



**Dr. Charles Herzfeld
1965-1967**

Interview: February 23, 2007

Interviewer: Please give us your name and tenure at DARPA.

Herzfeld: I'm Charles Herzfeld. I was at DARPA from 1961 to 1967. I was Director from 1965 to 1967. For the first two years, I ran the DEFENDER program—the ballistic missile defense program. Then for two years, I was Deputy Director of DARPA, and then the last two years I was Director—and the fifth Director of DARPA.

I: Tell me about how you decided to join DARPA.

Herzfeld: Yes. Good. The Berlin Wall is very important in this.

The time is May 1961. I'm at the Bureau of Standards. I'm sitting in my office. I'm chief of the Heat Division, one of ten divisions at the Bureau. Very important job. I was the first of a young crowd of new division chiefs, and I had a ball. It was very exciting, very demanding and so on. Secretary comes in and says, "Dr. Ruina from the Pentagon is on the phone. Will you speak to him?"

I said, "Sure."

On comes Jack Ruina. Introduces himself. Says, "I'm Director of ARPA, and I'd like to tell you about ARPA. Can you come over sometime and visit?"

I said, "Sure. I'd love to," without thinking more about it.

As a footnote here, I did some work at the Bureau, some scientific research with DARPA money, and didn't really know that clearly because when we got that money ARPA was just forming and so on. That's another story, really, worth pursuing some other time.

And so I went to see Ruina at ARPA, in the Pentagon, and spent two days there. I went there for one day and then came back, it was so damned interesting. And they laid out for me what they were doing and the different kinds of projects and the DEFENDER program, the Vietnam program, nuclear test

detection program. And then at the end, Ruina says, "I'd like you to come to the Pentagon."

"To do what?" I asked him.

Ruina said, "To run the DEFENDER program."

I said, "I have to think about it."

And I thought about it for two days and called him back, said, "No, I can't. I'm enjoying the job at the Bureau too much."

He said, "Okay."

Then I went to Europe for vacation and also to take some temperature standards to other standards labs and in August I was in Paris. I visited several standards labs there and my wife and I went to see our ambassador, General Gavin, who happened to be a personal friend of ours, and he said, "If you're in Paris, come see me." So, we went to see him at the embassy.

And he was late. He was two hours late and his assistant came and said, "He called. He really wants you to stay and wait for him. He really wants to see you." So, we stayed.

So, he comes in after being over two hours late—really terribly excited. He said, "Just come from President (De Gaul)—spent three hours with him. There's a huge crisis brewing. The East Germans have put up some kind of a wall across Berlin—the East and Western part—and we don't really know what's going on, but there's a huge international crisis."

So, at the end of the day, it was clear that the Russians were helping the East Germans and this was really big.

On the trip back to the States, I thought about it and decided, "This is war. This is different than it was before. This is serious." So, when we got back to Washington, I called Ruina and said, "Jack, is that offer still open?"

He said, "Yes, it is."

I said, "How soon do you want me to come?"

Well, two weeks later, I was at DARPA. That's how it happened, with the Wall. And sure enough, when the Wall came down a lot of things changed, and that war was over.

I: When you walked into ARPA, what was the organization like?

Herzfeld: A lot of people doing many things, not terribly well organized, but it was very early. It was early in the day. It really got its big start—the new ARPA—when Jack Ruina was director, and he had just taken over. He began, I think, in early '61. He hadn't quite gotten it in shape yet. He was recruiting leadership for the organization and trying to make sense out of what it was that needed to be done. So, it was all very much wrapped up around the idea that we were supposed to work on very important, what we came to call "presidential" problems in the sense the President, Kennedy at the time, but earlier, of course, Eisenhower, really were interested in, and to fix the problem.

So, how do you do that with these huge problems that we were handed? It's complicated. Not enough people. There was plenty of money. There was enough to do serious things but we had to staff up. We had to think

through what needed to be done—big job.

I: What were some of those big issues?

Herzfeld: Well, the first thing that ARPA was set up to do was to take on the space program—the whole space program of the U.S. government and fix it. It was in shambles. There were many uncoordinated, competing programs that didn't make much sense.

But very quickly the first Director, Roy Johnson, with the guidance of Eisenhower, started NASA, with all the civilian space stuff, which was a good bit. And another part went to the Air Force and on space, early warning mostly. And an intelligence community part that went to the intelligence community, overhead satellites.

And ARPA, when Johnson was President, funded all these early things that were started over Roy Johnson's signature. And the ARPA Director's signature is a powerful and wondrous thing. It gets stuff done. And so it was. And so space had settled down and was still an occupation for ARPA, but not the main one.

A new project that was started roughly at the right time was the Ballistic Missile Defense Program and that was what I wound up with and it was really a huge program, poorly organized, and poorly thought through. It started trying to do everything and some were sensible things. Some were less so and the main task was to get a handle on what the problems were, what it made sense to do.

Another very important and, to me, very interesting program was the Vietnam Program, which was set up to help the South Vietnamese and not primarily the U.S. military, but the South Vietnamese military. And soon after that it was extended—extended to help the Thai military—so we had large groups, like a hundred each, in Saigon and in Bangkok, where we had laboratories and so on. We had a lot of contractors in both countries working on the problems involved with the counterinsurgency at that time. It was mostly counterinsurgency by the Viet Cong in South Vietnam, supported by the North Vietnamese, but not yet with a lot of regulars—regular North Vietnamese Army.

And in Thailand, there were insurgencies starting up, mostly in northeast Thailand, fueled the same way. We were trying to help those. Again, a huge challenge. Not clear how to do that, what needed to be done—priorities and so on and so on. There were a couple of other things, some that were very important in retrospect, but going well so I was pleased to see them, but they were not anything I had to deal with immediately.

And the most, I think, important one, in retrospect, is the program that set up twelve interdisciplinary material centers at twelve different universities. The idea of the program was to get a number of scientific communities together—physicists, chemists, metallurgists, mathematicians—to work on materials in an integrated way. It was a new idea. At the time, the field was split academically into five different departments that wouldn't talk to each other, rarely collaborated. This changed all that.

Without that program being a success—and I think it was a huge

one—I think the electronics revolution of the seventies and eighties wouldn't have happened as naturally as it did. And that, of course, transformed the economy of the country and of the world and the way we live and everything else.

So, that's what was going on, and it was very exciting and very fast—(chuckles).

I: Asking what seem to be simple questions at DARPA seems to generate much larger challenges. You talked about missile defense, let's use that as an example "How do you stop a missile coming at you?"

Herzfeld: Well you start where you just did. A missile is coming at you. First of all, "how do you know?" So, satellites were built to detect the launches of large missiles. These were up there and scanned the earth, and the infrared signature of the missile plume would light up the sensor, and then that would get reported back and so on. So, that was element number one.

Second, radars were built all around the U.S. to detect the missiles coming in over the North Pole but also you have to worry about missiles launched by submarines, so these radars had to really cover 360 degrees and they didn't right away, but eventually they did.

Then the next question is what do you do about it? Well, there're two or three layers you have to address. First of all, you have to tell the president—(chuckles)—very fast, and the system was set up, indeed, to do that within minutes and the President was informed—you could inform the president within, say, five minutes. And a mechanism was set up so he could get his principal advisors that were in the loop for a statutory reason, like the Secretary of Defense and so on, together for a quick conference.

And then the military had prepared—DARPA was not in that that much—the response and the first response that people thought of was, "The missiles are coming in. We have to be able to threaten that we can launch our missiles against whoever the enemy is." So, that was set up, and DARPA did not work very much on that except an important part which we can come to later.

So, you have to understand what happens and the big thing is the missiles make a big signature in the sky. You know how they look. You might not see it visually, but you can see it through telescopes and see when the reentry happens, the ballistic missile front end gets into the atmosphere. It makes a huge plume somewhat like a comet. Well, you might be able to tell with a real missile warhead, or is it something else? So, you have to somehow estimate how much energy is going into that plume. That means you have to understand the chemistry of what's going on and you're right away deep into chemical reactions in space of a sort that you couldn't produce in a laboratory easily. And how do you look at that to find out what's really going on? So, that's another branch. Call it the "discrimination branch."

Then, finally, you really want to shoot back and explode the incoming missile before it hits anything, so you get into the interceptor business and, "Where do you place them?" "How big are they?" "How fast do they have to be to get there in time?" and-and-and. All that turned into, let's say, a thousand

different, little projects, some of which were very expensive.

We built an enormous radar on the island of Kwajalein in the Marshall Islands downrange from the Vandewark missile range, so we could look at our own test missiles and really observe in detail what was going on. And so on. I mean I can go on for several hours here. It was complicated, difficult and—but also wonderful. Let me give you a sideshow here.

At one stage early on in the project, people thought that when a missile is launched through the ionosphere, the stuff of ionized air that's maybe 50 miles up—that would make waves in the ionosphere—you could detect that. To detect these waves far away they built a large radar dish in Arecibo, Puerto Rico and by the time I came to ARPA, it was already clear—two things were clear. One, it was probably not the most important thing we could do for ballistic missile defense—but, second, it was an absolutely marvelous radar telescope to look at other stuff.

And there was a funny story that many people wanted me to kill this and I didn't because I thought there was really very interesting stuff being done both for military and also for scientific reasons. And this has been just an amazing success. The NSF took it over later. It's still there. You can see it on Google Earth. If you look at Arecibo, it's a thousand-meter dish that's fixed in the ground and there's a large structure to observe things and so on.

So, many things happened serendipitously that were not top-importance but had wonderful side-effects—fascinating side-effects.

I: You'd been Assistant Director. Was that under Dr. Sproull?

Herzfeld: Deputy Director and it was Sproull. Made a difference.

I: What kind of an organization was DARPA by the time you became Director?

Herzfeld: It had about eight major program offices. The biggest one was still DEFENDER; nuclear test detection; project AGILE, which was the Vietnam, Thailand and other places program; the material sciences; very importantly, IPTO, the Information Processing Technology Office, which was basically started with the ideas of a fellow by the name of Licklider, who deserves to be much more famous than he is; and the ARPANET/Internet came out of that and that was just being born. I had the pleasure of signing the first few ARPA orders for the ARPANET, and that we sort of knew what we were doing. It was a gamble but it was an important one.

And a couple of minor offices that were quite important. It was pretty well regulated. Every ARPA order—that's the funding instrument—had to say what was supposed to be done, why it was supposed to be done, what good it would be to the defense of the country, who was supposed to do it, who was supposed to run it and—little details like that. So, we got that in hand. I think it was really clarified when Bob Sproull was Director, and that kept on being improved.

The nagging worry now is that it has it gotten so perfect that it slows things down a lot? We were able to issue ARPA orders in one day. And we did a few and some of them are quite memorable. And I used to brag there

were only two places where you could come to Washington with a good idea, get a million dollars at the end of the day. ARPA was one and the CIA was another, and we were proud of that.

I: What were a couple of those ideas?

Herzfeld: Well, the most important one, of course, was —Bob Taylor tells the story better than I—he came in to see me and said, “We’ve really got to do this ARPANET thing now.” And he wanted a million dollars and I said, “Sure.”

And he—he claims—says in 20 minutes he had the decision. I doubt it took that long, but I was curious what exactly he wanted to do. We’d talked about it many times before, and I was ready. “Sure, go.”

And that one changed a lot of things. From the ARPANET came the Internet, from the Internet came the web. Changed a lot. That was just the first step. (Chuckles.)

There’s an old Chinese proverb: “Even the longest journey starts with a single step.”

I: When you became director and you looked at the projects in the agency, were there some you felt needed to be weeded out to make room for others?

Herzfeld: Oh, sure. Well, no for the top-level projects. I liked the lineup. I had had a hand helping Sproull getting us there, indeed, working with Ruina on that too, so I was comfortable with that. It was sort of kind of my list and in fact, when I was Deputy Director, I added one called “advanced sensors,” a kind of a specialty job—niche office—but very important and very high-quality. In fact, got heavily involved with the Vietnam War, although that wasn’t its primary intent when we funded it.

But in every program there were things that had to be dropped. But then the ideas were reprogrammed to start new things. Fortunately, ARPA and DARPA have never been short of ideas. We’ve always had more ideas than we could handle. We usually got money to do them. Money was not the main constraint. Good people to run the programs was always the constraint.

So, yes. I think we always did that, and I asked the Office Directors, the big program directors—Project Directors, I guess, to help me with that and I made them give me their priorities and we got together and drew the lines where you had to stop funding.

So, I liked to do that as a group effort. They knew about each other’s programs. I worked very hard to get them to know about each other’s programs. I tried to catalyze, and I succeeded occasionally, to have people from the different offices work together on something important. Computing and everything else comes to mind. The computer folks worked with DEFENDER.

There was something funny. We were—in DEFENDER, to get back to what some of the big programs were—this big radar I mentioned earlier was a very powerful machine and it took data at a very fast rate. The bandwidth was very large, the 150 MHz or something like that. For comparison, the old—the videotape is 10 MHz bandwidth.

But we had to record 150 MHz bandwidth. Well, there was no

machinery to *do* that with and they had really clumsy solutions for that and ARPA caused, basically, the industry for very wide-bandwidth recording to emerge. Wideband tapes, wideband recorders, wideband processing—all the stuff—videotape and—and beyond that's commonplace today ARPA started.

I: And infrared- DARPA helped develop that, too.

Herzfeld: It was very hard to do. It was a laboratory curiosity. It was known in the labs since the thirties and people did funny films and so on. It was awkward. Solid-state detectors were known since the late thirties, but very hard, very funny materials that aren't very stable and lots of other trouble.

ARPA helped make that into a solid material science thing and then the material centers helped. I mean they knew how to do that and they did. And the intelligence community was working on that and we all—the intelligence people and we—put them in orbit, put them on satellites, put them on the water, put them underground—everywhere. And they're now all over—the same technology—solid-state detectors—every camera, *everything*.

I: Of the application of scientific thinking to these problems, how different was that approach, then?

Herzfeld: That was very, very important and that was one of the interesting hallmarks of ARPA and still is. It was clearer early on because we were all finding our way, but we would start from a military problem. Say a missile is coming at you. First, what do you do; second, how do you know; next, a whole long series of technical things that you can think of that need to be done, need to be understood. Most of all a brand new science that's never been done, never been done as accurately, or as quickly, or as conveniently —like out on a range, or from an airplane.

Well, we learned how to do that. We learned that you needed communications that were very robust and very fast. We learned that we had to have very good telescopes. We had to have very good, very fast interceptor missiles—all of these—and so each large military problem was broken down into smaller military problems and each of these was attacked, but how do you really *do* it? First in the lab and then on the range and then can you get something you can give the troops, so you can drop it and it won't break? Standard problem in the theater.

This generated a very rich kind of a carpet of problems like everything woven together. These were not standalone things, they were together. And they were attacked by different people. We had people in the labs. We had people in the universities. We had people in—with—Boeing. We had Raytheon. Westinghouse was doing a lot of radars. It was a very complicated sociology infrastructure and we had a lot of helper organizations that helped us think about all the details and how they connected to each other.

We and our high-level advisors, like the Defense Science Board and like the JASONS, glued the pieces together in our own heads and understood how that was supposed to work together.

Now, one thing I faulted us then and still do: we didn't write enough

of it down. Now, we did start a classified journal about missile defense research and then broadened that to all defense research. It was a very high-quality journal but that died around 1990 and it was lost. It was a mistake. It died because our customers didn't care about that.

I: Why DARPA? The Army has a research and development arm. The Navy has one. The Air Force has one—why couldn't they address these problems?

Herzfeld: They can and some do, and some have done it, but it wasn't as systematic and it got too quickly enmeshed in the service programmatic concerns. Now, the Army budget, or the Navy budget became the issue and not how to do the job right. Now, I think, on the average, the Services have done very well. ARPA was called into being because the Services couldn't get together on the space program. They couldn't get together on the ballistic missile defense program. They were not interested in Vietnam, particularly. They were not interested in nuclear test detection. They were not interested in a major program to reshape all the material sciences. That was a very big idea, which was the idea of Jim Killian, who was then president of MIT and a close advisor of President Eisenhower's.

They had a lower-level view of all the problems we were all working on and ARPA/DARPA, because it works for the Secretary of Defense, who worked for the President, can afford to have a very high-level view of all the things it works on. And that helps. Sometimes it hindered us, but usually it helps. It helped me enormously.

I: The Services are ultimately the customers. What was the relationship then between you and your customers?

Herzfeld: For *some* things. They're the customer for things that go to the troops. Not everything goes to the troops. The Material Science Center output did not go to the troops except after a long, winding road. And establishing a connection through a ten-year winding road is what takes other people than military departments. The Office of the Secretary, if it's working the right problems is interested in very long-term problems. It used to be.

I think what has happened now is that it's more and more new-term problems having to do with—with the War on Terrorism, for example. People are not willing to think about very long-term issues and that's too bad. We, in the middle of a Cold War, we were not only allowed, but *requested* to work on very long-term issues. It was great.

I: What changed?

Herzfeld: That's a very tough question which I spend a lot of time thinking about, and I don't have a snappy answer. And maybe there *is* no snappy answer. I wish that the probable answers to that question would be studied more. I've spent a good part of my waking hours trying to do that, not necessarily in the ARPA context, but in—in the national security context.

I think the quality of the public discourse about defense issues has gone down and one measure is the reception I had from the congressional

committees when I was at ARPA and briefed them on our programs. There was genuine interest in the content, the meaning, and the approaches of what we were doing. And in later years when I've talked to—testified before—committees of Congress having to do with national security, it was much more political, much more near-term and, frankly, not very interesting technical issues. And that's too bad.

I: How did the public discourse get so diluted? Was it a brain drain by the computer science explosion, the dot.com explosion?

Herzfeld: I don't think so at all, no. No. And in fact, these explosions that you describe kind of went past the defense establishment wherever you find it. There's a good deal of talk about it but there's not as much understanding as there needs to be. People don't have the patience to deal with these things.

One of the big problems with our political discourse—it's a little bit off-topic but not a lot—is the 15-second answer has to be found. And in my life experience, which is fairly long, few really important problems can be answered with a bumper sticker.

I: When you were Director, what was the nature of DARPA's relationship with the Services?

Herzfeld: I would say basically friendly, basically cautious, occasionally opposition. But going in, it was guardedly friendly. Occasionally, we were in very uncomfortable situations vis-à-vis the Services. Let me give you a little story.

A week or so after I came to ARPA to take on DEFENDER, which took several months of revolutionary social construction, Jack Leonard comes into my office and said, "Charlie, we've got a new program, and you've got it."

I said, "Oh, great, boss. What is it?"

He said, "Let me tell ya. It's really important. It comes from the Secretary of Defense directly to us. Harold Brown helped to develop the idea, and he and Jack Ruina—who were very close—came to us."

The problem was the Soviets were building a ballistic defense system, a big one. Lots of money going into it, and we knew that. And there was evidence that was going on—clear evidence.

Jack says: "How can we make sure that U.S. ballistic missiles can penetrate Soviet ABM defenses? Figure out a program to make sure that we know that and that we're right. That's your job. Start now." And here we are.

I said, "Okay." Went into another huddle of a different shape and color. "How do we do *this*?" And, in fact, it was kind of a symbiotic relation with the ballistic missile defense. You had to understand defenses to know how ballistic missiles would penetrate, and it was true about either side, either way. And you had to be very judicious that you didn't let your sort of blue hat influence the red hat too much. And that's a very, very demanding task, and we did it. And I thought we did it very, very well, and one of the great things we, ARPA, achieved was that, as our program got under way. And, by the way, I briefed the Joint Chiefs of Staff on that fairly regularly. I have other stories we can get to sometime. We were able to assure the Secretary of Defense that he could

assure every President that our ballistic missiles would be able to penetrate Soviet defenses. We were able to do that for decades, and it was important. It had a lot to do with our president believing that he had a credible deterrent and letting the Soviets know that he thought so and, to some extent, explain that to the Soviets. And it worked. Kind of important.

I: What were the challenges early on with the anti-ballistic missile defense systems?

Herzfeld: The question was, if you are threatening me with a ballistic missile system—hundreds of them, thousands of them —what's my point of building a defense if I know that, one on one, most of yours will get through? The answer hinges on the details because you won't be able to be absolutely sure that you'll succeed with your attacks because of my defense, and vice versa. So, it raises uncertainty on both sides and uncertainty cannot be overcome by a first strike. So, it's very stabilizing. In fact, defenses are highly stabilizing and people who think they are destabilizing haven't understood the problem. Now, I know I'm in a minority but so be that. I mean that doesn't worry me.

The same is true, by the way, about SDI. Had SDI been deployed it would have seriously undermined the confidence of other countries' planners to execute a successful strike against the U.S. and that's what that is all about.

It's like saying if you have a shield and I have a sword. Should I have a little shield just in case you have a sword? And the answer is, "You bet." And if it's very asymmetric, the guy with the sword will win in the end. If it's sort of symmetric, we'll think a lot before we draw the sword. And that's the point of defenses always.

A better, easier example is tanks—the armor on tanks. A good anti-tank gun will defeat any armor. We know that now. Does that mean tanks shouldn't have armor? Of course not. You wouldn't dream of spending 5 million for a tank and not put the armor on—would you? Now, think about that. How come? How does that armor—answer is that in the armor/anti-armor game, a lot of things are going on, and you *do* want the armor. If the other guy gets the ideal shot at you, you're dead—it's obvious—but not the whole story.

You have to put the armor on the tank anyway and he rarely will get a perfect shot. But you can be sure that if you get a perfect shot, his armor won't help, either. So, it's a question of directing the flow of the attack away from critical things and that defense can do its job up to a point.

I: Why was the Chinese knocking a satellite out of the sky such a big deal?

Herzfeld: It's a very big deal, and it is a different subject, but I'm happy to talk about it.

The Chinese have said for the last ten years that I know that in a war with the U.S., the major advantage that the U.S. has is our space assets—communication and reconnaissance. And the Chinese always say, "Therefore, we must learn how to deny that to the U.S." —that advantage. That's straight military thinking. Every professional soldier thinks that. The Chinese are very serious people. They've shown us they can do it up to a point.

Does that mean they have control of space? Of course not. It does mean that if they really mean to they can make some trouble. Can we make trouble for them? I hope so. Of course. This is assuring that there's a balance and that the other side knows that there's a balance. It's a game called "deterrence."

I: Back to the relationship with the Services. At that time they were being pretty distracted by the rising conflict in Vietnam, weren't they?

Herzfeld: Not in '61. But in '65 when I became Director, the North Vietnamese had made the first large incursion into South Vietnam. That happened in the spring of '65. By the way, my first trip to Vietnam was in '63 and I'd been going there every year, for three times a year, for a week in Vietnam and a week in Thailand. I spent that time not in laboratories but traveling around and seeing a lot.

I think the Vietnam War was considered a sideshow for the military. The Cold War was the main show in a lot of ways. Most of our effort was on the Cold War-related issues so we had excellent relations, for example, in the study of the U.S. penetration aids for ballistic missiles. The Air Force launched a lot of targets that we helped design and we geared up the experiments to observe the shots, which Air Force helped us analyze and so on. Close collaboration, lots of money spent on it—by both of us up front.

I: Was there a relationship with university research facilities and programs at that time? Was the research done mostly through the government labs?

Herzfeld: Oh, no. The ARPA umbrella covered everything and integrated in some way or other everything. So, in all of the strictly military programs, you had lots of university people. They did research on the basic issues, like, oh, does the plume in a reentry vehicle wake develop and can you investigate that? You know—this took engineers and chemists and physicists, mathematicians—everybody.

How to integrate missiles and radars and communications into a system? We had people like Lincoln Lab and lots of contractors working on it, but also some university people.

I think ARPA/DARPA always had the intention of having the basic researchers help us with the analysis of their data and the application of their data to practical problems. That's not always understood, and it's worked sometimes better than others.

By the way, there's something I brought with me from the Bureau of Standards, where I had the job of involving academics and doing the research needed for the standards. The standards were imminently practical things that had to work on the factory floor when working on a jet engine. You had to have thermometers in a jet engine. Well, how do you do that? We had to help think through how to do that.

So, this notion that applications always have a trail of basic stuff — an underpinning of basic stuff—is something I've believed for decades. I think it's just a fact of life—of modern life. Not everybody has thought that through, I

think. Like to slice it horizontally, doesn't work. It works, but you get a hash. You get that stuff.

I: What is the balance that's struck, sometimes not struck, sometimes overstepped between basic and applied sciences?

Herzfeld: Well, you know, the distinction is artificial so I question the way you pose the question. But, of course, that's an old debater's trick—never give in on how the question is formulated. That's the first line of defense and attack. But there's a message here and that is that we make these distinctions in offense-defense, for example. They're very artificial; they're very arbitrary. We don't have a Department of Offense and a Department of Defense. We have a Department of Defense that's supposed to be both. The notion that defense is harmless and offense is bad is a civilian mistake. The real world doesn't work that way. It's an amateur mistake, actually, and all you have to do is look at the history of real wars to see that. And it's too bad. It's part of this low-quality public discourse, I think.

I: Well, there have been shifts in funding from basic research to either applied or maybe engineering execution.

Herzfeld: Oh, yes. And—and I think some of that's fine, and some of it's not, and it's not easy to judge at a distance. Unless you really know the details, you can't tell.

The biggest issue—this is another footnote which you can do with what you like, but it's important—is the distinction between basic research and applied research, and sort of engineering that also is somewhat artificial. And the distinction came in the Pentagon context as a budget exercise that McNamara stated as soon as he came to the Pentagon. In other words, to get some order into the huge Package 6, which is R&D—RDT&E—all of it.

They segregated it by who does it. If the academics do it, it's 6-1. If the labs do it—like Lincoln Lab or Los Alamos—it's 6.2. Boeing does it for an airplane, it's 6.3. In fact, if you look hard, these flow into each other smoothly, and I think we have gone too far in questioning the utility of more basic work being done—and that sometimes shows when we have problems with airplanes. The wings corrode, or the engines blow up.

I think there is no ideal way to do that, and I think people find their way. They try things. The drift has been toward lower basic research and there's kind of a funny notion going around in DOD that NSF takes care of all basic research. That's a model of basic research as if—as if it's a grocer, and if you hire a grocer, he'll have it on the shelf. That's true for flour and canned peaches, but it isn't necessarily true for technical results, because NSF primarily funds whatever people in the research game want to do.

ARPA/DARA has the unique ability to influence the basic research people work on by funding people. But we want them to go where the problems are. And that has worked really well, and still does, and DARPA is doing well now and getting better.

I: Is there a shift between long-term payoff and short-term turnaround? Is there a balance there?

Herzfeld: If you're in a war, the short-term becomes more important and that is as it should be. That doesn't mean you can stop all the long-term stuff. If you do, that's like eating your seed corn. You really oughtn't to do that, because next year you won't have any seeds, and you'll be *really* hungry. But, yeah, that happens.

I think politics has a lot to do with it. There's something called the Mansfield Amendment in 1968 or thereabouts. The Senator Mansfield put into a budget of the DOD a very strong restriction that everything DOD funds must be directly applicable to defense. What that assumed is that you can tell, and you can't. You know, working on a lot of things—there are national threats, for example, biochemical threats, information warfare threats that were not invented yet when Senator Mansfield was senator. It's a dangerous, dangerous way to go. I'm more comfortable with spending a few percent more on unconstrained things and having a much wider reach of ideas. I think that's the better bet for the—

I: —the so-called “relevancy”?

Herzfeld: Yeah. And, of course, everything should be relevant. When we worked on the ARPANET, we did so in part because we knew it would help, in the long run, the military command-and-control systems. But we also did it because, in part, it helped scientists do science better, and so it came to be—both of them.

I: Was that in response to Arcibo? –

Herzfeld: It was in response to the Vietnam War. He (Mansfield) did not like the Defense Department supporting some parts of the work in the Vietnam War and as I recall it, he was particularly opposed to the Defense Department spending money on social sciences. It was just trying to understand the enemy better, and I guess he was opposed to our understanding enemies. That's fashionable today, too, by the way, to not study our enemies. And ARPA makes history in this. A friend of mine, Cy Ditchmond, wrote a nice book about that, about his personal travails with that. He ran the AGILE program for a while.

Ruina started the social science program in ARPA, the first one, and Lickliger, who took it on, also was director of the social science program. I think it was a brilliant idea and it failed sociologically and politically. Well, that happens and you got to be willing to take risks that are not sure things, particularly if you're in ARPA—or in DARPA.

I think one of the modern problems is that we're not thinking enough about our friends and our enemies. We're not studying it with DOD money. And let me tell you there's nobody in the government who really knows how to spend that—spend for basic research with a very applied application in mind. The DOD is by far the best at that. Variety of reasons that are interesting to think about, but no short answer why. A lot of decisions have been made in the last few years that were not very helpful because of that lack of

understanding.

Incidentally, ARPA started something under my aegis in the Middle East. I went, as I said, to the Middle East a number of times. We had an office in Beirut and an office in Tehran, and I went there twice, three times a year. Began to understand something about the Middle East in the sixties that we still haven't gotten on top of. Why is it that in the Middle East, more than in any other society I know of, the history of family feuds are really important? The fact that your grandfather insulted my grandfather means we must be enemies until I can avenge my grandfather. Well, that's kind of a deadlock, and that's where we are.

What I asked the American University in Beirut—which is a very fine university—to do was to begin a study of the relationships of all the major families in the Arab nation—in the Arab community, because mostly they're Muslims. All the mosques have all the records of who married whom, going back well over a thousand years. It would have been possible to trace who are the influential families and what are they influential about, and who's related to whom and so on to really trace that and understand it. And it was a very exciting thing.

When ARPA got out of that business, the funding failed, but more importantly, the civil war in Lebanon began, and American University was basically destroyed. Its program died. It has not been resurrected. There's nobody who cares about it.

It would be really neat to know that today when you're trying to figure out, can these two people who are supposed to be working together really work together? Is that a sham? Or, is somebody else really pulling the strings on both of them, or what? We're clueless.

That's what we tried to unravel.

I: AGILE was star-crossed from the beginning, almost.

Herzfeld: Of course. Lots of reasons why. The war was so unpopular. I mean the decision to help the South Vietnamese was an Eisenhower decision way back, after the French pulled out. And I think it was the right decision, actually, but we have a lot of ways to miss the boat when we want to. People really weren't interested in it. And they said, "Oh, yeah. We gotta talk about that, too." It was kind of always the fifth wheel. The real focus was on Cold War things.

Now, when the North Vietnamese got really very seriously involved, that was then a part of the Cold War issues, but that never really persuaded many people, or took hold. It was messy. It was not mostly an engineering problem. ARPA/DARPA is mostly an engineering and science organization, and things that are not mostly that get short shrift.

Now, for reasons that are complicated to explain, I have been interested in things that are not just science and engineering, and I felt quite comfortable trying to understand that. In fact, I made a personal study of counterinsurgency. I went to find most of the people who had fought insurgencies and I got to know very well Robert Thompson. So, Robert Thompson, who won the Malaysian counterinsurgency templar, was the political figurehead. Robert Thompson was the doer, and he was a policeman—very high-level cop. It turns out the counterinsurgency was mostly a police problem—

not a military problem. And soldiers aren't good at being policemen. That's a long story that ends in today's headlines.

I thought we understood it, and—and AGILE ran biannual—no, semiannual symposia on R&D, and we still have some of that material, some classified, and they still have it.

And we developed a point of view which a number of people in the political science community thought was right. The people in charge of the Defense Department obviously were technical people, and there we are.

I: Letting McNamara know that he was fighting the wrong war?

Herzfeld: I tried that. Before I left, I decided I owed it to my country to take a major risk with my career. At first, I talked to Johnny Foster who was not happy with what I was trying to say, but let me talk to McNamara and arranged it. And I talked to McNamara and the issue was the point view, is it a military or a nonmilitary campaign. The role of combat versus protecting your side of the civilians was another one. And McNamara was totally uninterested. He explained to me that, "No, no—this is all—you're all wrong, Charlie. Let me show you how this really works."

He talked about food distribution and so on, not about the issue. That never got joined. He and I were on—on good terms, but it didn't really work well. We'd worked fine on other topics, but not on that one. That was a killer. It was a killer issue. And it is again.

I: Did you really say that to him? "You're fighting the wrong war"?

Herzfeld: Yes. That, "We're going to lose if we keep on doing what we're doing." And he was not amused. Nobody was amused. I was in anguish. I don't do that lightly or easily.

I: Talk about the Army and Project DEFENDER.

Herzfeld: Yes. Ah, yes.

Well, McNamara, he was very good at using the DEFENDER program to push the Army Ballistic Missile Defense Program, and he had me brief him—him, McNamara—twice a year on the DEFENDER program, with Brown and Ruina along and, later, Johnny. And he always made sure the Secretary of the Army was there, but without staff—minimal staff. And then when I told about a recent development, a new program or something, he would ask—Cy Vance who was Secretary of the Army then—"Do you know that Cy? Did you know that?" And so he was basically using me to beat Cy Vance over the head, which neither he nor I enjoyed, but that was my job.

But we were able to encourage the Army— "encourage" in quotes—to up the performance of a number of their systems a lot by demonstrating it was feasible. One that comes most to mind was the acceleration of the interceptors. We showed, by expenses, how a \$20 million program (which is now two—the equivalent of 200) that one could, indeed, build interceptors that were much faster than what the Army was planning. And so they compromised some. They made something in between—picked something

in between for the program that they were pushing.

I: For the NIKE?

Herzfeld: Yes, NIKE X.

NIKE X had new long-range and new short-range interceptors and this issue was about the short-range interceptor. And it makes a lot of difference in how big an area you can defend because that depends very much on—sort of on—the square of the acceleration of the interceptor.

I: At what point were they talking about putting bombs on the warheads?

Herzfeld: All the time. That's all there was at that time. Did you know that the NIKE Hercules had a nuclear warhead? All around the U.S. cities. There wasn't any other way to shoot down bombers. They were all around. Well-kept secret.

ARPA/DARPA was the first one to really systematically go after non-nuclear kill. We had two or three programs that we started there saying, this nuclear warhead thing may be necessary, but it's hard to do. There are lots of consequences. Certainly, some ground damage, so let's go for a non-nuclear kill. Hit to kill. Complicated business. That's what Vietnam had been for decades.

I: Tell me about the relationships and how necessary good working relationships are with secretary of defense or with DDR&E and how those changes may change.

Herzfeld: Well, it worked best when the Secretary was engaged in the issues and when the DDR&E, who is the main in-between person, was really powerful. There's been a flow—an ebb and flow in the DDR&E. It started out very powerfully with Herb York, Harold Brown and John Foster, then became less, then back up and then down again. ARPA/DARPA usually reported through the DDR&E. If the Secretary's involved in the program—if ARPA/DARPA works on his problems *and* the DDR&E is a powerful guy—it works wonderfully, unbelievably well. If any one of these is lacking, it's so-so.

I: Three physicists from the West Coast.

Herzfeld: Yes, all Livermore nuclear weapons people.

I: Was it easier being a physicist?

Herzfeld: Yes. I came from within the government, which was very unusual.

I: It seems that the level of communication, just being able to—

Herzfeld: It was excellent. It was excellent, but some of the issues were not physics. Vietnam was not primarily a physics problem. Really not.

I: Are there some projects like the software sciences that ARPA/DARPA couldn't, shouldn't deal with?

Herzfeld: Well, whether it should or not depends on if not ARPA/DARPA, who would do it? And the Defense Department, until you get to the Service level, has

no competence other than ARPA/DARPA. At the Service levels, each one has their own research organization and their own development organization. The research organizations tend to fund academics without much connection to applications—and the development organizations want to build ships, aircraft and tanks. They don't want to fool around with this stuff.

So it's a fatherless child, always an awkward situation.

I: The notion of transition. What was that all about during your time there?

Herzfeld: The neatest one and the most directly successful one was the M-16. That started in Vietnam, and the reason was that the American Army rifle which we gave the South Vietnamese was too big for the average South Vietnamese soldier. It was much too big. It was also designed for a different war. It was designed for engagements in Europe where the average engagement distance was around a thousand yards. In Vietnam, we found out very quickly the average engagement distance was 50 feet. So, you needed a small weapon with a very high firepower.

So, ARPA in 1960-'61 sent a thousand Armor Light 15 rifles—which is the precursor of the M-16—to the South Vietnamese Army, who gave it—at our request—to a single South Vietnamese Ranger battalion and equipped the whole battalion with the Armor Light. They loved it. It was wonderful. It had problems. It was not a fully mature design so it jammed too much and so on. It was light. You could carry large ammunition. It was very high firepower—fire rate, and it was good for short distances—not long distances. Not a good sniper rifle, for example.

Well, then McNamara said, "That's what we'll give the U.S. Army." The U.S. Army really was opposed to it. I made a number of enemies in high Army ranks because they thought I was pushing it very hard. I was not. I was kind of a follower of orders in that way. And the surprise came when General LeMay, who was Chief of Staff of the Air Force, bought 600 M-16s, the first buy in the U.S. military, for his Air Force security force that guarded the silos all over the North-Central West. And that broke the logjam. And he liked it very much. He thought it was just right for his guys. It was good. It was really useful.

And then the Army went along and took it and now, of course, I think it's not bad for in-the-city fighting, but it's not great in the country, where you can see for a thousand yards or two. You should like to shoot that far and it won't. So, now people are going back and all the sniper's rifles are back in other models.

Small arms people think it's simple. It's not. It's a very mature art and it's a very complicated use issue. They use it in all kinds of ways now—all kinds of people to do all kinds of things other than shoot people. I mean, except, it's all shooting people, of course. The Army likes to have everybody on the same rifle and I argued for some time that that's not really right, but the Army has arguments. I've never run an army so I'm careful about sounding off about it. But—but it's complicated.

But the other transfer took a while. It took maybe two, three years, and the Army redesigned it and made some things worse and it had to be

redesigned again. But in the end it was an ARPA product. And all Directors of ARPA/DARPA have been proud of it.

I: What happened?

Herzfeld: Well, it's a complicated story. The Army likes all this—all professional military like to redesign everything a civilian suggests. They say, "I'll do it my way." And sometimes it improves it and occasionally not.

I: Are they transitioned sometimes too early?

Herzfeld: Often. Yes.

I: How does that happen?

Herzfeld: Pressure to get on with it. The laser-guided bomb example is one. ARPA and the Defense Sciences Office built the first laser-guided bombs to attack a particular bridge in North Vietnam that cost a lot of lives trying to destroy but never succeeding. So, we built eight copies of a special laser-guided bomb that was supposed to hit the bridge and—and it didn't. It all failed. It was an Air Force-run operation, and we looked at it. This was after my time.

It was started when I was there but I'm not about to tell it, this is after I left. And analysis showed that there were a lot of things wrong with it. The design wasn't good. The manufacture was faulty. There was no maintenance. There was no training how to use them. There was no doctrine how to use them. These are the things, by the way, that go wrong if you transfer too soon.

The Services march to their own beat. And it's sometimes slow, but it has to be thorough, so that whatever they do really works for a million people and will work the same way every time. So, different from when you make ten or something.

Well, they went back and built another round—I think this time ten bombs—and they dropped the bridge a year later. So, that was too soon, not ready. They fortunately went back. And out of that notion came the idea of precision warfare—that we were going into an era where you could hit an individual tank, an individual truck, an individual machine gun nest, an individual command post with one shot. Not all over the landscape, but just the target, that that was feasible.

And that was one of the ways to conquer the overwhelming advantage in numbers that the Soviets had had there been a war in Europe—that higher quality. And that was a deliberate strategy called the "offset strategy." Andy Marshall had a lot to do with it. Several Secretaries of Defense, several DDR&E's. And we were just one of the first to put a hammer to the metal.

I: Well, Vietnam was a distraction for the Soviet surprise?

Herzfeld: I would say—well, there was no Soviet surprise. I mean that's a false story. I'll tell—well, it's an important point, but I'll come back to it. Please remind me.

We, at ARPA tried very hard—and by "we," I mean certainly Ruina and Sproull and I—to not let the Vietnam problem distract the rest of ARPA. I

don't think it did. I think that's just not true.

What we did find was that Vietnam was a great laboratory to try out stuff. Seriously. Laser-guided bomb was one, M-16 was another, and there were many more. That's a story on its own. There's nothing like a real war to try stuff.

The way the Vietnam effort of ARPA hurt ARPA was politically. Congress got mad at us about it for political reasons. Some of the military got mad at us for political reasons. Some Secretaries of Defense got mad at us and so on, but it wasn't the bread-and-butter stuff.

Back to surprise. It's now often said that ARPA was set up to prevent surprise. That's backwards. ARPA was set up to *provide* surprise—for our side. Preventing surprise by somebody else is much too passive. The best racers are those who run fast races. You've got to keep ahead of it all the time. You got to think capacity thoughts all the time, imaginatively how. "How can I screw up the other guy?" "How *really* can I make him uncomfortable?" And they will be unhappy.

SDI's a great example. We talked about it earlier. SDI was arguably the last nail in the Soviet coffin. They gave up, said, "We can't compete in that, too." It had an enormously depressing effect on the Soviet leadership. It was high-tech stuff. A lot of it worked—enough worked so it was plausible. That's how you win technology races—not by thinking, "What are they working on?" Well, you have to do that, too, but if that's all you do, it's not enough. I like to be an aggressor on that.

A lot of experience has been lost. That's too bad, 'because we don't transmit the stuff, and there's no continuity. One mistake I won't forget with the journal was it was just a scientific journal. There was no popular science press—disappointing. It's basically an anthropologist's history and a historian's history. There's very little about what actually was done. There's a lot about what people said to each other, very few documents. They're referents but they're not shown. In my ARPA/DARPA story, I actually *show* the first ARPA order. I show the directive that set it up. "Here it is. Read the text."

I: You come back to DDR&E at that point?

Herzfeld: Right. I came in 1990, and the Wall came down in '89, I guess. The war is about over when I come back.

I: What was the climate like after?

Herzfeld: Most people didn't know they had slain the dragon, that it had really changed a lot. It took several years to realize that. Few in America thought we had won the Cold War. No one—no one in Russia that I talked to and I talked to the Russians in 1991—doubted that they had lost the Cold War. Nobody. So, there's a curious disconnect.

And our task—when I was at DDR&E—our very early task was, "How do we build down? What don't we need?" I was chairman of the Nuclear Weapons Council, among other things, which was kind of to reconcile the needs by the military and the production side from DOE. In fact, both numbers were

going down. The need by the JCS went down, down, down every time we met, and the ability of the DOD—DOE to build weapons went—(chuckles)—down, down, down with that, so it was a matter of keeping a balance between these two races downhill. It's interesting. Quite the opposite of what the popular mythology imagines.

But we took a lot of weapon systems out on the standard weapon systems side, downsized a lot. And there were two things going on at the same time. There was a great deal of resistance to doing that. Particularly, the military said, "Don't slow down so fast." And from the civilian side there was hasty cutting. "Get more out," "Get more out," "Get more out."

And I often was in the middle of that, because I wanted to not destroy the military programs, which were there for good reasons. The good one was the B-2. I spent a lot of time arguing about how many B-2's it would make sense to have. And a lot of civilians said, "Basically, none," and the military thought they would buy 200 or 220—some number like that. We wound up with 20 in the inventory, which is what we have now.

And it was hard to find a rational way to do that. And what a couple of us thought about and I executed was to ask the military how many units of an airplane make an operationally useful unit? They came up with four or five of these big, big airplanes. I said, "Okay. Let's assume it's five."

Then the next question is, "Whatever we buy should be a multiple of fives." Should have five, or 15, or five, or ten, or 15, or 25—but not 17. So, we had started arguing about how many of these units the grand strategy required. That was made up, largely, of people didn't know what the right answer was. So it was settled on 20, which was a useful number. Same thing was done with a number of other weapon systems that had to be cut a lot.

At the same time, I thought some things were desperately needed—a father that they didn't have, really. So I tried to do some of that. Some of those examples are still a little hard to talk about publicly; because they're still...the problems haven't gone away.

Huge changes. Downsizing, this then had to be reversed. I mean that we're coming down from the Reagan buildup which ended with the end of the Cold War. And then people just wanted to go down. Actually, the Reagan buildup stopped halfway through the Reagan administration and the build-down started right about then and people wanted to take that way, way deeper than seemed like a good idea.

So, these were kind of standard issues. In all of that, there were few defenders of basic research, of how to bring it into the defense community. People always said, "Well, what do we do with these people? We really don't know how to talk with them"—which showed they were really not working the hard substance of the hard problems, 'cause they always had to go back to "*How do you understand*—(chuckles)—basically what's going on." And if you don't you're liable to make big mistakes, and big mistakes were made occasionally.

Systems got started that had nowhere to go because it couldn't really be done the way people advocated, because they didn't understand the technical detail.

I: With the big enemy gone were you effectively DARPA's boss at that point?

Herzfeld: Yeah, right. Right.

I: Was there a rethinking of what DARPA needed to refocus on?

Herzfeld: Some, which happened. That was not bad. It was done by good people and it put them on roughly the right road. What was clear then was that the information technology would be the dominant topic that would get into everything.

But some of the big problems did not go away. Strategic deterrence was still there. Russia and China were potential peer competitors down the road. So, you have to think about how you handle that. The smaller Air Force was clear. UAVs were coming along. That means fewer pilots and fighter planes. That made a lot of people nervous. It made me nervous.

Precision warfare had worked. I mean that was very soon afterwards. Desert Storm happened in Iraq, the first Iraq war. And it was clear that precision warfare works. It was also clear that we're better at that than understanding what the targets are. What do you really have to hit? What do you have to risk?

And along with that, everybody learned to go underground. So, you had command bunkers way down. Iraqis, even then, mixed civilians in with the military targets. That was Saddam Hussein's great idea and it happened. We hit what was clearly a strategic command post—and there were lots of women and children on another floor. So, that became kind of a standard problem. Different and not easy to solve. It's just not. And I'm not satisfied that we have that under control yet. Progress has been made but all the arguments about what happens in underground facilities, which are now the item of the day, they came up then, but they had been with us before. There had been underground facilities during the Cold War—lots of them—and those who really worked on those problems knew how hard it was to work on it.

We did some of that, and it was hard, it's hard in—in the sixties, it was hard in the eighties, and it's hard in the 2000's. So, it's hard.

I: Was DARPA in trouble at that point?

Herzfeld: I don't think so.

I was the defender. I think a particular Director was in trouble—Craig Fields. And he knew it, and everybody knew it, and that was very unfortunate and totally unnecessary, but it was handled very abruptly and crudely, and unnecessarily so.

But that was for personal reasons—for personality reasons and so on. ARPA itself was not threatened.

I: Because as DDR&E you were the ARPA "boss" at that time were you impacted by that, too?

Herzfeld: Oh, of course. That took a great slice out of me, because I defended Fields. Fields, in addition to very many important things and good things he did

for DARPA, wanted to affect the sort of economic technology policy of the country. Every Director of DARPA worth his salt tries to affect national policy about something or other, and sometimes it works, and sometimes it doesn't. And I tried to do that about our Vietnam policy. I failed. It worked fine for other stuff.

When Craig Fields tried to do that in his day, he failed with that one. He didn't fail with the other stuff, and he got people sufficiently angry that he—e was basically fired and that was unnecessary.

I: Promoted upstairs?

Herzfeld: No. He was fired and then I invented a deputy job for me, which he was promoted to. No, it was very bloody and very ugly. A lot of it rubbed off on me, but that's all right. It's all in a day's work.

I: High-risk job?

Herzfeld: You bet. You bet. And if you don't like the heat, go work in the icehouse.

I: Your trip to Russia was an eye opener—to go into the country. Tell me about that.

Herzfeld: It's an interesting story. It's a bit complicated. Background is that the American view fed by lots of stuff was that the Soviet Union was a huge superpower, immense strength and vitality and so on. Skeptics said, "Yeah, they have a fabulous military establishment—very powerful military establishment—not fabulous." And it had a really effective system for suppression, that everybody was watched all the time and held in check, but nobody—I did not expect the contrast between Moscow, which was a relatively modern city—I'll come back to a couple of aspects—and the countryside was so different.

In the countryside, there was no traffic jam. The main traffic was horse-drawn carts with rubber tires! Well, that's India—r, was India when I was in India in the sixties and seventies. It was more like India than like Germany and that was important, and nobody knew it—except people who had served there.

But in the great metropolis of Moscow people advised us, "Bring your own toilet paper." "Bring lots of packs of cigarettes. People will love you for it if you give them a pack." "Bring flashlight batteries." That's what we did. We brought—and everybody had gadgets, but no batteries. They'd run out of juice—hand-held, you know, whatever. Phones, no battery. That was just shocking.

I: What year was that?

Herzfeld: 1991.

I: After the Wall had fallen?

Herzfeld: Oh, yeah.

I did not get in before and could not go with my clearances. I could not go to the Soviet Union except on an official delegation. And the opportunity

came to go with Alan Bromley, Bush's Science Advisor. He asked me could I go along. I went.

I: At what point did DDR&E slide down the command chain?

Herzfeld: It went down before, when Bill Perry was under secretary for R&D and then went up when Bill was—I don't remember who the Secretary of Defense was. It may have been Harold Brown. This was in the Carter administration. Then it went down again and one of the reasons I was interested in the job was that everybody told me that they wanted to strengthen it again, and I thought that would be fun to try.

The fact was lots of important people did *not* want to strengthen it, and a certain amount of grief ensued, but it is now relatively weak. People are talking about strengthening it again. I think to really strengthen it needs a Secretary of Defense who really wants to change that, and the President really wants him to do that. Then it'll happen. Not before.

I: "I had clout." What does that mean? How does that translate?

Herzfeld: You can, if you have a connection open, with good confidence, go up the chain of command 'n' say, "I've got a really big idea I want you to let me do." The impact of some of the strategic balance things I told you about earlier were when I was there, and Jerry Wiesner was Science Advisor to Kennedy and to Johnson. I knew Jerry very well. We were very friendly, as well as everybody in the Defense Department. And, boy, that helped a lot. I would see Jerry at a meeting and after three minutes—I would say, "Hey, Jerry, there's something hot coming down your way. Please, bless it if you can."

Blessings came and flowed. So, that's how government's a human construct and that's how human constructs work. They work on the confidence of people at places, among each other. That's how you get stuff done. If the confidence isn't there, you can't do much.

I: Anything that surprised you?

Herzfeld: About DARPA?

I: Yes.

Herzfeld: Yes, that it was really possible to do big things. It was really possible and that I personally was able to do that. That was surprising. It is a huge surprise now, looking back on it. I'm amazed at what we did. And, in fact, Ruina sent me in an e-mail when he said he couldn't come to my eightieth birthday party—that he was still amazed at how young we were when we did all these things. We were kids in our thirties and forties and did okay.

I: An amazing picture of Johnny Foster. You were there.

Herzfeld: Of course.

Harold Brown once asked me when he left DDR&E to go to the Air Force. He and I knew each other well and were very friendly. He said, "Charlie, what are we doing here—what is it going to amount to in 20 years? Will it make

any difference?"

And I thought about it and said, "Yeah. Yes, Harold. What you've done"—and this was Harold's doing, in my view. Still is. He accumulated a number of very bright, aggressive, smart, serious people, maybe 20, maybe 30 over the years, who worked through the system and who would change our national defense posture. It was a national asset that he created that—wanting to have very serious people in the jobs he could affect. And he did. Marvelous bunch of people.

I: Do you think that there's something like "Gosh darn. I should have done that?"

Herzfeld: Not a lot, no. I'm not saying I'm satisfied but it was better than anyone expected, in retrospect. That'll have to do. Can you think of better? Yeah. I had some things I was trying to do that didn't really work but it happens all the time.

The positive list is long enough and big enough, the rest doesn't matter.

I: No regrets.

Herzfeld: Oh, regrets. I could've done some things a lot better and more smoothly. I was naïve about a lot of things. I was occasionally too aggressive when that didn't serve my purpose or anybody else's. It was in my nature then. I've curbed it a bit, I think. I'm not sure about that, but that doesn't matter either. You know, I do what I do. Can't change that.

Life is the only experiment you can't do over. And the way you did it the first way down, that's it.

I: DARPA does big experiments.

Herzfeld: Yes.

I: Any where you said, "Wow! That was a big experiment"?

Herzfeld: Oh, yeah. When I was able to show a House Committee on Appropriations a movie of the radar screen of the Kwajalein radar showing how the chaff—the needles of foil—Mylar foil deployed in space half the Earth away, and you could see the cloud grow and do stuff, tumble and so on, and they said, "Wow." And I said and I said, "Wow," that was something really big.

And the fact that it didn't get into military hardware as such in that decade is totally unimportant. It was part of the picture that made this country strong and healthy and confident and had the Defense Department work well with the Congress, even though there was serious opposition about some of it, and some of it was just political.

Another one was one of the things we did in our social science program, important, was game theory. We really made a major push in game theory, which is the theory and then experiments with games that are particularly non zero-sum games. Chess is a zero-sum game. One wins; the other loses, period. Real games in real life are non zero. They can both win, they can both lose. Maybe two—more than two sides.

And I took it into my head to explain game theory to the Congress at one of their hearings and people said, "You don't have to."

I said, "I want to," they deserved to understand this because it's important. And I took my shot at it and Mr. Mahon was Chairman of our committee. Mr. Mahon from Texas, he was an enormously influential congressman.

He pursued this as I told them and he asked questions and questions. And I finally got to the example, which was the easiest to explain, of labor-management dispute, where they can both, if they work together, ruin the company and then they're both out of jobs. But they can make the company stronger if they work together, or one can really win, and the other really lose, or they can *both* lose.

And Mr. Mahon says, "Oh, Dr. Herzfeld, that's just like politics— isn't it?"

I said, "Yes, Mr. Chairman. Exactly like politics."

That was one of my prouder moments. (Chuckles.) Very complicated issue, very abstract, very important, and the man got it exactly. That helps with the Congress when you do that. One more note on that. I learnt that in almost my first ARPA testimony—I testified before, from the Bureau. Nothing that interesting or spectacular.

But I realized I was giving a scientific seminar. That's what they wanted. They wanted to understand the underlying stuff that made what we were trying to build what it was. And I realized in the middle of testimony that, "This is going like a seminar." My next thought while I was talking was, "I know how to give seminars. This is working." (Chuckles.)

People are fascinated by understanding what's inside the box, and I think people who hide what's inside the box almost always make a mistake. You don't have to tell everything about everything all the time, but you have to open the lid enough so they can see in the box. Tell them about the moving parts and then you can close the box again. But they will have the feeling that somebody has really tried to explain the real thing to them.

I: DARPA is defining a new territory—asymmetrical enemies and things like that—terrorism.

Herzfeld: Asymmetric?

I: Terrorism. Can they play a role in that?

Herzfeld: Absolutely.

If people would let them play a big enough role. If it's engineering only, I'm not terribly excited about it. The terrorism problem is not an engineering problem. It's simply not. It has to do with what's in people's heads and what's in...in their heads for a thousand years. And we'd better understand that. Otherwise, what we have now is that almost everything that terrorists say is totally ignored by everybody in American culture. And that's deeply wrong.

They say things because they want to *say* them—and not only saying it because they want to manipulate us. They're telling what's in their

heads and their hearts and we'd better understand that. It's totally understandable. It's not always obvious but it doesn't take a lot of study to get there.

You know, there're lots of people who are experts in this but they're not listened to much. So, yeah, that's a good role.

By the way, another kind of asymmetry—all warfare is asymmetric. Always—always has been. Two enemies are never alike—they're always very different. So, they were applying asymmetric warfare as if it were a discovery, not to what's going on now just shows to me that people haven't understood what they've been doing all their lives. Warfare is *always* asymmetric.

Have a demonstration. Start two soldiers, one on each side, or two fighters. Start with the boots. They're different. What are the socks? They're different. The pants, they're different. The belts are different. The weapons are different. Their training is different. Their training manual is different. It's *all* different. It always has been.

The only time you get the same is in a civil war. In the American Civil War, the—the southern army was very similar to the northern army in a lot of ways. And that's what made it so bloody—they understood each other. They were working from the same script.

I: You said your time at DARPA was the time of your life. Why?

Herzfeld: I've never had so much scope and such resources and such programs and such colleagues before or after. I think that sort of says it.

I: What kind of future do you see for DARPA for the next 50 years?

Herzfeld: Either it'll atrophy, in which case it has none. And there're a number of forces that are working on making it atrophy. All the bureaucracies are trying to kill it, because bureaucracies hate new ideas and DARPA is a new idea machine. Fifty years is too long. I can't predict that far, but if it doesn't atrophy, and if the people who own DARPA wake up to what the real problems are then what DARPA does will be very different. I think it'll have much more to do with people as a source of a solution, with other people as the source of problems. How can you affect people? Not by external manipulation. I think that's there, but it's not that important—but by understanding them, by talking to them. How do you talk?

How do you pressure them? I mean deterrence is a pressure game, always. "I'll hit you if you don't behave."

And you say, "I'll hit you back harder."

So, I think some more, and—and so on. And the theory of that behavior is game theory. So, how do you apply game theory really in an intelligent way hasn't been done yet?

Biology is the physics of the century, you know. So, there's a rich series of problems. The brain is the—kind of the nucleus of the century. Atomic bombs. Well, the brain stuff is the substance.

Will it last that long, 50 years? I don't know. I think no one can tell. This is predicting science, and that's always dangerous—and for two reasons.

Scientists are very conservative over the short run and very uninventive over the very long run. People who make the best predictions about science far out are the science fiction writers. They're usually much more on the ball. Jules Verne had the answer, not Lord Kelvin, if you look to 1900—a time I *almost* remember—but not—(chuckles)—a lot of those things that they put up when I was young.

So, 50 years is too hard. If I were a science or fiction writer, I'd be more forthcoming, but I—I think that the nature of what a country means is changing. I think the nature of what winning means is changing. I'm thinking about that a little bit, about how the big revolutions happen. Mostly because people lose interest in the old issues. Walk away from them and say, "I want to worry about something else. I really don't care anymore about this. That was my father's problem."

And it's very hard to predict that. I'd like to think about it more. I'm reading a number of things now related to that. Reading a life of—a book about —Julius Caesar, who destroyed the Roman Republic, deliberately. And he did because most people didn't. He succeeded because most people didn't care anymore. They got into some 500 years of misery.

I'm reading about the great Presidents—which Presidents have handled well the modern problems that changed everything and for a long time. The "Long War." Eisenhower was one, clearly. Curiously, Reagan was one, during the Cold War. And it's hard to find out what Eisenhower did as President. Or, I've not read enough about it.

There's a wonderful biography of his life as a soldier and all the stuff we're talking about came out of his Presidency. And it was absolutely astonishing what an insight he had into the long-range forces at work. He was a very intelligent guy, contrary to what I thought and many others thought when he was President. I thought he was a bumbler, didn't know what he was doing. Boy, did I get set straight.

But I had the good fortune to get to know people who worked with him, that worked for him, and he was quite a different man than I thought.

So, there's a lot of thinking to be done of what are the lasting qualities, problems, solutions, attitudes when almost everything changes. Now I am beginning to get a sense of that because I've been around long enough. And life today is very different from when it was when I was a young man. And I know it and I'm astonished that I'm still around—and delighted, I may add, that I'm still around. But it's *different*.

And what I see a lot of people doing is taking what was great in our day and saying the future is like that and I just don't believe a *bit* of that. The Cold War ended because of a number of reasons but the main thing is that the Soviets stopped believing in their cause. They just didn't care anymore and walked away from it by that time.

Soon after they walked away from it, we walked away from it, too. And by the way, I've always thought the Cold War was an issue of who quit first, that nobody's heart was really in it forever. And for a number of reasons not fully articulated yet, that—that's how that went.

What we're in now is a Reformation in Christendom playing out in Islam. Islam is reinventing itself and that's going to take a while. And our future hangs on how that comes out.

Nobody's thought through that enough for my taste. I know I haven't. But that's what is, in fact, going on. It's an argument within Islam, and we're trying to steer that, not very successfully, because we don't take that point of view. People say that, but they don't really think about it enough to understand what they're saying. It's a fashionable saying. I'm not the first to say it. I may be the last in the sense that I'm—I don't plan to go on talking about it a lot, because it needs work, needs thinking.

So, what will DARPA do in all that? I really don't know. But I'd like the experiment of sitting down with a few friends and saying, "Okay, if we had all the things that Eisenhower gave ARPA, what would we like to have him put on our project list? What are the big things we ought to work on?" And not a lot of things that are just a carryover from the last 50 years, because that's likely wrong.

Because what they did—I mean they—the first list was all new stuff. I mean other people had dabbled, you know, but it was all new. There wasn't one 1918 problem on that list—not how to make the Navy bigger, you know, battleships bigger. It wasn't on the list.

So, it's a good question. It's a wonderful challenge, I think. And if I was king, I'd start working on that right away—(chuckles). Think about what that list should be like.

I: Thank you.

Herzfeld: I really enjoyed it. Thank you. Good.