really paint a trend line I believe in. Maybe I've seen too much.

I: Are there shifting answers to the question: Why DARPA?" Why do we have DARPA?

Fields: Well, that question arises all the time, and it's answered really not only by principle it's easy to read the charter and say, "Here's what DARPA is for," but it's also answered by the practice. DARPA has a track record of delivery that is simply astonishing. I remember one year, I was reading an industrial magazine, and it was giving 20 awards for technology innovation for the previous year, and seven of the 20 were DARPA projects. That's a pretty good track record if you realize the 20 covered the entire United States.

So, when you build up a track record and a backlog of success, people come more and more to believe in you and that practicality is more important than the statement of principle. It works fine.

I: You got there under Lukasik and he was struggling as Director because he'd lost almost 50 percent of his budget and 50 percent of the personnel through

projects being transitioned to other places. Was there that same sense when you walked in?

Fields: I didn't actually get that sense. I was working on the ARPANET. That was exciting, so I just never perceived that feeling. Moving out of the Pentagon strikes me as a great success, being far away from filling out all those forms. What a wonderful success! So, I don't think Steve really was sorry to move out of the Pentagon, but, again, he can speak for himself.

As for losing big parts of the Agency, that happens with some periodicity. SDI, Stealth, any number of things started in DARPA and then, you know, like an iceberg floe, breaks off. You can—in a bureaucratic sense—be sorry you're losing people and budget; in one sense you're unhappy. On the other hand, look at the great success from the point of view of technology transfer. Even if you get no credit, you have satisfaction. So, I don't actually view either of those events as bad events, and when they were repeated later, I didn't view them as bad events—like with SDI and the Stealth, as I said.

I: Let's talk about ARPANET. Can you tell us about the history of how that happened?

Fields: Well, Lick certainly was the visionary, in my mind, who foresaw a lot of what could be, and was sort of a mentor who nurtured the projects along. With the passage of time, there's a lot of revisionist history, predictably. I'm absolutely certain I didn't invent the ARPANET or Internet, so let me establish that absolutely clearly.

I think one of the earliest incarnations was Aloha Net in Hawaii, a three-node net, that established a lot of the ideas, and then it expanded and, of course, was onshore. Any number of people contributed to it. My recollection from being there every day from 1974 on was that I never heard anyone actually state a clear vision that is the same as what we now see. So, I think there were a lot of good accidents, but accidents nevertheless.

In the early days, there was certainly conversation about using it for communication, and there was a lot of motivation to provide robust infrastructure for the Defense Department. You know, everybody has their own motive for doing things, but the wonder that has come—this once-every-500-year-event, like printing—is a fabulous thing if you combine the ARPANET merging into the Internet. It had a lot of accidental elements. I think, you know, everybody who contributed should get some credit, but probably no one really invented it—including my friend Al Gore, who was just a terrific supporter of it, I might add.

I: You mentioned accidents, and I'm curious—what role does serendipity play in a lot of these developments?

Fields: It plays a large part. First of all, things fail. Despite your best efforts, they simply don't work. That may or may not be what you'd call serendipity. Let's leave that aside, but it's actually a large factor, and it always has to be remembered.

Secondly, even though projects are done for a purpose, there can be side effects that are even bigger and that you never predicted. In fact, we were

talking earlier about the early work in computer simulation and gaming as a way of training. There was a situation in the early '80s where there wasn't enough money in the defense budget to give adequate training time to soldiers to actually go out and shoot guns and drive tanks—fuel, and so on, and so forth. And without training, performance suffered—so that was a bad thing.

And so folks said, "Well, we can use simulators," and about that time there was the rise of very high-quality, high-fidelity simulators driven by DARPA technology. Ivan Sutherland—Evans and Sutherland displays things like that.

But there were too few of them, because at $15 million or $20 million each, you're just not going to have a lot of simulators, hence, not a lot of soldiers who have a chance to practice. And so one of our staff, Air Force Lieutenant Colonel, later Colonel Jack Thorpe, came up with what turned out to be a brilliant idea, but which could have been completely lost. His idea was if we could produce a simulator that was low-fidelity and low-cost, maybe it will allow students to learn just as well as high-fidelity. Maybe you don't need all of those brilliant and beautiful displays that show you every tree and every leaf, and you'll be able to actually learn. So, we produced a number of these multi-thousand-dollar, instead of multi-million-dollar, simulators and lo and behold, students learned just as well.

Then his second innovation was to say, "Is it possible to network several thousand of these together so that several thousand soldiers can concurrently learn, and you can basically teach how teams work together? Imagine that you're in a tank simulator looking at your low-quality display, seeing another tank elsewhere rotating its turret to point its gun at you, while somewhere else in the network there's a soldier in another simulator who's turning his turret to point at the tank on the screen, and that *is* you." So, that was the vision.

And, again, it might not have worked. And we brought together a set of contractors to deal with the issue. We told them the idea. We said what we wanted to do—and realize, this was a long time ago—and their reaction was, "That's absolutely impossible. It can't be made to work. It's too hard, much too expensive and so on." And this was on a Friday. And I worked on it over the weekend and thought about it and reconvened the group on Monday, and I said to the group, "Well, here's my idea for how to make it work." And the contractor group, they all said, "That's a *terrible* idea. We can do a lot better than that."

And they were completely correct. My idea was a *terrible* idea. It would work but it was a bad idea, but they said we spurred them to actually find a solution and, you know, after that it's history. The Defense Department has networks of simulators so that large groups can train together and every night, as a spin-off, 15 million teenage boys—and now more and more girls—fight it out on the Internet battlefield, playing games and a whole industry has been spawned. So, there's a lot of serendipity.

I: The law of unintended results.
Fields: Sure.

I: Are there any other examples that come to mind?

Fields: Well, I'm sure there are. Maybe I'll think of some more during the course of our afternoon, but that's the one that seems most poignant.

I: Now, did the other Services catch on to this and say, "Wait a minute. This is a pretty good idea. Can we use this here?"
Fields: Yes, it is a pan-service issue. It's a joint issue, and in fact, this kind of technology is being used more or less by all of the Services. So, that's a good thing.

And also a good thing is there's not a lot of recollection that it's a DARPA project. We get great success when people embrace it as their own and forget where it came from. So, you know the truism that you can accomplish a lot if you give someone else the credit? Well, it's certainly true. It's human nature again.

I: As you grew did you become more aware of the breadth of the programs that were in the DARPA stable?
Fields: Of course, predictably.

I: Did you become Deputy?
Fields: Um-hum. I was Deputy Director for Technology, before that, Chief Scientist, and a whole string of titles. Each of these jobs was worse than the one before. Well, I think most people would say that the best job in DARPA is being a program manager. You're closest to the technology. You're dealing with your personal passions. You're closest to the university and company contractors, the private sector, and you're having a lot of fun every day. And every step up is a step away. But, of course, if you don't accept it, someone else will. It's human nature.

I: So, you got the call to become Director. How did that happen? Tell us about that.
Fields: Well, I think that's literally the case. I got the call from the Deputy Secretary of Defense to become Director, and we talked, and got along, and it worked out just fine. It wasn't that big a step or that big a difference. Quite different than many of the other Directors who came in from the outside for that job. Here, it was much more an evolution and a continuous change than any discontinuity.

I: Who was the Under Secretary? Was that Herzfeld at that time?
Fields: He was the DDRE, and I have tremendous respect for Charles. He's terrific. We were actually on e-mail this morning.

I: You became Director, and what were your thoughts? Now you're in the driver's seat. Did you look around and say, "How am I going to manage this?" or "How do I approach the control of this beast?"
Fields: Yes. Well, as always, the boss isn't in charge. That's what the paper says; but that's not the reality. If you have to give someone an order, you've failed. It doesn't work, especially when your staff is made up of 150 brilliant

prima donnas. It just can't work.

In my mind, the most important thing the Director of DARPA does is recruit new personnel. You could argue why it's a good or bad thing for DARPA personnel to turn over and to bring in fresh blood. On one hand, you get new ideas, and on the other hand, you lose experience, but I think history shows that the turnover is a good thing. And so, there's a constant effort to find new people who have the right personality characteristics and the right training, experience, and contacts to come in to work at DARPA. Nothing more important than recruiting and retention. It's hard to even imagine what number two is, it's that important. But I'd lived with that for a decade-and-a-half earlier, so, again, there was nothing all that different.

One thing I had to deal with as Director that was a little different than before was a little more interaction with the Congress than previously, but, again, I had interaction with the Congress from day one—a little more focus on strategic planning for the Agency, but really there was no big change. You're sitting in the driver's seat, but you're really not in the driver's seat. The program managers should be, and are, in the driver's seat.

I: I'm curious—where do you find these people? Because people are the key to the whole success.

Fields: People are the key to the whole success. Well, actually, there are several keys. One is people, and that's number one on my list. I'll get back to answering your question, but I don't want to forget the others.

It actually is important to have a good-size budget. I know that sounds crass and crude and rude, and it's low-class observation, but the fact is if you don't have enough resources, you can't get anything done, no matter how smart you are. And so my explanation earlier about needing to get a tiny percent of a big number to still be a big enough number to work with still applies here.

It's important not to commit suicide. DARPA's exempt from no loss. The Agency doesn't do what other parts of the government do, which is create a whole body of optional regulations to constrain itself from succeeding, and so that's actually an important part of the culture. And there's a long list in addition to having great people, but nothing's more important than great people.

So, where do they come from? Well, everywhere. They come from universities. They come out of the Services, like Jack Thorpe. They come from companies. They come from nonprofits of all sorts. It is a constant effort and a constant struggle to find people who have solid technical backgrounds. I mean actually know something, are entrepreneurial, have zeal, have passion, and also have some management capability—because these are managed efforts. Now, nothing ever follows a plan, because serendipity occurs, but having a plan is better than not. And knowing how to manage, and what a budget is, and what a schedule is, is not universal in our society or any society, so it's just a constant effort. There's no one place that you can go.

I: Is part of the role protecting people who are out on the fringe?

Fields: Absolutely. And that's true at absolutely every level. Office directors

protect program managers, and the Director tries to protect office directors and so on. It's one of the reasons the higher-level jobs are less fun, but it's true in many large organizations. There's nothing particularly unique about that at DARPA. You want the program managers to be able to do their work. That's the most important thing. Ask questions? Sure. Hold them back? No. That's why you hire them in the first place. You have to have a high tolerance for being embarrassed and being kept in the dark and being surprised. That's fine. That comes with some self-confidence.

I: What about the background during your Directorship? The background had changed. There was the collapse of the Soviet Union, and so the traditional perceived threat. How had the security issues changed when you took over?
Fields: I don't think that was fully worked out then, and, quite interestingly, it isn't fully worked out now. There is still a lot of uncertainty about just what we should be doing. Let's leave now for a moment and go back.

I think there was still a residue from the Soviet Union. Remember, the Soviet Union fell during that exact period. It wasn't so clear they weren't going to re-form. There was a lot of anxiety about the fact that they might re-form, and so I don't think there was as big a change as you might expect.

It took years afterwards before you could see some diversion, and now, of course, you *do* see diversion toward counter-terrorism, and toward rogue states, and so on. Things don't change as fast as you might think by reading the history books, because when you're there living it you might not perceive the change. So, I don't think there was anything I can really report about that.

I: Was there a sense of change of in Congressional budgeting, for example? Stories of peace dividends, for example?
Fields: Well, I don't remember any peace dividends. I always had a fine time with the Congress. They—and not only the Congressmen, but equally importantly their staff and, perhaps even more importantly, the Committee staff—always had a good relationship with the Agency. They knew what they were doing. We told them what we were doing. We didn't keep secrets. We had accountability. They accepted the notion of our having vague plans in exchange for pretty continuous delivery of great results. Those two trade off. And we, by and large, got the budgets we asked for. There weren't any serious problems with what I'll call constituent interests or pork, which people talk about. You know, there would be a few congressmen who would ask you to look at their constituents' activity and so on, but there was never much pressure.

Once in a great while, there would be some pressure, but then when you explained why you were pursuing the course you were pursuing, most Congressmen sort of got it. And once in a great, great while, you'd have to do something you'd otherwise prefer not to, but it's a tiny, tiny percent—quite different, I might add, than in other parts of the Defense Department, where it's a larger percent.

I think the reason for that is the respect the Agency gets, and I think the reason for that is the Agency had then, and has now, something like a strategic

plan. And when you explain how you have a jigsaw puzzle and the pieces, and you don't want to throw a piece way, you get a good hearing. So, I don't have any really interesting, unhappy stories about the Congress. It all worked out just fine.

I: Did any shifting of budgets occur within the Defense Department budget that might have impacted your customers?
Fields: Well, nothing all that radical. Affecting DARPA, some parts of the research apparatus, like MIMIC and the manufacturing programs that were outside DARPA, were moved into DARPA. I think the senior management within DOD had the belief that a DARPA style of management would help them facilitate those programs and probably did. But, you know, more or less the same work was done by more or less the same people, just with a little loosening of the self-imposed regulations; a little more focus on the outcomes and the output, on a sort of gung-ho, can-do attitude toward the whole matter. I don't think there was any big change.

I: The transfer of SDI in and out of DARPA -- were some elements of SDI brought back into DARPA at that time?
Fields: Not to my recollection. I could be wrong, of course, in that recollection, but I think it went off and has been out ever since. You know, that's become something of a political matter, and there are also really serious technical challenges, but I think that DARPA laid all the groundwork, as usual, and spawned a child.

I: Transitioning the technologies to the customer—how did that process work while you were there?
Fields: I think the process was about the same before I came and after I left, and to me, there are two, key elements in the transfer challenge. One is it's really good for DARPA to bring military folks in for two-, three-, four-, and five-year stints at DARPA. They bring expertise, but more importantly, they bring the technology back to their service in their heads. Nothing's more important than that.

Secondly, one thing DARPA does that many R&D organizations don't do is actually build prototypes that work. Building things that work and giving people the opportunity to use them is invaluable. There's no substitute. You can't explain it. You can't do a demo in an office. You have to get it out in the field and have someone use it and say, "By *God*! This is fabulous. It makes a big difference to me." And occasionally you get feedback that it doesn't, and that's good, too. So, being willing to build prototypes and get them out, and sometimes in quantity, and having the service personnel rotate in and out of DARPA are two, very important elements.

Now, to get back to that very crass subject of money—prototypes are expensive. If you're just an R&D organization, you develop the intellectual property. You develop the knowledge, the insight, the science, but you don't build a prototype; it's a lot cheaper, but, then, it won't get used. There are

exceptions, but just a few.

And so I think those are really the two keys to success in all of this, and, you know, I remember a conversation—I won't say with whom—early in my time at DARPA, when the ARPANET had a few nodes, and basically it worked just fine, and almost all the principles were done, and working more or less debugged. And he said, "Why don't we just stop the project right now? We're not developing any new knowledge."

And so I had to go through a laborious argument, explaining why we had, in effect, to spend money on sales, on marketing, on transfer, even though we weren't spending money on new technology. And I'm happy to say that argument was accepted, or we might very well not be doing our Googling today. Who knows?

I: I didn't know that it came that close to being nailed at that point in time.
Fields: Well, I'm not sure I want to say "that close," but it's worth understanding that it is an argument—how far do you take things? But if you don't take them far enough, it's as if you didn't do them.

I: My understanding is that you really tried to recruit Victor Reis to help with the relationships with the Services.
Fields: That wasn't my motive. I just thought Vic was and is just a really smart guy; lots of passions, lots of interests. Good communicator. Understands. I thought he was a great guy. There was no one reason. It includes that, but not uniquely.

I: He was reluctant at first to become Deputy, and I think you quoted Liszt: I want to "throw a javelin into the future."
Fields: That's exactly right. I am a failed pianist, and that's a quote from Liszt from his late years, when he was doing highly atonal music. It sounds like Schoenberg. And, yes, that is absolutely right.

I: What were you trying to tell him?
Fields: Well, I was trying to tell him, "We're doing things at DARPA that really matter and that will matter into the future." I'll give you an example. Nothing to do uniquely with Vic, but to show how something can work.

It's probably been years since you've heard a lot of debate and discussion about the field of artificial intelligence. DARPA invented the whole field and paid for it. What's happened is that something so initially controversial has now become so lacking in controversy that it's basically pervaded all of society. The techniques and approaches are no longer uniquely labeled as "artificial intelligence." They're simply in everything. I have kitchen appliances with software in them, computers that have what would've been labeled "artificial intelligence" 20 years ago.

That, to me, is "throwing a javelin into the future." It's leaving a legacy you can be really proud of, and I think every DARPA alumni is proud of something they've done, and maybe many things they've done. So, that was what I was

trying to communicate—that you can make a difference.

Partly, this is a question of age, I might add. My wife and I are about the same age, and we had a conversation some months ago where we asked each other, "What year did you like the best?" Now, of course you can't find a single year, but completely independently, we both named the '60s and the '90s—the '60s because everybody thought anything was possible, and the watch-word was "make a difference;" and the 90's basically because of the Internet explosion. Everything was changing, and we all thought we were making a difference.

I think that still pervades DARPA.

I: The idea of artificial intelligence. That interests me because it sort of came and went through DARPA because the ideas were ahead of the technologies.
Fields: And some still are. But it's something that will never succeed and yet has already succeeded. We're no longer debating about whether you can produce a piece of software that acts more intelligently than people, because the answer is that with some things, the answer is clearly yes, and with other things, not yet. It's in software everywhere, and yet we're not labeling it "artificial intelligence," so it gets neither credit nor blame. I think it's a fabulous success without credit.

I: When you look at the menu, there's a turnover in personnel. I'm going to guess there has to be a similar turnover in projects to make room for other projects. So, when you look at the menu of people and projects, what kind of tool did you use to make those decisions?
Fields: Well, let me comment on one of your premises first, which is correct—but it's so important, and it's worth underlining—and then answer your question, if I may.

It's really important to have turnover of projects, to have nothing immortal. My slogan about this is that at the end of a three-, four-, or five-year project, , whatever you said it was going to last at the beginning, if you want to continue, you have to prove that the next three, four, or five years will make as big a difference as the last three, four, or five years. As things become incremental, as you well know, they plateau, and that's not DARPA's style. I mean that's just the wrong thing to do. So, that's an important thing.

As for the question you asked—how do you decide what to do next? There's principle and there's practice. Let's deal with practice and then principle.

Practice: Is there a program manager around who wants to do it? If not, it's not going to happen. Are there contractors around who have the capability of doing it and also have the passion? If not, it's not going to happen. Approval within DOD, or financing from the Congress are much lesser concerns regarding whether it will happen or not happen. So, they're very pragmatic issues, and, of course, the state of science and technology has to make it possible. You want to try things that are 10 percent too hard, but if you try something that's a 1,000 percent too hard, you just get frustrated. Ten percent too hard, you're building muscle, and so, you know, there are some pragmatic concerns.

Principles were approached, at least by me, in the following way. Early in

DARPA, in the early'70s, one of the programs I was involved in focused on quantitative decision analysis. This is a whole set of techniques and tricks Bayesian probability theories applied for creating models in which you make assessments and express assumptions explicitly and try to draw implications. The process of doing it is almost more important than the answer, because you bring things onto the surface, and people can argue and debate them, and so on. And I decided when I was Director, as an exercise, I would try to create a decision model for what DARPA should do that would go back to first principles—namely, what in priority order are our national interests.

Well, you can't just ask anyone, "What are our national interests in priority order?" and actually get an answer. That's not a normal thing, so I decided it was probably harmless for me as a poor technologist to simply state my opinion, and if anyone disagreed with it, they could simply change it. I mean, it's a lot easier to get folks to edit things than to create things in the first place. And so I created a quantitative model that had national interests in priority order, and the Number One thing on that list was keeping the United States from being the object of a nuclear attack, and then it went down from there.

That national interest is Number One was underscored by Steve Lukasik, who once quipped that if DOD couldn't do that, who needs DOD? That's really Number One. Steve's a very bright man. And I then went to say "Well, what systems do we need to accomplish that? Which ones will happen anyway? Which ones won't? If they won't is it because of technology or something else? If it's technology, which technologies are going to happen anyway? Because industry's already investing in them, and there's no reason for DARPA to use DOD's money. If not, then maybe those are good fodder for DARPA, if there's an idea. Having a desire without an idea is insufficient. You have to have a desire, but if you don't have a shred of an idea, what are you going to do? You waste money."

And so you go through a little decision analysis like that, and you can pretty quickly come out with a prioritized list of what kind of technologies you should invest in and then go ahead and do so. You don't want to take any of this too slavishly, because that's silly. It's pedantic. What you want is to get some insights, and so, for example, one of the insights that came from that exercise in 1989, was that industry's investment in information technology was rising so fast and was so large, that, unlike 1969 and 1979, maybe DARPA needed to spend a little more money on materials technology. And it could afford to depend on some commercial COTS technology, which, with hindsight, I think, was exactly the right decision.

So we actually boosted our investment in materials technology, because that was being under-invested in by industry, and it was key to being able to make the systems to accomplish some missions for DOD.

This kind of analysis was really sort of an eye opener, and a useful one, and it was a guideline at the end of the principle-end of the spectrum for what you should do, but nothing overcomes practice. If you don't have a person to do it, it doesn't happen. So, that's the trade. Theoretical thinking/strategic thinking versus what can you actually accomplish?

I: Did people come and go relative to the projects that were shed, or the projects that were added?
Fields: By and large that's true. It's not all that easy to find a program manager to take over someone else's program. It happens, but on average, a program manager and a program are linked. Sort of like love for life.

I: That's got to be one of the hardest things to do as a manager—take over something else, or to see your own project go away.
Fields: Absolutely.

I: You mentioned support of materials sciences—what kinds of projects?
Fields: Well, just to give you an example, batteries. If you go to Iraq today—and I don't urge that you do so—and talk to any of the Marines, or Army soldiers who are there, ask them how many pounds of batteries they're carrying around; it's a large number. I don't want to say what the number is, but it's a very large number. If they could carry fewer batteries, they would be better off—the soldiers, the Marines. They could have better communications. They could do better computing. They could carry more water. There are lots of advantages to having better batteries, and the challenge of batteries is, in part a fabrication issue, but in part it's a materials issue. That's an example of what I mean.

I: Electronic processing? Materials?
Fields: Oh, yes, there are many. I just gave you one example. DARPA's had a long-term investment in chip making, wafer fabrication, and the design tools to go along with it. Much of the information-technology infrastructure in this country came out of DARPA. I use the word "much," because it's a gross exaggeration to say "most" or "all," but "much" is not a gross exaggeration.

I: When you looked into the future from that in time, what did you see? What kind of things did you anticipate?
Fields: Well, let me answer the broader question. By and large, DARPA proposes things rather than receives orders. Depending on personalities, there are good relations with the DDR&E, good relations with the Undersecretary, with the Deputy Secretary, the Secretary, and with the Congress; and once in a while, DARPA does get guidance in a literal sense. But by and large, DARPA is a sort of self-organizing system that comes up with ideas and proposes doing them. There's complete openness, transparency, accountability, and responsibility. None of those are issues.

But in terms of where does the impetus come from, it's almost always from DARPA and rarely from the outside. Not like, "Here are the three things you will do."

After I left DARPA, I worked in industry for a number of years, and I was talking to the CEO of an extraordinarily large manufacturing company, and he was describing their process wherein the marketing department would give instructions to the head of R&D about what they needed. They need a battery

with these characteristics, a lens for a camcorder with these characteristics—"Figure out how to make it." That just doesn't happen with DARPA.

It was sort of self-initiated. What you want at DARPA are people who are self-starters, if that's a phrase, who are very good at identifying and solving highly unstructured problems. This is not a question of, you know, "Here's the problem. Find a solution," but figure out what the problem is in the first place. It goes back to the difficulty of recruiting personnel. Not everybody can do that.

I: DARPA's role in the chain of command sort of shifted throughout. Where was it during that period?
Fields: I haven't the foggiest idea, and I don't know what it is today. There is paperwork that says what the chain is, and then there's the reality of daily life. The reality of daily life depends on personal relations and personalities. I mean I had, I believe, a great relationship with Charlie Herzfeld. Charles is brilliant. I like brilliant people. He was a pleasure to deal with. We never had anything but good communication, and we still do.

At some times in the past, DARPA's Directors have had extremely tight relationships with the Secretary. It depends on the Secretary. It depends on the time. So, I think that the "chain of command," as you're describing it, is legally in force and in place, but practically, not all that relevant. It's the people that count—and maybe that's the way it should be.

I: Dr. Herzfeld believes DARPA should be pursuing the "big idea," the presidential issue, the big geo-policy issues. Was that the case during that period in time?
Fields: I think it's been the case all the time. If something is small enough that someone else can and will do it, then someone else can and will do it. There are always a sufficient number of unsolved, giant problems for DARPA to worry about. I never felt, and I don't know anyone else who ever felt, that they had to hold onto a problem as their territory. If someone else wanted to do it, let them have it. That's perfectly fine. Again, this goes back to the self-confidence issues.

Why would you go to work there? You go to work there to accomplish big things and have the satisfaction of success, or at least to know you tried. So, I'm exactly in line with Charles.

I: I meant to ask this earlier: did DARPA use the contracting agents?
Fields: Largely. It had its own contracting office as well, and it also had this other transactions authority part way through my tenure there. But by and large, it used contracting agents. Contracting agents in the Services are an integral part of the transfer process. I don't think they're as important as the two other issues I raised earlier—the rotation of military personnel in and out of DARPA proper; and the construction, distribution and fielding of prototypes—but they play a role in that as well. There's always been a debate, "Should we contract ourselves?" ("Ourselves" being DARPA.) "It'll be faster," and that's probably true. But in the end, you give up that relationship with the Services, with your customer, and I don't think anyone has thought that was a good trade. I don't.

I: You mentioned doing things faster. It that part of the key to DARPA's success—its agility, its ability to respond quickly?
Fields: Well, speed is really important. People's interests are perishable. Technology ideas are perishable. Many adversaries are faster than we are. Certainly today that's the case, and it's a scary thing to contemplate, and so speed of the entire process is important.

I always wanted—and I'm not sure I succeeded—to have the least paperwork within DARPA, the fewest signatures. The more people who have to sign, the more likely it is that someone will say, "No". And do you take a risk? Yes. But how often is that risk actually realized in terms of a bad outcome? Very infrequently. So, I think agility is quite important. DARPA had then and still has an internal contracting organization that basically does the work. I was always happy with their performance, but seeking more. A difference between being pleased and being satisfied. I was always pleased, but never satisfied, because if they held up something, program managers were held up. And they didn't. They felt they were part of the team. That was an important thing.

I: Were there pressures on the organization during that period of time to become bureaucracies?
Fields: Of course.

I: To become too cumbersome?
Fields: Of course.

I: How do you fight that?
Fields: Just refuse.

I: Where did the pressures come from?
Fields: Everywhere. But always, I've always found, and I think that every other Director's found, that there were just enough allies to avoid these problems. I mean enough people now realize that DARPA works well the way it is, and it isn't going to get better with more paperwork; that you can avoid the problems. And so, I don't think, you know, it's a constant threat, but DARPA's never been politicized, unlike some other parts of the national-security community, where you have literally politically oriented appointments. Tony's not a politically oriented appointment, and no one else has ever been. And, you know, basically there are few things that the world leaves alone to just succeed, and DARPA's been one of them. I hope that continues.

I: What was one of your biggest challenges?
Fields: Well, I mentioned earlier, and I'll repeat it because it's the biggest challenge: recruitment. There's simply nothing more important and nothing more difficult than recruiting the personnel. And as I said, I can't even think of what Number Two might be. Dealing with Congress, the Services, with technology transfer—nothing was as important as finding the right people.

I: Were there any surprises that popped up?
Fields: Well, technology was constantly surprising. The serendipitous applications were constantly surprising, but I wouldn't say there were any surprises other than that.

I: Let's take a look into the future. What do you think are the keys to maintaining DARPA into the future?
Fields: Well, DARPA's culture is pretty resilient. No two Directors are alike. I'm expressing, you know, my views, which are absolutely certain to be different than everyone else's—but there's a center of gravity. We want to keep Directors who are about the same. You don't want to have radical swings. You want to keep the organization working about as it does within the Defense Department and within the federal government. And so I think that as long as the track record is realized and recognized, things will just keep going. So, that's a structural answer. I'm not worried so much about the structure.

The fact is that the country, and it turns out, in consequence, DARPA, has some very, very tough technical challenges now and in the future because the threats are so different. I mean nuclear proliferation, biological warfare, chemical warfare, radiological attacks, cyber attacks, EMP—these are all on top of the "traditional" problems. High-speed torpedoes, diesel submarines—there are any number of things that are of the traditional sort. And then you deal with cases of things that are not quite war, like insurgency and occupation in Iraq today, which we're really not quite ready for. So, there are some very large problems being faced by the country that are in addition to the old-fashioned problems and, hence, being faced by DARPA.

Coming up with technical solutions is not a slam dunk. DARPA, most people don't know, has probably the most advanced biological warfare program for countering biological warfare in the world. They're doing brilliant work, but that doesn't mean it's working. It's partly working. It's on the way to working, but it might not work. Some might work, some might not. It's a very big threat. It's a big issue. And I can go through a long list of these things, but I won't.

And so I think the substance is really my concern. It's not the form. Form is fine, but the substance is very hard—harder than it's ever been, actually.

I: Does DARPA have a role to play, for example, in the Global War on Terrorism?
Fields: Of course, and it's playing that role today. It's doing some of the most innovative work around. It will continue to do that. A lot of it, actually, is challenging because technology may not be the answer. In some cases it will be. In some cases it won't be. A key to counterterrorism is great intelligence, and when the adversary doesn't use electronics, isn't quite visible, technology may not be all that easy to apply. So, you know, DARPA can play its role, but it can't do everything.

On the other hand, there's plenty it can do, as I started to say, with biological warfare, nuclear detectors, radiation detectors. There are all sorts of

things.

I: I read somewhere that one of the DARPA Director's roles is to get the intelligence community to help figure out where the threats might arise, what might be going on, so that it can come up with the technologies to solve those questions. How do we do that with such almost impossible intelligence?
Fields: Well, let me address a different question and then, slightly, your question. DARPA's always had a really good relationship with the intelligence community, and it's of two sorts. You mentioned one sort, namely learning from the intelligence community what are the challenges, what are the problems. The other is DARPA's always had very good relations with its opposite numbers in R&D in the intelligence community because they're really smart and so they can work together and get something done. That's a very valuable set of relationships that persist to this day.

As to what can you do? Well, I don't want to veer off into the intelligence community. I can't talk about that.

I: Some of these perceived threats that might be coming down the road—you had some listed in your speech about the fortieth anniversary. I'd like to revisit a couple that were very intriguing to me. The environmental threat intrigued me because of its biblical/historical nature almost. Tell me about that.
Fields: What struck me, and the reason I wrote about it was that we don't talk and think about it much anymore. As you point out, it's a biblical threat. This has been going on for thousands of years. People try to deny water to others or poison water supplies, or they want to create draught. There's nothing new about that kind of thinking. It's harsh, but there's nothing new about it. And, yet, we don't really worry about it all that much. I don't know that we have groups sitting around and thinking, and this goes back to DARPA's role in avoiding surprise, how would an adversary do that? What could they contribute to causing us problems, or causing our allies problems? I'm equally attuned to the fact that causing our allies problems is causing us problems, because so much of what you need to accomplish in the world nowadays requires a coalition—not just the U.S. acting alone and unilaterally.

So, I can't say that I have a specific about that particular topic vice any others like refugees, and immigration, or the drug problem. I think they're all things that could be inflamed by foreign powers that wanted to be more subtle than an all-out war. I'm a member of the Defense Science Board, and we are doing a pretty large study at the moment, looking at future war and saying, "What happens if a future war involves nontraditional threats of EMP, cyber attack, bio, chemical, radiological, and nuclear blackmail?" And that also includes attacks on our domestic military infrastructure, for example, or attacking bases to impede deployment. People don't worry about that quite as much. What happens when there are attacks on civilians in wartime and, as always, the DOD is called in to help local authorities because we have the greatest capacity, and that distracts from fighting abroad in a war? I'm thinking of this as an instrument of war by an adversary.

When you complicate the picture with these possibilities, acknowledging that all of them won't happen, but our intelligence isn't good enough to tell us which ones will happen and which won't, you get a pretty scary picture of the future.

So, I think there's a lot for DARPA to do, and it's actually doing a lot.

I: You raised a whole bunch of images in my mind just now: the anthrax scare, the "Attack of the Killer Tomatoes," which I really love because if kids were doing gene splicing in their basement 10-15 years ago, who's doing what now? You have genetically altered rice invading other species of rice. Am I off-base in thinking that this is scary?

Fields: No, I think you're absolutely right. As has been true in the past, I think you have to put nuclear weapons at the top of the list. We—where "we" is now the Defense Science Board—did a study on weapons of mass destruction a couple of years ago that was a nice, quantitative study. And while you can create assumptions that'll lead to any possible outcome, we tried to be reasonable in our thinking. And if you look at the effects of nuclear weapons compared to any of these other exotic technologies, there are still more zeroes at the end, by far. So, it's still true that there's nothing more important than avoiding a nuclear attack.

And yet, look at the ways to do that. We worried for a long time about ICBMs, but what about the private plane? What about smuggling by ship? We have porous borders. We'll continue to have porous borders unless we give up civil liberties. We don't want to do that, and I agree. So, I think there's a lot to worry about. I'm not trying to be dismal in this conversation, but you raised the subject.

I: We're talking about the threat of anthrax, or of contaminated peanut butter or Brussels sprouts, or whatever—the impact that it would make on a society.

Fields: Well, as we all know, there was a huge amount of terror from a tiny number of deaths. And I don't mean to be crass and minimize inquest, because any deaths are too many, but look at the effect. Look at the effect in the Washington, D.C. area of those two snipers. Hardly anybody was killed. As a percent, it's insignificant, although, of course, it's deplorable; but such a tiny amount can cause such a big change. And so I think there are vulnerabilities in our society that are going to persist. They're intrinsic in the way we are, and it would be foolhardy to think that an adversary doesn't understand those vulnerabilities and won't exploit them.

So, these are challenges that are partly technical and partly not, and they're certainly things DARPA's worried about right now.

I: That's the essence of terrorism.

Fields: Exactly. Terrorism is a technique. It can be used by governments, transnational groups, whatever. Starting in 1995, the Defense Science Board began a series of studies that looked at transnational groups, weapons of mass destruction and biological warfare, and it was a good set of work, and it all

paralleled DARPA programs. There was a lot of cross-talk back and forth between this advisory board's work and DARPA's work which meant that when 9-11 occurred there was a body of research there on the shelf to be used, both in outside studies and DARPA projects.

You know, I started DARPA's biological technology program. I hired Ira Skurnick—a brilliant man—decades ago, because I thought we would need it. And, you know, it grew over time and evolved and changed direction because that's what should happen. That's what DARPA's there for—to get out ahead and have something ready when you need it.

I: Was there any trouble trying to convince people of the importance of starting up biological defense systems?
Fields: Of course. When something has never happened, when there's little intelligence about it because, you know, while you can take a picture of a submarine, it's pretty hard to take a picture of what's in a test tube from afar, and—it's hard. And it's—you know, typically in these discussions, you start off with denial and quickly go to despair; the denial part being, "It hasn't happened, and I'm not sure it will, so it won't," and then the despair part being, "Oh, yes, it will happen, but I can't do anything about it because it's so horrible."

Actually, neither is true. I mean there are things to do, and even though it hasn't happened yet, by and large it's pretty likely it will so let's get prepared. But of course there is a problem. I mean it's hard to convince people to be visionaries.

I: You mentioned that nuclear attack is still Number One on the list.
Fields: It's my Number One.

I: Antiballistic missile systems and BMDs have sort of come and gone. Where are they now? I mean, what is the thinking with antiballistic missile defense systems?
Fields: Well, I think you want to separate two things. One is ballistic missile defense to protect the United States, and ballistic missile defense in theater to protect our forces, or our allies and so on. One of those is technically a lot easier than the other, although both are really hard. So, when you say "come and go," if you're dealing with budgets and popularity, that's an accurate statement. If you're dealing with actually having it, I think "go" is more accurate than "come." It's just very, very hard.

Then let's add on top of it the fact that the goal here is to avoid being the object of a successful nuclear attack—not ballistic missile defense—and you have to look at all the other ways that you can deliver nuclear weapons. Unfortunately, there are a lot of them, and in our free society it's hard to close off all those channels. It's a lot better to keep people from getting them in the first place. Once they have them—I won't say it's too late, but it's edging toward too late.

Again, DARPA's working all these issues as we speak.

I: Space war?
Fields: There's an attitude many people have—I guess I'd have to say most people have—that space should be free of violence. And the word "should" always catches my attention. I would just as soon Earth, in general, be free of violence. It *should* be, but, you know, it's just another altitude; it's just higher up, and if enough people think it isn't going to be free of violence, then it's not. So, realistically, the country has to have the technologies, and DARPA is by far the most innovative in developing them, so that if violence comes to space, we can hold our own and do well. It's unrealistic, otherwise.

The argument against that, of course, is if you're ready, maybe that makes it happen. The Chinese anti-satellite activity was not a great surprise to me. It *should* happen. Someone, sometime, is sure to believe that the word "should" doesn't apply to them. That's the case here.

I: Are there any big DARPA problems you see on the horizon that'll be particularly challenging?
Fields: Well, I've named so many of them already, I can't really put them in a priority list and say, "This is the most difficult." Certainly, the most imperative is the nuclear proliferation problem, but all of these nontraditional means are challenges. What's happened, ironically, is that technology has made it possible for lesser powers, including actors who aren't states, to be really troublesome. The Internet is their command-and-control system, assuming they use it. That gives them vulnerabilities, I might add, because they have to depend on it, but it works pretty well. We built it. FedEx is their force-deployment system. Works pretty well.

Technology just changes the rules of the game, and it's a constant race. I mean we develop technology. Someone else does something else. We develop more. Someone else does something else. That's been going on for thousands of years. There's no reason to believe it isn't going to continue. That's why this issue of agility is so important. There's an expression that George Heilmeier sometimes uses of getting "within the turning radius"—this is like a fighter plane—of the adversary, and so we just have to be faster, turn faster, be more agile. Everything we do, we should be looking at the countermeasure right from the beginning, and sometimes we do.

I: The idea of the threat from Radio Shack technology—is there a tendency to overshoot with high technology, and forget for a moment the low technologies?
Fields: Well, both can be troublesome. There's not a lot of high technology in the IEDs in Iraq, but they're certainly causing a lot of grief. You know, we look at the number of deaths, but the number of maimed and wounded is vastly greater. The number of young men and women whose lives are changed for the worse forever is a *huge* number, and that's from very low technology. And then our dependence on high technology and our information-technology- centered way of approaching war makes us more vulnerable to countries that aren't.

So, what do you do? Well, do you stop using information technology? No. Do you try to make it less vulnerable? Yes. Will you fully succeed? No.

That's the constant challenge and the constant debate.

I: One of my favorite stories is the Silly String story. That was the most popular Christmas gift to send to the G.I.s in Iraq because they could spray it into the homes, and it would hang over the tripwires.
Fields: I'm sorry. I didn't know what Silly String was.

I: Low-tech.
Fields: Yes, right. Absolutely. And this actually goes back to a comment I made earlier about DARPA wanting to do high-impact projects, not high-risk, long-term, expensive projects, although most are long-term, high-risk and expensive. If there's a way to do something that's low-tech but requires innovative, creative thinking - that's a DARPA activity.

I: Are there new applications of existing technologies that are seen in bright, new ways?
Fields: There are. I mean, I actually know a number of current examples, but, you know, I can't really talk about them.

I mean nothing substitutes for smart people, and giving smart people challenging problems is important. There was a '60s, early-'70s theory of how to do R&D. What was the theory? Build a beautiful building, put it on a mountain, or next to the ocean, or near a lake or something. Populate it with the smartest people you can find. Give them all the money they want, and say, "Do great things."

Well, it turned out to be not so efficient. A much better approach—I'll call it the DARPA approach, but others emulate it—is to have small teams of people, not just individual people. Give them some hard problems to work on. Give them some timescale challenges, maybe even some financial challenges. The artistry is making it not too hard because then nothing occurs, and then something really great can happen. "Necessity is the mother of invention."

And so I'm very much a fan of anybody's activities, and particularly DARPA's, which center on small teams of smart people who are given a tough challenge and told, "Go to it. Find a solution, no matter what." Amazing things happen when you do that, but you have to start with the smart people.

I: Were there projects started under your Directorship that you're particularly proud of?
Fields: There are two reasons I can't quite answer that question. One is that I sort of integrate my whole time at DARPA, so it's not a pride of this calendar year versus another calendar year—I don't think that's quite what you meant. And the other is, there are so many projects DARPA's done, and I'm proud of the *agency* doing it. You know, the moment I think of a project, I think of all the people who worked on it, so I can't take so much personal pride as personal satisfaction. There are different kinds of appreciation in the words "satisfaction" and "pride"

But all the work on training is terrific. Most people don't appreciate that one of the reasons our military is the best in the world isn't the fancy equipment

we have, and it isn't that we're big and rich—those contribute—but it's that we have the best-trained soldiers on Earth, by a *wide* margin. Well, everything DARPA did in training is sort of underappreciated, and that's just fine, because it's had fabulous results. But I'm very pleased and satisfied that DARPA did that, creating this whole information technology infrastructure for the Defense Department, and—oh, by the way—as a spin-off, for the rest of the world. How could you not be proud of that?

Materials science—you know—we talked about that earlier. You like your composite golf clubs? Thank DARPA. A huge amount has come out of that, and, of course, the military applications are legion.

And, solid-state fuel for rockets—I mean the list is endless. Some of these happened before I came. Some happened after I left. Some overlapped. Several while I was there. The timescale really doesn't matter. This is just the greatest innovation engine that's ever been, and so everybody associated with it wants to protect it.

I: Conversely, were there any projects you sort of look back and think, "Gosh, I could've pursued that"?

Fields: Well, a little different than "I wish we could've pursued it" is, "I wish it would have worked out better." That's a little different caste. We did a project called the National Aerospace Plane that sort of petered out. It never quite achieved its goals. There were a lot of spin-offs and so on, but it didn't quite make it.

Sometimes projects don't quite make it because they seem silly. I'll give you an excellent example of that. In the, 1970s—I think it was, but who knows—Ian Sutherland, a brilliant man, walked into my office in DARPA and said, "I want to build vehicles that have legs instead of wheels or treads." And he gave me excellent reasons why they should be better vehicles. And I inferred that he expected to be thrown out, but he wasn't. I thought it was a great idea, and we went ahead and developed a lot of technology and a lot of prototypes. You see these around today. But it never quite made it because the silly factor was just too great.

On the other hand, we have adaptive suspensions in automobiles that go right back to that work and you don't even think of it as a DARPA project. So, did DOD get quite what it needed? Well, not quite what we envisioned, but those adaptive suspensions in military vehicles, those adaptive seats in military vehicles—I don't know if you appreciate that one of the limiting factors in speed of travel by a land vehicle is that it jostles the driver so much. You have to actually protect the driver. So, all that technology just sort of got simplified and got changed and, you know, sort of leaked out, and went here and there. Never quite gelled. I look back and say, "I could have a much better vehicle with legs than wheels, but it's just too silly."

So, these things happen.

I: Isn't it back as Big Dog?

Fields: It's all around. The amount of work that continues to go on—and it

sounds much better to say "adaptive suspensions" than say "legs." It's university research. It's some industrial research, but if I buy a new car, it's going to have an adaptive suspension, and I know where it came from.

I: I think, under Dr. Heilmeier, DARPA really pushed off into the prototype area. Were there new manufacturing techniques that had to be invented?
Fields: Sometimes. Some programs were manufacturing in their own right, and some didn't look like manufacturing, but really were. DARPA had a program at one time called MOSIS, working on how to produce very small quantities of chips with very short latency. It was a design project, in effect. Before then, if you, as an individual, wanted to get some semiconductors made—no problem—you'd pay someone to do a design, send them off, get a million made, and you'd use one. Well, that's not good. And for some of our applications, you only want a small number, and you want to get them fast. So, DARPA did all the work. Done.

Another thing that was manufacturing, but of a completely different character, was SEMATECH. DARPA was the funder of SEMATECH. There was an example where a set of semiconductor manufacturers wanted to band together and cost-share with the government 50-50 to develop new process technology for semiconductor manufacturing. It got off to the usual slow, confused start; then got into its grove and did great. It did a lot of work. So, manufacturing is as legitimate a technology as anything else, and DARPA's done a lot of work and continues to do a lot of work.

I: Talking with Dr. Cooper, there was this front-wing aircraft, and I can't remember which one it was, but he knew that it wasn't going to go anywhere, because he knew that air battles had gone beyond what it needed to do, but he continued the project because he felt a need for developing the manufacturing techniques. Were there projects like that you saw coming around?
Fields: Well, there aren't any projects in my personal experience that I pursued because of the spin-off rather than the project. We had plenty of spin-offs, but it was the project that counted. I consider it legitimate, but I have no personal examples of that sort.

I: Projects that were continued because—oh, that was the area and this will get to that and projects that were continued because the science was good?
Fields: Oh, there are plenty of projects where the science is good, and they have been continued because the science is good, but that's a little different in character. You know, we do have this thing called the National Science Foundation, and despite anyone's concerns, they really do quite a good job. So, you know, trying to do their job has always struck me as not quite right since DARPA has more than it can do on its own job.

But, sure. Are there projects that go on and that create good science, and sometimes you can't resist doing more? Fine. DARPA is an umbrella that's pretty large. You can do a lot there, and it works out just fine as long as there's enough of the central sort. These great challenges like Charles Herzfeld talked

to you about.

I: How does DARPA walk the line between basic and applied sciences?
Fields: Well, I think that the only people who worry about it are those who actually aren't doing the work. You can't draw a line between the two. You don't say that one comes before the other. Today, we still don't have an absolutely perfect theory of ice, and yet we all enjoy ice cream. Technology oftentimes precedes science. I don't think that's a topic. That's just—you know, for what I'll call the real practitioners, that's not a topic. That's a Ph.D. thesis for someone somewhere.

I: Or a bean counter topic.
Fields: Well, it's not even there. I mean, the people I used to deal with—and I hope the people that Tony Tether now deals with—are smart bean counters. They know better than to even raise that question.

I: The tumultuous stint towards the end of your tenure. What happened?
Fields: Well, I think you should check the public record.

I: Okay. That's fair enough. Is there anything else that you would like to talk about?
Fields: I can't actually think of anything in particular. We've covered a lot of ground.

I: Revisiting the decision-making process about how you determined which projects go where—can we talk a little bit more about that?
Fields: Sure. There are a lot of factors that go into it. You need a passionate program manager and you need to have an idea. One of the most common circumstances that arise is someone who really wants to solve a problem, but doesn't have an idea. When they don't have an idea, they come up with all sorts of ways to skirt around that lack, like, we should set up a "center of excellence," or we should build a "test bed." Those are clues that you don't have an idea.

Having an idea doesn't mean you've solved the problem. It means that there's a thin possible thread between an insight that could be completely wrong and a solution later, but it's somewhere to start. So, having a passionate program manager and having an idea are almost sufficient conditions for starting a program. You need money. You need to have a plan. You need to have contractors who are ready, willing, and able to do the work, but the first two are the big ones. And lacking either one, you can't proceed, and you can't believe how often you lack one or the other, or both.

I can't give you examples on the spot. I wish I could, but I know there've been any number of cases in my career at DARPA where there was a really interesting idea to do something, but there was no one to do it. There wasn't the program manager around to do the work, and it just never happened. Too bad. Things fall through the cracks, and, you know, who knows what would have happened?

I: The DARPA impact—anything from GPS to the Internet, the mouse, the test ban treaty verification; tell a little bit about the sense of participating in history.
Fields: I'm thrilled every day. I mean, every day I encounter something I'm enjoying, or someone else is benefiting from that DARPA did, and I know DARPA did it. Sometimes it was when I was there, sometimes not. Doesn't matter. Usually DARPA gets no credit. It doesn't matter. The sense—to use your phrase—of "participating in history," of making a contribution, is so strong, and that goes back to the age issue—it was 1960s culture. And every time I'm in a country café in Texas and someone's sitting there talking about the Internet while wearing a cowboy hat and boots, I think, "Wow."

So, it's tremendous—absolutely tremendous. I can't exaggerate how satisfying it is to have made those contributions, been with people who made those contributions, been in an organization that made those contributions. And that satisfaction is why DARPA continues to be able to recruit people.

I: Your history at DARPA—you were there during almost a geometrical growth in knowledge, application. Do you see that continuing?
Fields: Well, no, I don't. And the reason I don't is because it's pretty hard to maintain DARPA and make it too big. So, while in the world there is continuing to be—if you want to call it geometric growth, fine. DARPA can't get to be too big and still be DARPA. And I think every Director appreciates that.

So, the DARPA budget had grown a lot before I was there and then afterwards, but it's more or less at a plateau at the moment, in concept. If you start adding a lot more, you're just not going to be able to keep up the entrepreneurship, nor the excellence of program managers and so on. So, I think there's a self-limiting factor in order to achieve that excellence.

You know, after leaving DARPA, I've worked with lots of organizations and you get a statistical phenomenon of regression to the mean. The bigger the organization, the more it's like the mean. Well, I don't want DARPA to go that way, and no one else does either. So, I think that's the limiting factor.

I: Is there a chance that DARPA will be outpaced by other R&D organizations?
Fields: That's not high on my list of worries.

I: Terrific.
Fields: Terrific.

I: Thank you, Craig. Dr. Fields, thank you so much.
Fields: My pleasure. Thank you.