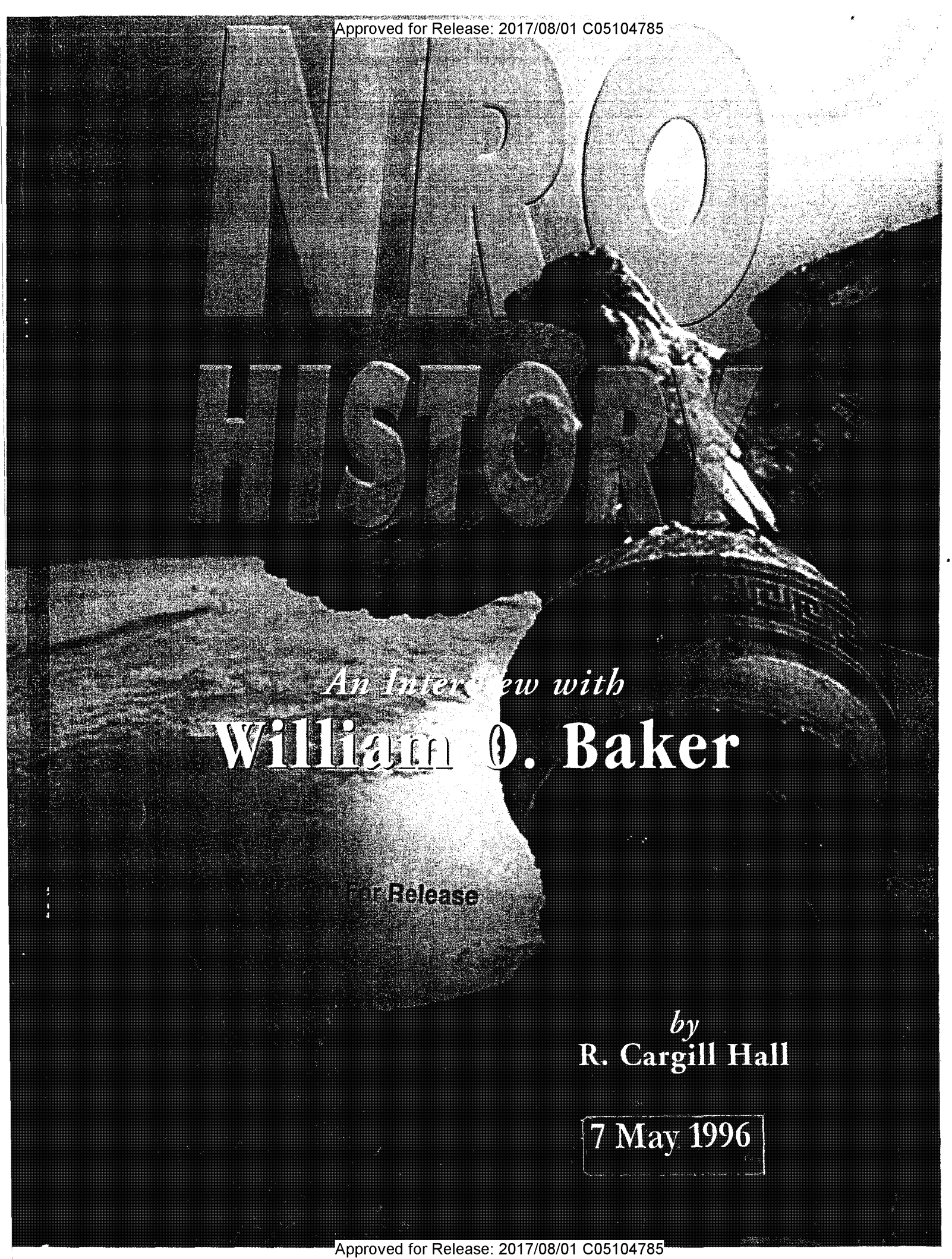

NRO HISTORY

An Interview with

William O. Baker

by

R. Cargill Hall

7 May 1996

NATIONAL RECONNAISSANCE OFFICE

ORAL HISTORY PROGRAM

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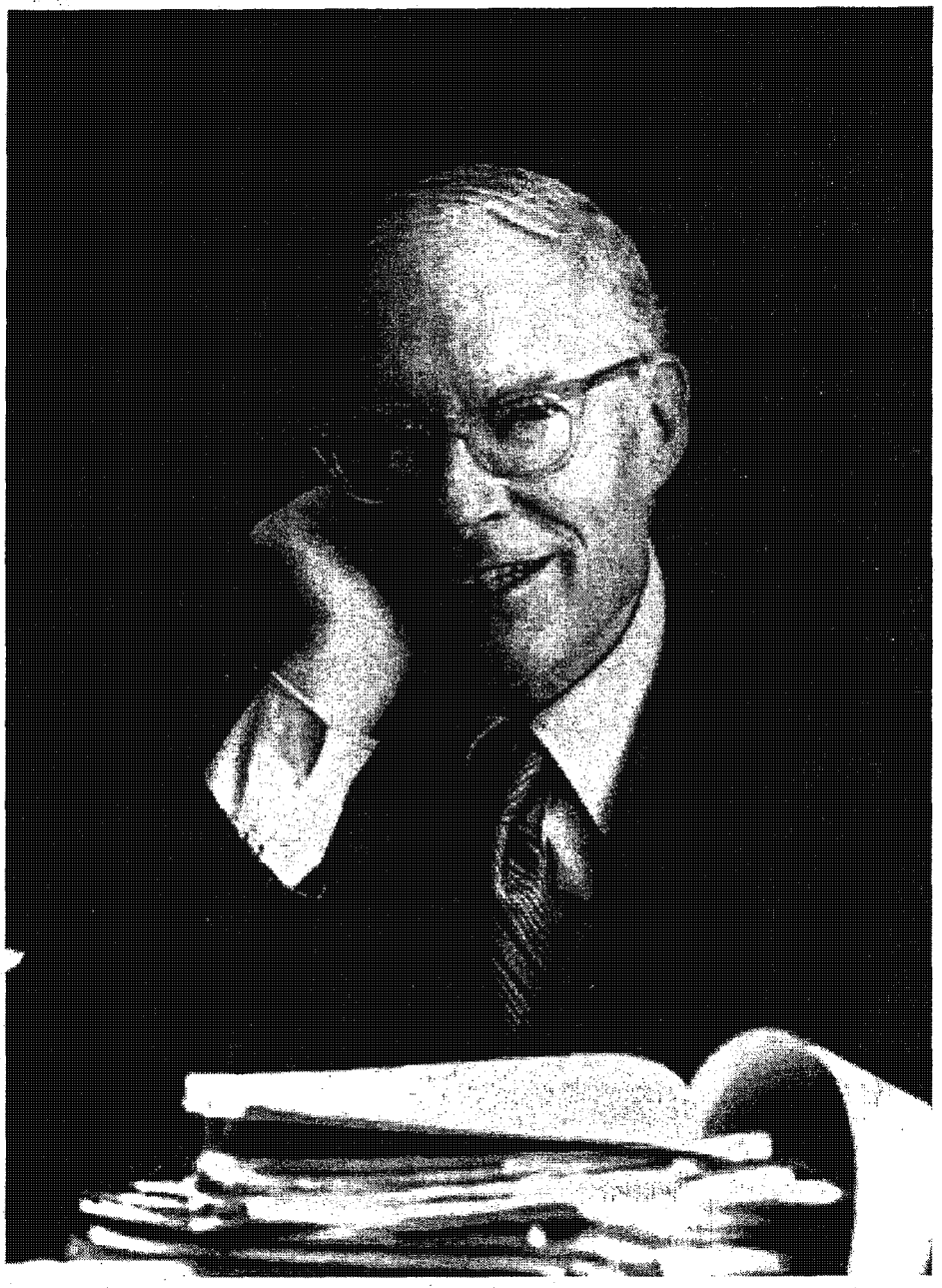


William O. Baker

PREFACE

I conducted this interview a few years ago with the former President and Chairman of the Board of AT&T Bell Labs, who served as a science advisor to Presidents Eisenhower, Kennedy, Johnson, Nixon, and Reagan. Our discussion was wide-ranging, though focused in general on the issues of improved intelligence collection during the Cold War. Dr. Baker nonetheless shared his views on numerous related matters including early science advice for President Truman, the decision for thermonuclear weapons, President Eisenhower's seminal Technological Capability and Signals Intelligence (SIGINT) Panels, the evolution of the President's Foreign Intelligence Advisory Board, human and instrumented space flight, and advances in communications, materials and solid state science and technology that made possible overhead reconnaissance systems, among other subjects. His recall of contemporaries and their contributions in various disciplines helps illuminate a fifty-year period in which intelligence collection changed dramatically.

Dr. Baker granted the NRO permission to print this interview. It is UNCLASSIFIED.

R. Cargill Hall
NRO Historian
20 April 2001

PROCEEDINGS

R. CARGILL HALL: Let us begin with a general discussion of your post-war efforts here at Bell Labs—some of the things you were involved in and how you came to be approached to work with President Dwight Eisenhower.

WILLIAM O. BAKER: Yes. That, of course, was much after our regular war work. And we had a kind of continuum in that President Truman recruited my superior, Dr. Oliver Buckley, as the first science advisor. Neither President Truman nor Dr. Buckley had any very clear convictions about what they should do. They had a very amiable relationship but there was no real projection of science and technology, indeed, into national security or general affairs. President Truman, of course, was very alert to the fact that the Soviets were hard at it. And we had a little adventure, stimulated by Secretary of State Dean Acheson and President Harry Truman, which a number of us here pursued—one of the more visible members, for example, was William Shockley, who later helped invent the transistor. But Secretary Acheson was the real element in it, and the group estimated for the President when the Soviets would get a hydrogen bomb. And this study was done by this little group that we called Project 18, which was essentially detached from the Manhattan Project. We did specific things there—as a matter of fact, James Fisk, another one of my contemporaries, predecessors, had an independent vision and a report—it's all heavily documented, actually, of the chain reaction possibilities of uranium and fission. Like about the time Dr. Peirls was doing that in Britain.

We did things about uranium isotope separation. We had not been in the weapons field at that point. So this group was selected and it is relevant to your general mission because it alerted a number of us to the challenges of intelligence and technical assessment, which were very clearly going to face us. Acheson said that he had consulted Oppenheimer and others. Our report is in the archive—Herb York knows something about that, too—later—much later—Herb got interested and we made a lot of it available to him. At any rate, on the basis of what we knew about the Soviets—about their whole technical base, about their whole industrial capabilities, about their atomic and particle physics alike—we asked: how soon are they going to get a hydrogen bomb?

What we did then was see if we could design one. And they said—they being Oppenheimer and his advisors—that we came very, very close, so close that Truman decided that he had better deal with that, and you remember he earlier had met Soviet leaders at the Potsdam Conference, and so forth. But he realized that eventually they [the Russians] were going to do it.

H: What year did your group come together for Project 18? About 1950 or '51?

B: Yes. Yes, just about that time. And we can dig out other papers, although the total—I have a fairly good documentary background—the total findings are still buried deep somewhere in what was then the AEC [Atomic Energy Commission, Department of Energy today].

H: Probably still classified.

B: Yes. But your timing is quite right. This is relevant because it not only alerted the White House to the things that were shaping up, but also, because it indicated that in the Cold War total technology was going to be very central, it was going to be very dynamic, that we'd better get our minds on it. Now Eisenhower was entirely alert to this, but he did not at first feel—and had no basis, really, for feeling—that he was going to launch any new initiatives there, you see. But what he did was to assign the people that knew about this and that were concerned about it—especially Isador Rabi at Columbia—to the science group in the Office of Emergency Management. And Eisenhower said, "I've got this group which is supposed to be forward-looking with respect to defense and challenges to national security of the USA, so I will have them be responsible." And he appointed some of us—additional ones—we had been interested in that, but we hadn't been active members of it—but he appointed several of us to that Emergency Management Office group.

H: Now this was shortly after he was elected, in late '53, somewhere in there?

B: That's just about right. And we then had meetings. They were very relevant concerns. George Kistiakowski was interested in some of this stuff. And, of course, James Killian. Killian was not in that group at that point, but he was interested, too.

H: And your group's mission was to . . .?

B: Was to respond to the challenges to the USA's security. Incidentally, at one stage, it was called the Office of Defense Mobilization, the ODM—I referred to that as the Office of Emergency Management, you see. In the ODM part of the thing, in one of the chairs was a general named Lincoln, who was very good. They knew, thanks to Truman's interest, you

couldn't trust what went on at Potsdam and you couldn't trust Stalin, and that there was instability there. I don't know what the details were. So they were alert to that.

Okay, that ODM science group met fairly regularly in the Executive Office of the President, and it tried to see what the challenges were likely to be. This is all relevant to your point of when did President Eisenhower get deeply into the game? And he was alert, as I said, and sympathetic to this ODM office, and he got regular reports from it, until, of course, Sputnik. Now Sputnik really did turn him on, as you know. And Eisenhower had had a group that was looking ahead due to things that Truman had seen about threats to the country, and this group was not attached very formally to anything, but was part of the interest that the Security Council had at that point. It was aware of Soviet missile work, of nuclear work, of various indicators that the intelligence community had received, so that when Sputnik went up the President had this background of using non-official or, you might say, civilian or extra-official technical people as consultants. Killian was an important one of them.

H: Now I believe he had been brought in—he had served with Lee Dubridge on the science panel in the Office of Defense Mobilization and Eisenhower had asked them to set up the Technological Capabilities Panel in '54 . . .

B: That's right.

H: . . .to look at some of these issues; and so they were separate from the group you were in with Rabi and some of these other people.

B: Yes.

H: And they came back with that TCP report in early '55.

B: Exactly right. Now you see, that panel recognized the need for a lot more information. They knew, owing to some other very good work at Fort Meade, that SIGINT particularly had derived information vital to the US. They didn't know what the future of SIGINT was going to be. And actually there was a fairly strong move, which I am sorry to say, some of the military were sort of committed to—to push the effect on intelligence down and push the production of new weapons up, you see. It's understandable.

Yet the President and this TCP and others—realized that we needed this technical information and that the Soviets would probably do something unexpected and fairly ominous—they weren't quite sure what. So that happened, with the hydrogen bomb, and then the whole thing went overnight to a very high concern in President Eisenhower's mind. Now Andy

Goodpaster was a major element in that; but he [Eisenhower] saw—he had a wonderful balance between what we really needed in weaponry and what we needed in information and intelligence. So then you remember there was this notable speech that Eisenhower broadcast to the country after Sputnik. That speech, you know, I think you made a good reference to that in some of your earlier sketches, but that will become—should become a historic element of very high importance. I'm reminded of this when they gave us a little prize a month or so ago at the AAAS [American Association for Advancement of Science], but it caused me to go back and read this speech and really relate it to what's happened now.

And what Eisenhower did in that speech essentially was to outline the role of science and engineering, not only in national security—which, of course, was his first agenda item—but in the whole progress of the country for the next 30 or 40 years. It's all there. Now, of course, Killian had a big hand in appraising that.

H: In 1956, in your biography, you point out that you were then called on formally to assist the President and the science people with national security problems. Can you tell me who approached you, and how you came to this assignment?

B: Yes; this was sort of through the Technological Capabilities Panel and my colleague Jim Fisk, who served on that panel. And what they said was well, SIGINT looks like the only way we're going to learn anything significant about the Soviets, but it's sort of hopeless because you haven't made any progress and there might be some kind of new ideas that would come out, so let's put together another group that is dumb enough or ignorant enough or whatever so that they wouldn't know how difficult it was, and see what they could recommend doing about it.

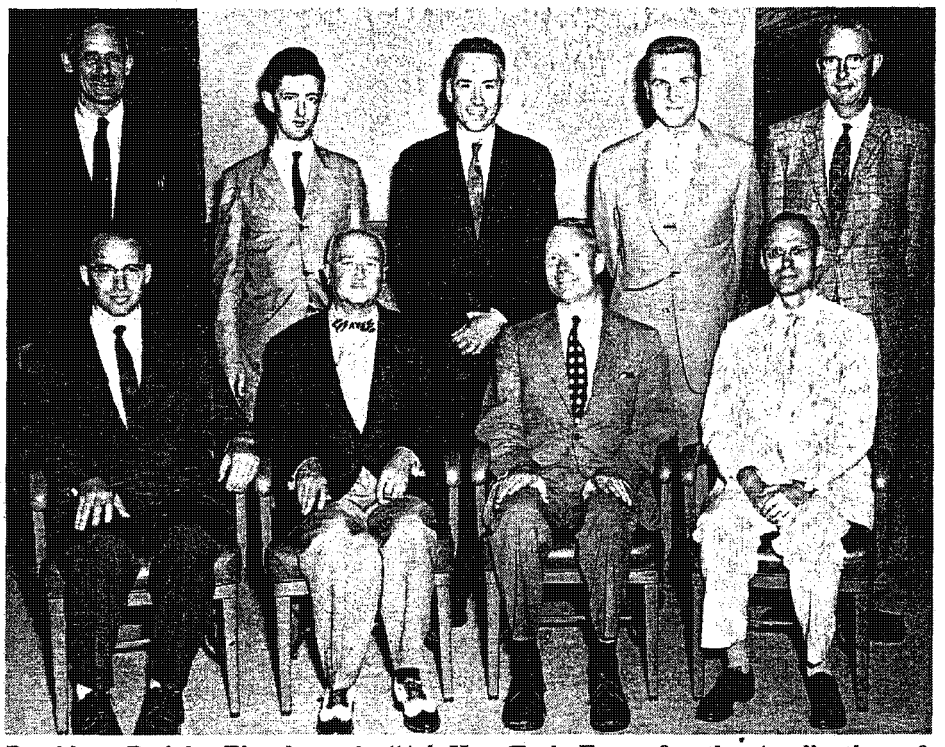
And this was very much the philosophy—they were perfectly candid about it, of course—and we had a great time. But the President called for us to produce suggestions or whatever of interest, and some of the great names in the NSA [National Security Agency]—but particularly Friedman—who was a very nice person and his son actually came here and worked with us quite a few years later—said "what are you going to do? What can you do?" I mean, those people were very thoughtful and very clever and they'd done everything they could think of, they thought. So the group that the President then urged us to put together was formed in that context.

H: And who were in the group? Yourself, Dr. Fisk?

B: Yes. You want to know who it is—who they were?

H: Yes.

B: I should have gotten that out before. If I can find it, I have a picture of them all (see illustration, below). There aren't too many records now. My papers are not that well organized. The SIGINT group took about a year to do its job, and operated between 1956 and 1958. Arlington Hall was the place where a lot of these things were accomplished. At any rate, we then pursued studies that were eventually sent forward first to Allen Dulles and John Foster Dulles as well, the Secretary of State, and then of course given in person to the President.* And what we proposed was a rather different program of cryptanalysis and SIGINT collection, and this of course begins to get to your point of how the collection issue evolved and we proposed—we didn't have much knowledge of satellite collections at that point—but we proposed some fairly modern ones.



President Dwight Eisenhower's "Ad Hoc Task Force for the Application of Communications Analysis for National and International Security," circa 1957. Left-to-right standing: Henrick Bude, Oliver Selfridge, Nathan Rochester, David Huffman, and Luis Alvarez. L-to-R seated: Richard Garwin, William Friedman, William Baker, and John Pierce. Absent: John Tukey

The Navy, of course, was fiddling with its little satellite at that general stage, just around the time of Sputnik, and the National Science Foundation, as you probably remember, was also interested in the satellite model or its equivalent.

*The group carried the imposing overt title "The President's ad hoc Task Force for the Application of Communication's Analysis for National and International Security." Known informally as the SIGINT Committee, the group submitted its report to the National Security Council on 23 January 1958. The "Baker Report," as it came to be termed, recommended *inter alia* accelerating and improving the collection and processing of SIGINT, altering collection priorities and responsibilities, increasing cryptanalytic research, and adopting selected organizational changes at the National Security Agency to effect these recommendations.

H: The IGY [International Geophysical Year] business.[+]

B: Yes. And we pointed out certain things to the President, and I think at that point, after a certain amount of shock or reluctance, or whatever, we got the NSA more and more excited about the fact that at one end the new computer and computational capabilities, along with language studies and the beginnings of the digital age, were coming on strongly and, at the other end, these collection possibilities were very real, and so the NSA became very cooperative. We had a whole series of NSA directors that mostly were responsive, indeed. All during that time, we did attempt to inject into cryptanalysis as much of the new digital technology and the solid-state circuitry as we could. The first transistorized computer, you see, had not yet been put together. It was put together by people here at Bell Labs, and was called TRADIC under Air Force sponsorship a couple years after our first report to Eisenhower. But it was clear that the whole field was expanding very rapidly and we would have an opportunity to create very compact collection systems—processing systems on the one hand, and on the other hand, very good possibilities of analysis—cryptanalysis. So this took a multi-dimensional form, embracing the very new frontiers of solid-state circuitry, electronics, and, on the other hand, the propulsion and space-based work which Sputnik had driven us into, which the Air Force already had a very good feeling for and had done a lot of work on.

H: About this time, at the Woods Hole Nobska Study in '56, the plans began for the undersea cables that would be used as kind of a trip wire to tell where the submarines were passing around the world. You are acquainted with that development?

B: I'm not quite sure I'm following, what. . .

H: This is where the Navy laid the cable, but it was the undersea cable. . .

B: Oh, you mean the SOSUS system? I thought you were referring to some particular element of that. I think I will go get that archival volume that I meant to bring in. Maybe I can find my photograph with it. At any rate, that effort started quite a long time before in terms of submarine detection, submarine systems warning, particularly during the second world war, and then the SOSUS domain was outlined later in the '50s, as you say, and became a major issue of our work, which is discussed in this volume. I got into that quite early—I didn't have any particular understanding of how important it would be in strategic intelligence, such as

[+]A few years later, in 1959, President Eisenhower approved and in 1960 the Navy launched the first successful SIGINT satellite, known as GRAB.

the Strait of Denmark, or the other things which we implemented up there. But rather, a fellow named Mason here, was so good at signal detection in the water—compressional wave detection—that he initiated some submarine detection and protection systems. We wanted to install these in battle craft with underwater domes, and I got into this through the fact that we had to match so-called **P** (rho) quality, which is the velocity of sound and the density of the medium of our detector, with that of its surroundings, which might have been a steel ship, you see. It was quite a challenge for materials. We had a little installation, all very secret, up here at a lake, and we did finally succeed so that most of the domes or installations in the submarine and larger craft of the latter part of the war—but particularly when the strategic submarine business got going—were made that way. I'm not sure whether this reflects on your interests.

H: Those were developments made at the end of World War II?

B: The first of the submarine detection was in World War II.

H: But the domes and things to which you refer . . .

B: Well, that extended much after the war, because what went on there is all relevant. When it became clear from Soviet information—and particularly the exploitation of SIGINT—that they had a very extensive missile threat, then the use of our strategic submarine Polaris system became very vital, you see. And that promoted a need to detect their submarines and to defend against them, have ours equipped to respond to what was going on, which became very compelling. So yes, we then combined those functions. I remember very well President Eisenhower's first exposure to a Polaris launch. We happened to be meeting at Camp David, which at that point he often made available. We used to go up there a lot because he was really very interested in the details of how this defense technology was coming along, you see. But anyway, they had him up at Narragansett Bay when the Navy had the first launch ejection. Again, it was one of those things that nobody believed would work, you know how that is.

H: How can you pop the missile . . .

B: Out of the water. Eisenhower, when he came back from being up there, was shaking his head, and he said, "I got soaked." They got him so close that he got drenched, you see, when this thing broke water. Just one of those little businesses that one remembers. But it did work. We had a long connection with that stuff. But not so much now along the lines of your first point about submarine detection and the tracking systems—as it was how you could construct missiles which would function in a submarine environment.

H: Now you joined the PFIAB, [President's Foreign Intelligence Advisory Board] as it eventually became called, in the '58, '59 period, I believe?

B: Yes, when he first set it up. As you say, it wasn't called PFIAB then, but it was . .

H: Board of Consultants on Foreign Intelligence Activities, I think it was [PBCFIA].

B: Yes. Now, you see, Killian was the primary factor there, too, because the President recognized that he represented a continuous transition from the Technological Capabilities Panel to this intelligence group. At the same time, he was setting up PSAC [President's Science Advisory Committee], which had a broader science advisory function. He was very alert to the fact he had to do both these things.

H: The role of the President's Board of Consultants was primarily to review the work of the various intelligence organizations and make sure that new technical data was being properly applied?

B: That's right.

H: I know there was a big change at this time; for example, the CIA [Central Intelligence Agency] and Allen Dulles had to move from the old OSS-HUMINT approach of agents jumping out of airplanes with a backpack to suddenly employing technical collection systems that operated overhead.

B: Yes. It was a transition that they claimed they have never recovered from. Because they thought that jumping out of the airplane was really the only way to do it. And Dulles was very clever on what he was running then. I think most of this has been fully released, but you would know about it, anyway—there were things like this tunnel in Berlin where they tapped into Soviet communication lines. The CIA tapped into it, but that sort of thing was about as technologic as they felt they could get, you see. They were very proud of that.

H: From what you were telling me, President Eisenhower was keen on seeing that new technology was going into the intelligence agencies and seeing that they were making first use of it.

B: You're precisely on beam—quite right.

H: Tell me, I get the profound impression from reading the documents and talking with people that James Killian was extremely influential in his dealings with Eisenhower.

B: Oh, yes. You see, Killian was a classic—the classic case. When we were talking about those days, he translated science and engineering into policy terms that Eisenhower just

absorbed, in which Eisenhower just delighted. He just felt very comfortable with it. We haven't had much of that since. Kistiakowski did learn a lot of it and did do some very good things of that sort. Dubridge—well, it began to taper off. Both the President's and the advisor's counsel tapered off—went down to zero after a while.

H: Yes. Indeed. And how did you perceive Edwin Land's contribution to all of these activities?

B: Oh, yes, well, absolutely primary, of course. Now Din had been very much interested in the Technological Capabilities Panel and led it's intelligence sub-panel, but he was not so much interested in the SIGINT part and never really—he wasn't part of our group—I'll get you that photograph eventually—but SIGINT just didn't happen to interest him a great deal. But photo-collection did, and the idea of imaging as a supplement to—as a signal in itself—as a supplement to whatever we got electromagnetically, interested him a great deal.

H: You could marry the two; you could get indicators from one area, then confirm it with the other.

B: Yes. And the imaging was to him a very natural form of communication—a very immediate form of communication. On that basis, he was always uneasy about the primitive imaging systems we had, where an agent will carry cameras around, or maybe conceal the camera in something, you see. Accordingly, he was very interested in air transport of cameras and thus when the U-2 issue began he became a very strong element in the U-2 part, but he didn't go into the aeronautical structure so much. Richard Bissell did that, and he went busily around getting people to do things—we produced a coating that was radar invisible—not quite, but almost.

H: The beginnings of Stealth technology, then.

B: Yes. We did recognize the Soviets would catch up with us but we did that for many years, or several years, with a very low reflection coefficient. But Land kept the real essence of imaging going on that. He encouraged Bissell to get the right platforms—the right structures—in place at the CIA.

H: So Bissell became the prime mover at the Agency in terms of convincing Dulles that we can apply these overhead systems. We can get information back from them. Then, of course, you had Project CORONA following after Sputnik, and you have almost a cascading effect with these new technical collection systems coming on line.

B: Yes. Yes. Bissell was a leg man in the best sense of the word. He just went around constantly doing everything he could to get that going. When events later got him all tangled up with Cuba [The Bay of Pigs], that was a mistake. That just wasn't his pitch, his thing, you see.

H: Land used to call it the "dirty tricks department" and I have the impression that Bissell kind of lost the confidence of Killian and Land after that.

B: Yes. Yes. And it wasn't altogether his fault. What happened—you implied this very well a little while ago—involved, the culture of the CIA, which favored jumping out of the airplanes and so on—that was very strong, and they figured well, Bissell has done all this good work on the U-2, and CORONA, he must be good at everything. So they tried to project him as their cultural element for Cuba. And that was a mistake.

H: Indeed. Now one of the people that moves in and out during this whole period with President Eisenhower, is Jimmy Doolittle, from Shell Oil Company. And he's on one panel and then on another, and his role in all this has always mystified me. You've tempted me on the phone by saying "well, I worked with him helping to set up the NRO and things of that sort." Could you talk a little bit about his role in intelligence and how you came to work with him?

B: He was highly respected by President Eisenhower, of course, as he was by everybody. His interest in aircraft and air operations and how you got technology applied was continuing and eternal and he'd done all this stuff over Tokyo and so forth. He was a tremendous patriot, and the result was that the TCP deliberations interested Jimmy a lot and we said, of course, well, now he's got a very good idea of what aircraft can do and can't do and therefore, as we on the intelligence board, advisors, as they called us, go along, we just ought to have that kind of knowledge available. And we had a high respect also for Jimmy's objectivity. Although he was very loyal to the Air Force, he didn't let it twist him, you see, and so he became the last word as far as we were concerned on the margin of liability of the U-2. He gave the President very good information that the thing eventually was going to be shot down, and, at some stage, we all joined with that. But he knew something about it. And so that's the route by which we became closely affiliated.

Now, there was a congenial element there. We just got along very well. Jimmy got along very well with several of us in that group, showing he was interested in how new science and technology were coming along, how the frontiers of solid state were coming along, what they would do. You'll find Ed Purcell, I think, feeling just the same way. Jimmy was just a.....

H: Now was Doolittle on PFIAB as well?

B: Yes.

H: He was? He must have been highly trusted by the President, because Eisenhower entrusted him with an investigation—an examination of the CIA in '54. What's good? What's bad? How can we fix it to work better? And among other things he reported that they needed to bring technology in. I've seen the report. Doolittle didn't have a formal position in the Administration. He was really sort of on call, a confidant. Is that perhaps the word?

B: He just wanted to be very free to pursue the things that he felt were vital and the President and other Presidents felt that this was the right thing to do. But Jimmy sat there with Land and me when we invented the NRO. But Jimmy had a balanced view—well, sure, some people are going to say the NRO should have been in the military. Some people would say it should have been in the Air Force. Somebody would say it should have been in the Navy, because they were fiddling around with these little satellites. But Jimmy said well, it's a national resource and let's make it that way.

H: After the U-2 was shot down in May of 1960, President Eisenhower asked Kistiakowski, who was then his science advisor, to conduct an evaluation of the SAMOS program, which the Air Force had, which was really struggling—it was a bandwidth problem along with the limited readout time available over the ground stations, one thing and another. And he and a few other people conducted this. Land got involved—I wouldn't be surprised if he hadn't talked to Doolittle and others about the evaluation. By the end of 1960 Eisenhower agreed to establish what eventually became the NRO. And in that process, you and Doolittle got involved. Can you tell me how that came to pass.

B: Yes. Well, as I said, we really invented it. I can see the scene right now. That we were at a meeting of the intelligence board, that we felt had to move in this field.

H: Now this is PFIAB?

B: Yes. For the reasons we cited a bit ago, we agreed, and Jimmy Doolittle was a very important judgment element in this, that space reconnaissance better not be in a particular military service. It better be a national resource. And we had Artie Lundahl [Arthur Lundahl, Director NPIC]. Artie had already done some interesting stuff. You remember the photo interpretation business.

H: In fact, Eisenhower set up NPIC [National Photographic Interpretation Center] about the same time in late 1960. He wanted it to do the whole thing on photo-interpretation for the National Command Authorities.

B: Yes. That's right. NPIC was an essential element of it. So we said we're going to create a new group. It's as simple as that. Now, those records should be accessible to you.

H: The PFIAB records I haven't seen yet.

B: The story will be in there in some detail. Like I say, I hope they get those out for you—we tried our best to preserve most of that stuff but it goes to presidential libraries and it gets lost very, very quickly. It's very awkward.

H: Well, I think they still come classified with a.....

B: That's a traditional factor, but just the plain discovery of information that's perfectly open or long ago declassified is often very difficult.

H: That's got to be one of my next efforts, to get down again to the Eisenhower library and go through some of this stuff.

B: Yes. But there are record people like yourself, you see, who are oriented well enough to know what to look for.

H: Tell me about General LeMay. In the mid-50s he suddenly got religion and wanted the U-2. He wasn't too interested in it before. How did that.....

B: LeMay was a fascinating case. LeMay, of course, we knew a little about him. I may say, by the way, that Twining and I had to deal pretty rigorously with LeMay when we were trying to get some kind of a useful balance on this persuasion of Kennedy, regarding Cuba because LeMay said, "Oh, there's no question we've got to do advance nuclear testing—we've got to do it right away. You just go tell him we're going to do it." Well, we said "It isn't going to work this way." [Referring to the resumption of nuclear weapon testing.] But anyway—LeMay, we got to know something about LeMay through Shockley, of all people, this is the same Shockley I was talking about earlier, the co-inventor of the transistor. Shockley was a very brilliant physicist—theoretical physicist.

H: How would he know LeMay?

B: Well, because after the war we were looking at the general Air Force—that is, the Army Air Forces combat in the Pacific, which, of course, was crucial. People at that time had asked us to do that. And operations analysis in the broader sense of the word was just starting in,

you see, and it involved people doing training and the like. And Shockley was impressed by the fact that LeMay had a wonderful record, or a remarkable record, of bombing initiatives in the Pacific Basin, but it wasn't nearly as good as it could be, according to Shockley, probably because there wasn't a sufficient level of training. And, by gosh, he got hold of LeMay and persuaded him with very eloquent, simple explanations that he should invest in training those aviators about ten times as long and as much as he had previously. And LeMay thought it was sort of nonsensical—but nevertheless tried it. And he was so pleased that he went in and established that at SAC.

H: It really worked.

B: Yes. And so that's the way we heard about LeMay and we knew this was going on with some of us who worked with Shockley.

H: So he told you then about LeMay in the 50s and what you might expect.

B: Yes. That opened a certain channel to LeMay or introduced a certain behavior in LeMay and it made him at least less abrasive, or less dismissive, with respect to new technology. Now, I'd say that is about as far as it went. I don't think LeMay, for example, had much sympathy for space technology, at least that was our impression. He was, oh . . . tolerant, but not very interested.

H: He, I understand, had tried to make a play to take over the U-2 and get some of these technical assets along the way. Once they started to work, then he was converted.

B: Yes. But he wouldn't have been an initiator or supporter of NASA. He wasn't drawn in for civilian science. As we recognized it, there had to be a NACA, and NACA was going to be changed into NASA, and so on. And we didn't find people—Shriever was a good one of them—who were very sympathetic to that, you see. But LeMay—I think he thought we scientists were interfering with his business.

H: In the early 60s Bud Wheelon came on to work for the CIA . . .

B: Who was that?

H: Dr. Wheelon?

B: Oh, Bud—yes, yes.

H: . . . and he was responsible for really setting up the directorate of science and technology there, putting it on a firm footing, getting it going.

B: Sure.

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H: Now, of course, that shop had reconnaissance assets for the Agency and about the same time Brockway McMillan came down to Washington, I think from Bell Lab.

B: Yes, that's right.

H: And then he was named the director of NRO to succeed Joseph Charyk.

B: Right.

H: Well, at that time the NRO was responsible for overhead collection—you know, all of the overhead systems—and I think for some reason that these two fellows got crosswise with each other.

B: Oh, they sure did.

H: Can you tell me what was going on?

B: I think it must have been a classic case of personality conflicts, because they got crosswise over night and they stayed that way. We were very fond of both of them. I, of course, grew up here with McMillan. He was a mathematician. I worked with him and had him do things, and he was very effective analytically, and very devoted to the Air Force. I think he became an Assistant Secretary later on, before he became DNRO. So his track was right down that line and he felt a deep suspicion of the culture of the CIA, you see. Now Bud Wheelon, I think, felt that he had to live with that culture and he developed his own way of dealing with it, and the result was that they didn't trust each other—I think the distrust was larger on McMillan's side than on Wheelon's side. They're both quite well and quite active now. McMillan lives up in Maine. As you probably know, Wheelon, of course, is very active currently. And it was a fascinating case, because they were both very good and they were devoted to the country.

H: It is my impression that Wheelon was determined to see that the Agency remained involved in building and launching, with the Air Force element of the NRO, these reconnaissance satellites. And McMillan, who was ostensibly in charge of the whole thing, felt that he couldn't get his directions followed down there at the CIA because Wheelon was doing whatever he wanted to do. That's my impression.

B: Yes. Well, I think that's right. Let me be sure I understand your point about directions or sense of direction. You mean that McMillan couldn't even get information from the CIA?

H: Well, he was the titular head of this whole operation and would be the person responsible for funding and approving the new starts and the systems that were coming on, but

Bud Wheelon just kind of took the ball and began running with it, and then he'd find out later that Wheelon had already started a new program. That information wasn't provided.

B: Yes. Yes. This is what did happen.

H: I think that resulted in some of the tension.

B: You're absolutely right. This bothered Brock a lot, but—the big BUT is, and I guess PFIAB has to answer for some of this—we in PFIAB felt a little competition of this sort wasn't a bad idea. We hated to see it, abrasive as it was; and it really did promote bad blood; they really did call each other liars, and the like. But, in fact, we felt that the CIA was making enough progress—and heaven knows it was slow at the time—and moving away from this culture of people jumping out of the airplanes that you cited, and getting into systematic technology and really having somebody there that knew what we could do with it, which Wheelon did, you see. We felt that that was so important in preserving, in converting the CIA, that we didn't land hard on them in these disruptive contentions. Indeed, they did pull some tricks of different kinds that offended and worried Brock McMillan. But we thought well, better go ahead with that and let Wheelon do what he can. McMillan will have a major resource, anyway.

They had to find out what the Soviets were doing, and about the whole missile threat era, and so they were going to do reconnaissance and imaging collection for very clear and understandable reasons. And McMillan was set strongly on that course. And we felt that was going to go steadily along, which indeed it has, right up to the present day, virtually.

H: So, the competition was, overall, not bad—had the Air Force element or the Agency element of the NRO . . .

B: No. They still don't like each other.

H: There was a debate over continuing to pursue—now this was later in the 1960s—the MOL [Manned Orbiting Laboratory] Program. Can you elaborate on that at all—help explain the debate that took place?

B: Okay. We had formed, in the meantime, the Aerospace Corporation. Trevor Gardner and I. Gardner, by the way, was a strong supporter of Air Force technology over all, and missile work and satellites in particular. Of course Bennie Shriever was a major operational part of it—this is all written up. You've probably seen their Aerospace history volume. But some of these details were not known to its author. And we thought one of the things this corporation

should do, was that they should see what the manned space situation really amounted to. We were worried about that. We had all sorts of good new instruments, the instruments were expanding very fast. The charge coupled device was looming up, for example, came right out of this building over here. And imaging looked to be widely vigorous. Land felt that way. But people in the Air Force and some other places thought that manned space flight was the only thing to do. And it was the only humane way to pursue this issue. And we, therefore, saw the MOL being projected as the answer to the dreams of those who favored very much more extensive manned space flight with the intelligence function being a primary element of it, rather than some kind of meteorological or astronomical mission, or something else. And we just could not find what you were going to get out of it beyond what we were already doing in intelligence.

H: It is very much more expensive to put a man into space . . .

B: Oh, heavens, yes.

H: . . .feed and care for him, and get him back again.

B: Overwhelmingly, so. So we had a pretty lively struggle on that. PFIAB took a keen interest in it. I think the NRO did not really get very heavily involved. The passions in the Air Force were so great that the NRO wisely stayed away from the issue, if it could. And this became a cultural theme in the Air Force—at very high levels of the Air Force—vis-à-vis others, and NASA, to some extent, although NASA would have been agreeable if all that money had been assigned to it. PFIAB in particular, said, "well, it's not going to produce better intelligence—whatever else it might do."

H: Not certainly of the kind, and for the cost, that you can get with automated systems.

B: That's right.

H: I think, ultimately, the U.S. couldn't afford to pay for two large systems, one very powerful instrumented system that also was being built and that ultimately was flown. And I think it came down to President Nixon to decide and he called a halt to MOL.

B: Yes. Yes, that's right. We were very, very closely involved in that.

H: Was it not basically an issue of an automated versus a manned intelligence space system—what you can get out of it.

B: Yes.

H: I think, looking forward for a moment, that this is the difficulty with NASA's manned space station. You've got this huge project underway and it's searching for a mission to perform in space.

B: Oh, exactly. It's sort of pitiful, you know. This fellow Walker, a member of Congress, came up here from New York, and he was exploring with the same passion and bias that you are accurately recalling, about space missions without any basis. We had quite a long session with him.

H: Well, manned flight has a lot of drama and all the rest, but I was reminded of it on the TV recently looking at a shuttle flight with this person riding a stationary bicycle while he whirled overhead, and I think people are beginning to ask themselves: "can't we pay him to do it down here on the ground?"

B: Of course. We gave a lot of testimony about 20 years ago, I guess, to the appropriate House and Senate committees that were examining what they thought the future of space technology should be. You can learn all this stuff—not everything, of course, but a lot of it in that testimony. I was very interested because as you probably will come to in some other context—we had been asked to develop some kind of re-entry warhead protection for the missile system, which we did—the ablating shield, but that became a crucial matter in manned space recovery and I was hoping that we'd get a cheaper form of the shuttle or other space vehicle so that at least that part of it would reduce the price. But that really has never happened. That is a trivial matter as far as the bigger question, whether you get anything out of manned space flight, anyway.

H: Going back again to Trevor Gardner, who you've mentioned several times. Now, he was one of the few Eisenhower appointees who left the administration over irritation with the boss.

B: Now this was . . .

H: Trevor Gardner, who had been the Assistant Secretary of the Air Force for R&D, I think. What was his complaint? Eisenhower was doing so much at the time. They had started Polaris. They got the U-2. All these advanced things were going, and he still was unhappy.

B: Well, Trevor, of course, was not long on patience. Trevor was worried, as we all were, about the adequate levels of defense funding and he was very loyal to the Air Force. And I think—I knew him extremely well, and I think he just got frustrated at budget realities—budget

limitations. He got frustrated, of course, to some extent at the bureaucracy in the White House or the people in the OMB [Office of Management and Budget], or BOB [Bureau of the Budget], as it was known then, who said "well, you can't have unlimited money and you've got to use it carefully and so on." Trevor just decided that was unpatriotic.

H: So, really it was a case of his believing that Eisenhower should be spending more?

B: I think it was as simple as that.

H: In retrospect it is interesting, because when Kennedy came to office on the missile gap issue, he soon found out that there wasn't any; they should have briefed him on it before the election. But he ended up having to choose among the defense systems already begun. Eisenhower had so many systems going. Atlas, Titan, Minuteman, Polaris, the B-70. Kennedy had to cancel some of them. People don't realize that.

B: That's right.

H: Anyway, I'm not here to interview me.

B: No, but what you're saying is right.

H: So, in your view, Eisenhower had done a very remarkable job.

B: Yes. Oh, my, yes. Tremendous vision.

H: You served on the PFIAB for a long time, along with Richard Garwin and others. Can you tell me about its evolution from Eisenhower period through the 60s and into the 70s?

B: Sure. We can dig as much detail as you want out of this. But to give a kind of overview, in the beginning, there was Killian; PFIAB was very much a personal resource for the President. Killian met with him a lot of times; Killian had almost—sometimes almost a daily interaction there. Killian had a very sensitive and perceptive view of what that group could do. We had some politicians on it, Darden, the former Governor of Virginia. We had people who had been in the military. We had leaders—Lovett, who had been Secretary of Defense. And there were others of us on the board who were interested in technology. So it was never a narrowly focused group, limited to technology. It was a very sophisticated—leave me out of it—group on national policy and it became an instrument of national policy.

H: But working specifically in the intelligence arena. PFIAB in national policy—but it was in the national security, national intelligence area . . .

B: Yes. When I say national policy, of course I, mean national security, but its activities extended far beyond intelligence.

H: Its scope embraced national security issues in general?

B: Yes, that's right. And that was part of the result of this personal posture that it had which the President wanted and which Killian understood extremely well. Now, it maintained that posture for a good number of years. Kennedy sensed that. Kennedy was enormously responsive. He and Eisenhower did not get along on some things, but he must have felt that that general theme was appropriate. Kennedy liked people. Doolittle, Land and I, he would take us in the mansion, as they called it, the living quarters, and we would go around and talk about national affairs and then security, and I remember one time the President of Finland was there and Kennedy took two or three of us in there to describe to this fellow how the United States got its security, you see, and how we did this and that—and he followed us in. And this was a very congenial period indeed. Now, Kennedy did somewhat the same thing about the PSAC—very much about the PSAC, and so he kind of had a sense of how to join these things. He and Wiesner got along very well.

Everybody has pointed recollections. Maybe I have told you this before—and stop me if I did—but when Kennedy was inaugurated he moved into the oval office and somebody at that point, in Eisenhower's National Security Council, of course—had given him some kind of briefing on his responsibilities in case of a missile attack or other attack, but apparently they didn't put it in a way that a newcomer could follow—after all, Kennedy was surrounded by all kinds of novelty and new responsibilities and so forth, and he didn't get much of it.

So the next day he got Wiesner and me in there, I guess it was Mrs. Kennedy who had brought up this desk which was John Adams' desk—not the one we talked about earlier, but a big desk, and Kennedy said he didn't know what was in it, but he put himself in it. We said "Mr. President, here is a thing which is connected to your responsibilities for BMEWS [Ballistic Missile Early Warning System] and for national responses." Did I ever recount this episode?

H: No.

B: And Kennedy said, "I don't know how to do it." And we said, "Well, there's a phone here, another one over there should tell you what crisis it is," and the news at that point was supposed to give at least several minutes of warning of a missile attack, three or four minutes, something like that, and of course Kennedy got very interested. He said, "Where's the

phone?" He couldn't find it. Kennedy and Wiesner and I got down on our hands and knees and we got under the desk and found somebody had put it in a drawer, and we finally found the darn thing. And then we explained the whole technology, which Kennedy was very interested in, and apparently that, among other things, made a fairly strong impression on him. Because he would get us in there—particularly Wiesner and me—I'll get back to PFIAB in a minute—not so much on a specific PFIAB context; it was on a sort of a technologic context, and he felt he needed that connection. And we would go over his surprise attack problems and early warning problems and so forth, so that when October '62 came around, he felt that we were the ones that he was going to turn to, and as a matter of fact he had me in there during the executive committee—EXCOM meetings you remember that they had, Bobby Kennedy and others—during that whole crisis and I sat back along the wall during that period.

H: Art Lundahl came in to brief Kennedy with the picture boards?

B: Yes. So that was a kind of an outgrowth, related to PFIAB in that Kennedy didn't take the details of PFIAB as seriously, as intimately, as Eisenhower did, but he was very gracious about it and he made it clear to Dulles—that's Allen Dulles—that they should pay attention to it. Now you remember there was a transition there, John McCone came in as a new director of the CIA [November 1961] and, of course, McCone made a big thing of that. McCone didn't understand what was going on, whereas, Dulles did, but Dulles didn't want to see quite as much CIA dependence on technology as we had proposed or as PFIAB was sponsoring.

So this was a PFIAB transition period which extended right through to the Johnson Administration. With Kennedy an awful lot of things happened. For example, in one of these executive committee meetings he assigned me to see whether we could create a national communications network for command and control in the Caribbean. Mr. Rusk brought that up. Mr. Rusk said "It's terrible, Mr. President. We don't have any adequate communication links, even in the Caribbean." Of course that was when the Soviets were in Cuba and they thought they were going to do something about it. So anyhow, Orrick and I undertook that. Orrick was Deputy Secretary of State at the time, and we organized the FTS—the Federal Telecommunications System—on the basis of that, with the President giving us all kinds of support for it.

That was the context, then, when Kennedy was killed and Johnson followed on quite systematically, but did a thing which was rather reminiscent of Killian's role with Eisenhower,

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namely, he got Clark Clifford to be his man with the PFIAB. Now Clifford had come in with Kennedy, and we all got along very well with Clifford, and you've seen Clifford's memoirs and all the rest. But Johnson must have felt—he almost said he did, I guess —that if he had a real politician in there he would feel pretty good about it, but all those technical people, and those diplomatic people—Murphy, who had been Deputy Secretary of State during Potsdam, and all that sort of thing, you see—he didn't pay much attention to them. But Johnson did one thing that was exceedingly pertinent: he took us into his thinking and his inner circle just as deeply as Kennedy did. This is all on your point of how did PFIAB move along. And he took us in extremely.

There's another poignant thing, which I'll always remember, and that was the day that President Kennedy was shot. Clifford and I were having lunch in the White House mess. There wasn't anybody else of much stature there, and we came in to sit down for lunch. There was a nice military-derivative secret service man there, and we said hello to him. After he had finished his lunch, he walked out and about two or three minutes later came back looking as though he had seen a terrible ghost, and he said "The President's been shot." And we then learned from him as much as we could.

That was when the first word got to the White House, but the point is that as regards the PFIAB history, that it turned out there wasn't any real line of command established at that point and we didn't know whether this assassination was something of a military character or of a national security character, so Clifford and I immediately got hold of the Pentagon, and sure enough the Joint Chiefs were not on duty, they were not informed, so we did communicate what we had just heard to them. They, well, quickly set up an emergency command line, which is relevant at this point because you wanted to know about PFIAB. There is a case, for example, where the PFIAB's information and function were directly actioned in command and control and in assuring the national command sequence.

Now, what I was saying was that Johnson did take us into his heart, or whatever passed for his heart, and he would have us over there at lunch. And I remember things about that, too. Johnson of course was a communications specialist. He loved to communicate technology. He claimed his telephone shocked him in the shower once. But there was this little dining room for the President—just a little private one, of course, but it was used a lot during the week, and he would get two or three of us in there—Land and I, in particular—and sometimes he'd have the

CIA director there, or sometimes somebody else. We'd talk about what seemed to be the pressing policy issues, that's where your question about PFIAB comes in again, and sometimes about the technology issues. But sometimes we would get a very poignant feeling that the President wasn't with us. We would be describing something the best we knew how, and time after time what happened was—I finally learned to look—that he was on the telephone. He would sit at the end of the table, you see, and he had a telephone there, and he would get on that telephone, and we would be talking to him about important matters and we thought he was paying attention, but he was on the telephone. Nevertheless, he was very responsive. I remember one time—if not more than once—he'd get us in toward the end of the day and describe what some of these policy issues seemed to be. In that case it was mostly Clifford. I was usually involved there. Policy was a political feature of PFIAB that interested him. But he did share a lot of his feelings and concerns.

Now, the situation changed significantly, but not terribly dramatically, as Nixon came along. Just to carry this through his administration fairly systematically, Nixon got along well with Clifford, and I think maybe for some of the same reasons Johnson did.

H: Clifford stayed on the PFIAB?

B: Yes.

H: And you and Garwin and some of the other people just continued with Nixon?

B: I guess I was the one that stayed most consistently, because I had been kept on—although I was put on the board by Eisenhower, I really had a Republican background. Kennedy and Johnson had kept me on and we had some connections with Nixon during the Eisenhower time. The Nixon connections were sort of tenuous but nevertheless, friendly. And he would come around. So, yes, I can get you the chronology. They do have a little booklet—I can put my fingers on that, too—that shows the membership shift. Maybe you have that.

H: I don't. I'd like to see that because the membership is important through this period.

B: Well, you should have that.

H: Land stayed on it a long time, did he not?

B: Yes. He didn't stay on all the time. I stayed on till Carter, when he essentially abolished it, and then I was put back on by Reagan in '81. But anyway, Nixon carried on to some extent this sharing of deep secrets or of deep issues or of deep national problems with some of

us, which is really a very interesting thing, because, later on, PFIAB lost that connection completely. But this was an element of government, of course, very secret, that these people pursued. You have a sense of this, you see, and how Eisenhower, himself, got interested in the subject. For example, the day before the attacks on Hanoi, Nixon went through a great deal of agony in deciding what he was going to do, and he had us in there to say how difficult it was to decide whether to mine harbors up there, or whether to bomb Hanoi or not to. And he got to baring his soul; he said "I reach out and the people of the country never respond the way they did in Eisenhower's time," and this was again in a PFIAB context, actually, because he wanted to talk about the same policy issues, you see and his way of dealing with them. Okay, that was an example of what PFIAB stuck with. Ford, and Rockefeller came in at this point as....

H: Now, this is Nelson Rockefeller?

B: Yes. That was Rockefeller. And Nelson Rockefeller was very interested in PFIAB and spent quite a bit of time at it. So, when he was made Vice President, he was off and running, you see, and felt this was an essential resource for the presidency, which, of course, he had his eye on rather strongly. So this collaboration that I've been speaking about all this time was preserved during that period pretty strongly by Nelson Rockefeller, as well as Nixon's occasional forays into a very close collaboration.

Now, when Nixon's administration ended, Nelson Rockefeller had a lot more to do, of course. He tried to keep the idea of PFIAB going as being a resource for President Ford. President Ford was very courteous and very kindly, but we never had the connections with him that we had before. To some extent, Ford felt, I believe, that Rockefeller was taking care of that and that he didn't have to attend to it. But actually, Rockefeller didn't really like a lot of the—Rockefeller didn't pay much attention to—the Nixon staff, the Haldemans and the Erlichmans, and so forth, which was good taste, and secondly, when Ford brought these people from Michigan in, he didn't care very much for them, either. So there wasn't really this very intimate affiliation or relationship that we spoke about earlier; it began to decline at that stage.

H: By this time, Nixon had disbanded the PSAC.

B: Yes.

H: So Dubridge had gone.

B: Yes. That's right.

H: So there was a downturn here in science and technology in that regard.

B: Oh, definitely, yes. You see, what happened was Dubridge had more and more difficulty with the bureaucracy and with the national community of science and engineering, including industry. Nixon had this advisor, a fellow named Magruder, who told him—"well, you should be making prosperity out of industries, out of technology, and you don't have anybody that's really speaking for it," which wasn't quite true. But Magruder did get the President's ear. So we were asked, on the same basis that we've been talking about these last many minutes, we were asked, "What, could you do in that regard?" "Well," we said, "we have a fellow, if you want to try that, who knows about industry and who knows about science and is highly respected. That's Ed David." So they appointed Ed David to be the President's special science assistant. Ed David came from here. And Ed came in and worked hard at it. His connection with PFIAB was minimal. He just didn't have time and he didn't have a very close affiliation there, though he was entirely sympathetic. But the whole thing kept going down. So Ed finally resigned. At that point, the President had said he was going to abolish the PSAC and the office of science and technology.

We had done another thing for Kennedy. He had assigned me to say how the science office should be organized after Killian. You see, Killian had, of course, a personal kind of organization there. And so we set up a little panel, which I can give you the full documentation on, which you probably don't want—that I chaired that had Dubridge on it, Rabi on it, and we invented the OST [Office of Science and Technology], which was a White House science office. It was used up to Ford's time, and in Nixon's time. But, you see, Nixon said, I'm going to abolish the whole thing. Well, we pled with him not to do that. Schultz, who was Secretary of Labor then, Director of the BOB, had sort of favored this. Schultz thought we were going overboard in matters of higher education and things like that. And Nixon was on the verge of abolishing it. We said, "Well, Mr. President, statutorily, in this whole OST part—the Director of the National Science Foundation has a role in being your assistant. Won't you let him be your assistant and therefore not abolish this office." And he agreed to do that. Guy Stever, who was a good old Air Force aficionado, took that job for a year or two. Now, Stever, of course, was highly sympathetic to PFIAB, but he didn't have the role or the power of that sort to turn that much attention to it and so it just kept sliding down, and by the time Carter was elected, they assigned one of our colleagues in the OST—a black man, a very nice fellow—to tell me and a couple of others that he was going to abolish it, which he did.

But all I can say is the succeeding role of PFIAB, when it was resurrected, which I do have illustrations for, was much more bureaucratic and didn't have this personal connection, although Reagan was extremely friendly about it.

H: I am struck with the irony that Carter abolished PFIAB just at the time that a new overhead system came on line that was so powerful, the electro-optical system, you recall, just after he took office, in January of '77.

B: Yes. 1977. But you're right. This was part of the irony of the situation. You are absolutely right. And very amusing.

H: There are a few other people that I would like to obtain your observations of their contributions. You earlier mentioned Robert Lovett and General Twining.

B: Who?

H: Lovett.

B: Oh, yes. yes. Wonderful person.

H: Defense Secretary, among the first to arrange with President Truman and the Joint Chiefs of Staff to approve overflights of the Soviet Union.

B: Oh, yes. Yes.

H: And General Twining was privy to this. Unfortunately he is no longer living. Eisenhower picked this up and by this time they had gotten the British involved in doing some of it. At least that is my recollection. These early overflights that took place before the U-2 began—did you ever get involved with them or with Twining?

B: No, not much. We just heard about it and we got some inputs from the findings. But no, we were not much involved. See, the details were difficult, closely held. It became political.

H: Oh, yes, it was very highly compartmented.

B: Brock McMillan, by the way, got his introduction to the subject through some of this activity in that we got him in as an assistant staff person in PSAC in the very earliest stages, long before the NRO had emerged, and he saw some of those results. Lovett, of course, was very sharp on that. He knew what the uncertainties were. And he knew what the probabilities were. And he served with us on PFIAB very effectively.

H: The other person who played a very strong role in the Eisenhower administration and moved up from Assistant Secretary of Defense for Research and Development eventually to

become Deputy Secretary was Donald Quarles, who also hails from Bell Labs, and it is my impression, based on the records, that he was instrumental in fashioning the original space policy for "freedom of space." You could over-fly the USSR with the IGY satellites and other things like that.

B: Well, he had a role in that. That was something, though, that Eisenhower, again, was very personally in favor of and very personally committed to. But Don did have a role, and I think he had a very foresighted understanding that this would become a world capability. It would become a kind of new culture and frontier in space and, therefore, we ought not try to compartment it, limit it, or shrink it, exclusively to some weapons system arena.

H: What kind of person was he?

B: Well, Don was a fascinating fellow. I worked with him here for a long time. Electrical engineer, essentially, who was able to see the operational, political—and to some extent social impacts of technology, of engineering, and of how you got these things to work to serve public purposes. Now, a great deal of his work involved telecommunications, making telephone systems function. He had a strong sense of quality assurance, quality control—which was very good—what I'm talking about is a special doctrine of quality assurance which Shawhart invented. Quarles introduced a lot of that into the military, he made sure that their manufacturing acquisitions operated that way and he had a very keen interest in seeing it adopted.

We had the Western Electric Company here, and it was our source of all this stuff we had to use. We had to use equipment for 40 years or so in those times. Unfortunately, most of that has gone away now. And Quarles was a fellow who said, "yes, you can engineer and manufacture components which will last 40 years." Now, that approach was a very different deal, understandably, for the military services than they had been accustomed to. If equipment lasted over a couple of battles, that was long enough, and they had a point. They were right about not worrying too much about longevity if you were going to use it in battles. But Quarles had a sense that you'd better bring in some of this quality assurance and long life and reliability and quality, generally, into weapon acquisitions. He would have been a good exponent of the acquisition issues that are now so prominent and so difficult, which Deutch and others are dealing with.

H: Was the President's science advisor, Dr. Killian, involved in the creation of ARPA, the Advanced Research Projects Agency? How did that start up?

B: In very much the same context of Eisenhower's concern that we weren't using the resources of the USA for these terribly compelling Soviet challenges in '56, '57 and so forth. Specifically, they had a fellow from Chrysler who had said this. He had been taken over to the Pentagon as a civilian advisor, and "Ed," he said, "We're, just not using the latest technology. These missiles that Shriever and the other people are designing are not going to be up to date the way the acquisition process is going, specifically the nose cones." And Eisenhower had said—I think Goodpaster was probably part of this, too—"Well, I don't know if these nose cones will work—whether we are ever going to be able to use these bombs, if we are ever to deliver them, if we have to do it." On the one hand, the plan was sort of conventional—partly based on Soviet intelligence, too—to use a copper-beryllium alloy for the nose cone. The structure of our bombs was going to be much more sophisticated than the Soviets' and was therefore more sensitive because our bombs were more sophisticated and so much smaller, and therefore we didn't know whether they would survive reentry or not. So the people in PSAC—Kistiakowski was quite active in this—said, "Well, you'd better find out." Turned out the missile people—TRW and others—couldn't really assure anybody that the nose cones would work. So, I said we'd better get with this and ARPA was one of the earliest components in getting people to do it. Now they reached out to various people including Games Slater and myself—not the great physicist John Slater, but a fellow by the name of Games Slater who invented fiberglass. We were tapped by ARPA to do something about missile nose cones, because we knew about polymers and macromolecules. And what we did was to study the situation with ARPA people and the composite nose cone that GE picked up, and developed in their Valley Forge scheme. But this was the context in which ARPA worked. They looked for good circuitry. Obviously, I think the missile nose cone was one of the major successes.

H: Those tests for the nose cones were flown in '58, '57?

B: Yes. That was so heartening to Eisenhower that he took the actual test cone into his office. You've probably seen the picture of that. I was there when he did that; it was the darnedest thing. The nose cone had been recovered down in the South Atlantic, as you know. There is an island down there where the Air Force did a very good job of setting it up. Eisenhower insisted they bring it home and they took it into his office.

H: The PFIAB evolved over time and had different people coming and going, how did it interact with the U.S. Intelligence Board that was made up of big players—you know—the DCI and . . .

B: Well, we interacted cautiously with them in the sense that I think we pursued issues with very much President Eisenhower's encouragement and that of his successors, with a certain amount of arrogance with respect to the White House sources, realizing that these bureaucratic entities, as diligent or patriotic as they were, simply weren't with it. Things were moving too fast for them and they had not enough technical skills in their background. So we regarded these things like the U.S. Intelligence board as inevitable accessories. You could get White House orders through them. We wrote various orders. I remember writing one that said the NSA should remain the responsibility—delegated responsibility—of the Pentagon, the DOD, and so on; they were going to cut that out of the Pentagon. In fact, the USIB wanted to give it [the NSA] to the CIA. I said to myself, "That will be a disaster." But anyway, we wrote that executive order and various other ones. They had to go through that sort of circle or alley, or whatever.

H: And then eventually, the National Security Council, would issue an NSCID [National Security Council Intelligence Directive] this or NSCID that.

B: Yes. Yes. That's exactly right.

H: You remind me of the tension that existed between the CIA and the NSA over which organization was going to process what part of the SIGINT business.

B: Oh, yes. It's still there. You see, Pat Carter—I don't remember him as well as the other NSA directors—but he practically lost his job because he did a little processing on board the vehicle, which you have to do eventually. But this argument that you're talking about of doing certain ground processing as is delegated became so intense that they finally drove Pat out.

H: I've moved through all of the primary questions that I had. In retrospect, looking back at this seminal period in which you served and served so many different administrations so well, what to your mind were some of the key technical innovations that made possible this tremendous revolution in overhead systems?

B: As the President of the British Association for the Advancement of Science pointed out to the leading scientists of the time, none of this would have happened without solid-state technology. Now what we did, also, in this Office of Defense Mobilization era that you

mentioned, pre-PSAC, was to say that materials science, engineering, and solid state were on the verge of a major revolution, a major advance. And it was going to make things possible—machines, missiles, aircraft, and a whole lot of stuff—that nobody ever imagined before.

Okay, we also said that this is likely to happen to some energy sources; it wasn't quite as dramatic, but, as you perhaps remember, fuels were not quite as good as we had hoped they would be. And the nuclear fuel business emerged. Nuclear powered aircraft came up, and we spent a lot of time trying to dispose of that because it wasn't a good idea, but nevertheless, that was a major element, too. It was Shannon's information theoretic work that initiated the digital age. Those two things, materials science and solid state technology, were absolutely central frontiers. The value there is enormous. We thought it was pretty good when it was happening. When we look back it looks better than ever. I mean, we got results much beyond what people thought would happen. People will probably feel the same way about the biological science that we are doing now, in the middle of the next century or the end of the next century. At any rate, this materials/solid-state work and digital circuitry, with, of course, the theoretic base of Shannon, is what were the biggest moves.

H: In digital imaging, the charge coupled devices . . .

B: Yes. The charge coupled devices, that's very important —that was a by-product or derivative, actually, of the solid state circuitry in that photons and images can be handled very elegantly in a semi-conductor matrix, which is possible again because of the solid state revolution and the semi-conductor era, and what Smith and Boyle, in that building over there, did was to show that you immediately deal with levels of sensitivity, and illumination, and other things which were considered to be impossible.

H: What year did Smith and Boyle . . .

B: About '62 or so. I took it out to the NRO immediately. It was two or three years before it was announced publicly.

H: A year or two later, the DS&T at the Agency began to contract with Westinghouse, I think it was, for some of the early work on it. But I didn't know it had started here at Bell Labs.

B: Oh, yes, it was the charged coupled device. And now you find all these references from people who don't know the history—thinking, as you say, that it came from somewhere else. Boyle retired. He's a very creative fellow. And he retired fairly early and went to—his

family had some interests on—Prince Edward Island or Nova Scotia. And he still lives up there. I always regretted that he had; I think he would have invented some other things. As a matter of fact he did a little work for Bellcom before he retired. But this charge coupled device with Smith was a beautiful discovery. Now, it's still going on. I think I might have mentioned to you on the phone that a fellow here—we do a little optics nowadays, and photonics—discovered a way to mount a mirror on a charged couple device base so that you can get 360 degree coverage. This was only two or three months ago. The NRO people know about this and they are going to be using it, but this was a very clever adaptation. There is another point I should perhaps bring in, that you put your finger on, something I find interesting. We said during this whole PFIAB intelligence evolution—that industry of course is in a major mode; it's learning a lot of things. Because of the Soviet threat, we were going to see to it that the ordinary competitive restraints on industry were not followed in the intelligence community. We're going to see to it that the intelligence community gets the knowledge it wants immediately, and we have confidence that it will respect it and not short circuit it. Well, the charged coupled device was one of the first examples of that.

Now this thing called SASA—you know something about that group? A retired General, John Morrison, has a kind of a little association out there at Fort Meade called the Security Affairs Support Association. He's committed, and SASA's committed to this same idea: that you could get things from industry before they are generally available and that their proprietary aspects will be respected by the Intelligence Community. That's a very strong group. Wheelon got recognized there a year or two ago. It is a good group.

H: Yes. I'm a member.

B: Well, I'm awfully glad you are, because that's a resource for the United States that no other countries have. You know, the other countries are amazed; they don't see how we can handle that, but it works out.

H: Are there any other technical innovations that come to mind that you deem especially significant?

B: Well, of course, materials has a huge spectrum of them and involve things like high temperature turbines for aircraft, rocket nozzles—we've mentioned the ablative nose cones, but also the heat shields for almost all recovered systems, I believe. The whole arena of sensing systems beyond imaging has things like temperature and, of course, the secondary derivatives

of photonics, a whole new field is the laser, which was discovered here by Townes and Schalow over in that building. That wasn't until '58 or so. But, of course, the implications of that are endless—boundless. One can do all sorts of scanning work with lasers, they're vital for intelligence, but its impact on the rest of science and engineering also is just enormous—tremendous. Well, I'm still not giving you much of a complete survey. I guess the only thing to say is that you see the whole idea of the structure of molecules and atoms, see, I'm saying practically nothing about nuclear physics and nuclear energy. And then they did discover—Davidson and Germer in New York before we moved out here—the diffraction of electrons, the diffraction of electrons that are able to act as waves. Now some people would say that's the most important scientific finding of the century and in many ways it is; you are going to hear an awful lot about that in the centenary of the electron, which will be coming up next year. But that did not have anything to do with defense and with intelligence programs. But subsequent things such as the scanning tunneling electron microscope, certain acoustic microscopes, the atomic force microscope that C. Quate has been developing recently, are all related to some of our earlier intelligence and military technology efforts—which in turn, you see, give us a handle on the nature of matter that affects everything.

H: I'm constantly amazed as I've worked through this history at the number of advances that were applied first in the national security arena. And later, these advances ramified with terrific impact throughout the commercial world. The little camcorders that people carry; they have no idea where they came from.

B: Absolutely. This might be a time for me to get that archival book that illustrates your point about how things got started up here, at least, on the defense phase. Secondly, I will dig out at your convenience, some more of these illustrations. I've not organized these very rationally.

H: Well, I'm just glad that we could sit down for a few minutes . . .

B: People really don't have a chance to absorb it, the advances that came out of the revolution in intelligence technology. But they can absorb it through your essays.

I don't know if you saw Mrs. Cheney's report in *The Washington Post* a couple of days ago about the revisionist history—the change in teaching history, American History. Very well taken, I think. She pokes fun at them. A lot of stuff that is propagated now in schools is just wrong, and it just doesn't establish anything. I belonged to the National Archives Commission

for some time and I went down there just a few years ago—I spend a lot of time now trying to restore literacy, get educational reform—and I said, "can't you set up a little group that will show what happened in American history around the Civil War, and around a couple of other times?" Well, after a while they did, a woman named Brown. And they have a little group there now—it's very small—that will receive a history writer, historian, like yourself, and actually show them what the records said, you see. But the astonishing thing is how little that's been done with the records and how much just seems made up today.

H: Some of the young historians have some very curious notions about how to do history. They start out with a preconceived hypothesis . . .

B: Oh, yes.

H: And then they select the facts to fit it, discard those that don't, and you come away with a very strange result.

B: Sure, well let's ruminate on some of those things.

H: One of the questions I had involved Jerry Wiesner, who was picked as the science advisor to serve President Kennedy, as I recall. Is that not true?

B: Oh, yes. He was the leader, yes.

H: And he prepared for the incoming President a report on science and technology and the space program, and in it he did not have a lot of good things to say about manned space flight in terms of what might be possible. Yet within a few months Kennedy announced that we were going to go to the moon. Can you talk about that reversal?

B: Your reconstruction there is absolutely correct. The subject was just not one of the PSAC items. And there was astonishingly little interest in lunar flight, because it seemed so unlikely, so impossible. We were trying at that point to focus on the space technology and space performance, which on one hand was heavily oriented towards national security, as we said, and on the other hand, on space science things like the Hubbell telescope, that x-ray spectrum thing that Friedman did, the other Friedman.

Now what we heard was that the President talked to his political advisors and they said, "you have to do something which is exciting and revolutionary and exceptional, like going to the moon." And this—the gossip at the time, I think I was working down there at the time—I think it was the 19th of May, a Sunday, and he didn't have us in but he had apparently some political associates who said that he had decided to say that they were going to try to go to the moon.

H: That was one area in which the U.S. could beat the Russians, I think.

B: Oh, yes. The Soviet competition was a tremendous part of it. So, there was no consultation with Wiesner or anybody else at this point. Wiesner, I think was very thoughtful and he'd been told this announcement was coming out; somebody told him that. But he was very supportive in saying "Well, we don't know what is in mind, but we'll be sympathetic and responsive," and one of the first things that came up later got a great deal of rather sensational attention. That was, do you do this by orbiting the Earth and going directly to the moon, or just by orbiting the moon and landing there that way. And Jim Webb was, or the NASA people were, uncertain which way to turn. So they had long discussions. Should the military be given the job—the Air Force? Should it be NASA? How should they do it? And they got with us and said well, it's going to be a test of reliability and of particular predictability; we'd better set up a separate enterprise, and that's the way Bellcom was formed. Now, Bellcom worked on the thing. Deming Lewis, who later became President of Lehigh University, and who had worked with us here for 20 years, became the first president of Bellcom, and he and others figured out that they thought it was possible that they would—with some concern and some restraint—they would nevertheless try to organize an enterprise which would engineer that journey.

Right up through the planning stages there was still a lot of uncertainties about how you optimize the fuel and the guidance issues, and this question of orbiting, where and how, a fellow named Sid Darlington, a mathematician, who's still alive—he's quite aged now—Sid Darlington is up in New Hampshire, I think—figured out that you'd better do it by orbiting the moon, and that this was really a very essential issue. But they supported this—they being the people at the White House—people around the President. It was the way the thing worked out—there were practically no hitches—people just worked together, hit milestones, and it was a classic example of what cooperation and sophistication in engineering and technology could do. So that's the kind of thing that I think Purcell and others will illustrate.

The attitude toward that was somewhat affected by our experience with communication satellites, which was I think one of the reasons Bell Labs and Bellcom were picked for the engineering. That was a good illustration of how a novel enterprise like NASA could mobilize the resources of the country. See, we—Pierce, who got the Draper Prize a couple of months ago in Washington, and a fellow named Clarke, who's the science fiction man from Ceylon, had the idea of a communication satellite. But the first one was Echo and that provoked some

intelligence interest, as well, you see. The Navy people had got radio reflections from the moon and we had a telescope at Greenbank, which we also worked with. And they were very serious about that and they intended to intercept SIGINT reflections from the moon much more extensively than we thought really would be useful. Nevertheless, we did work with them on the design, and I think there's some comment in here, of an enormous antenna at Greenbank—actually it was going to be in a slightly removed place in West Virginia. The West Virginia Congressional delegation really thought it was Manna from Heaven, because it would take a lot of construction money and also a very large staff there.

H: It was going to fill up a whole valley. It was an enormous thing.

B: Yes. And so this was the kind of thing—in fact we cooled that off some and felt that the Echo satellite had some opportunities, it maybe could be used as a reflector, and then we moved independently, you see, toward Telstar, which became quite reliable even though it did not have the 24,000-mile geosynchronous position. Those things all led the President and the government, I think, to believe that the country could mobilize its resources and could get what was thought to be impossible, actually done, which was a pretty good exercise. And if you express that historian's view, you have opened the horizon pretty wide, because the examples of that in the history of the USA are not very many. We've done good things with inventing and manufacturing automobiles, and good things in refining petroleum and you've got the octane numbers up. And the good things in the polio vaccine, which is still rather primitive. But when you come to enormous things like the moon shot, there isn't very much to compare it with.

H: The Manhattan Project, maybe.

B: Yes.

H: Well, you say that Wiesner wasn't too keen on it, but once the President made up his mind, he saluted, and . . .

B: That's exactly right. And Wiesner was very cooperative in getting people in PSAC interested; they weren't keen on it, either.

H: Well, it was basically an engineering project.

B: Yes. Now, you see this has some obvious analogies that I think interest you and which I suspect you will think about, and that's the ABM. The ABM is almost as incredible as the moon shot. It hasn't quite hit us that way—and there's a lot, in the meantime, a tremendous amount of polarization going on among people about doing it or not. Actually, you see, this is

well written up here; we not only have the early Nike but the Nike Zeus, and some of the other attempts showed you can do sort of an ABM deal. We operated that site out there in North Dakota for several months. But the theme is, if the United States really got against the wall, if China or Cambodia or whoever else really began to threaten us, if we can mobilize the country the way we did for the moon shot, I'm pretty sure we could survive.

H: I think that the point defense ABM is a very realistic thing; the orbiting space system, even using kinetic kill systems, gets very . . .

B: Very difficult. Extremely difficult. But you know, it's not a thing that one easily writes off. Perhaps more than that it may be too difficult, and there's some things that we've faced—we've mentioned the nuclear-powered aircraft and so on, of perhaps not so much difficulty—but technically not very appealing and those have been written off. But what I'm saying is that the opportunities to do things that are reasonable—and as you point out, the orbiting part you can't be sure about—but to do things that are reasonable are really pretty compelling. And the U.S. can do something there that is achievable. Now that is illustrated by the following: as we got along with the Nike Zeus and its derivatives, the software issue, of course, became very, very baffling, very challenging. And the National Academy of Sciences had a little panel that said it probably won't work. If we'd had a panel like that about the moon shot, I suspect they would have said it won't work or it's not feasible, you see. And I'm afraid that's even true—more true, now. The National Academy, which I'm very fond of and have tried to support for many years, can be neutral or political; nevertheless, there is this matter of "national spirit" involved, and in the moon shot you really had that spirit mobilized.

Now, what I'm getting to with ABM is that this little panel said "you can't get software that will be adequate for Nike Zeus." Or even any kind of reasonable point defense against an incoming intercontinental missile. And that was right at the time—it happened to be during my watch. We were building, a building that still stands, now used for other purposes, out in Madison, New Jersey, which was committed to the ABM software and use of the best computers we had at the time that would do that. And it worked. We got people in and we produced it, you see. And everybody agreed, including people at the National Academy, that yes, that software would work. But it shows that the best intentions, the best thoughts of what you can do in science and engineering, it's not necessarily definitive, not necessarily complete. Some assessments are very good. But they shouldn't all be taken at face value, which we tend to do nowadays. And of

course, it's going to get worse, because we have a budget stricture, which is very real, and we have also disarmament strictures, which people hold to, and various other social distresses that confront us. And the result is that this judgment of what you can do—whether you want to do it or not is a different matter—but what you can do is not very comforting.

H: Another question in this regard. From your perspective, those in your generation who in World War II served the government at the Radiation Lab, at Caltech, or wherever, came back again to serve during the Cold War, in the 1950s and 1960s, but there doesn't seem to be the same national spirit today. What is your opinion of the younger scientists now, who might not even be inclined to support national security interests?

B: That's right. There is a disinclination or withdrawal. That came in about 10 or 15 years ago, when we had quite a fight about maintaining even some NRC [National Research Council] committees on national security. The younger members wanted to abolish them. And what remains there now is quite primitive. Harvey Brooks can tell you more about that.

H: A sea change came along after Vietnam. It seems this was a watershed.

B: I just don't know. The threat to national security doesn't seem nearly as compelling to these folks. Whether, if we hadn't had a Pearl Harbor, whether we'd have all been as intense during World War II, I don't know. Our history of disarmament, of course, is not good; we did all that stuff in the '20s to little effect. I think I would take refuge, and this is probably naive and probably too primitive—I would take refuge in the idea that people are overwhelmed. The intent of being able to combat nuclear weapons or nuclear accidents or epidemics or plagues and so forth is sufficiently poor so that they think, why bother? Why do it? And that's an oversimplified view, but I'm afraid there is a fair amount of that. That's why your chronicle is so important, you see. When we as scientists actually faced up to it with a recognized national challenge, we could do it, and we did do it.

H: And people gave their time to serve the government.

B: Yes. Now epidemics may be the next move. I don't know. I've served many years as chair of the Rockefeller University and I'm still chair emeritus and have spent a fair amount of time there. The next to last fellow that I recruited as president, was Josh Lederberg; his predecessor was Fred Seitz, who was the President of the National Academy and who, by the way, is very, very good on these matters. If you want some perspective of how national security policy arose with respect to the national community—Fred Seitz is just the right person. And he

would be glad to talk to you. He's getting more and more interested in history, himself, nowadays. Anyway, his successor was Lederberg; you may have heard about Josh. Josh, Nobel Prize winner, could give a very serious analysis of the possibility of plagues and epidemics, and that we were finding there and many other places where our traditional therapies, antibiotics and so on, are failing because of the adaptation of the organism, you know. And of course, AIDS is a classic case. But AIDS is only one of many, many options and possibilities. So maybe we won't have to face that and maybe we'll need to read your history of the Cold War and say well, we're going to organize and do this again.

H: Turning back to the moon shot. You alluded to how NASA was sort of over-awed when it got this tremendous assignment. James Webb reorganized the whole space agency to prosecute Project Apollo. When Apollo ended some years later, it's my opinion, or my view, the agency leaders had come to believe this was a way of life. Instead of an exception, they looked at it as the rule and they began to go with manned space flight and they cut back these other areas of instrumented space flight. From your standpoint on PFIAB and elsewhere, on PSAC, was there a general sense that NASA with Payne and Fletcher were going down the wrong path with the Space Shuttle—they didn't come back to the original mandate that pursued space science and applications?

B: Well, I think you are identifying a terrible problem or terrible responsibility of an agency like that, and that's to make judgments that are objective and full of the best technical insights one can get rather than being political. I was kept on, of course, in various administrations, but also for general technical advice, perhaps because of the intimacy we had. When Jim Fletcher was first made head of NASA he hadn't spent a lot of time in Washington. And I thought he ought to get to know these bureaucratic technical heads—the people that we referred to earlier that were the statutory heads of technology. So I gave a luncheon for him at Blair House, which was sponsored by the President. And Jim Fletcher came and we had a very nice group and I introduced him as the fellow who came to Washington to kill the Space Shuttle, which was not terribly appreciated by Jim, but these people—and many people were there—had sort of a moment of silence and then they took various reactions and positions which were really quite fascinating. Some of them would have supported the view, you see. But most of them recognized that he was going to be subject to terrific pressures, including industrial ones, to carry the thing on, to expand the budget, and do the rest of it.

And in the long run Jim gave in. But he was amused and then somewhat intrigued, a little shocked, but Jim was a person of high personal integrity. We knew his family very well. As you know, his father was here for forty years, and his brother was here, and so on. So if he'd had a certain modicum of spiritual support, maybe a certain access to the history of how technology had evolved for those days, I think Jim might have taken a different position on the shuttle. But I've given you a long answer because it did become subject to politics.

H: I've always been fascinated with NASA under T. Keith Glennan, when it had a broad spectrum of space exploration and manned flight was one component, but after Jim Webb prosecuted Apollo it appeared that the manned flight now predominated in the space agency's agenda.

B: Yes. Now let me take you on a little tour of NASA for five minutes or so, if you don't mind. I've never said this for publication. But I think the way things have evolved now make it appropriate. After Keith, who I think managed these things very well, finished his time, they came to the question of the next administrator. I think it was largely Wiesner, but others perhaps, too, said I should be the one selected. When Kennedy was inaugurated we had a bad snow storm, you remember, and he had given the Vice President the task of dealing with space technology and some of those issues in this field. He also gave him the responsibility of following this question up, who was going to be the next Administrator? And so the Vice President asked me to come down there and I came down; I had to walk from Pennsylvania Station up to his office in the Senate office building, you see; he hadn't actually moved into the Executive Office Building. And I'll never forget, I got through the snow; he asked me how I made out, and I said, "Mr. Vice President, you had better transportation here during the Civil War, because the horses could have gotten through the snow, and I had to walk."

Anyway, the outside of his office was just littered with people, because they knew he had the responsibility for many appointments, and they wanted appointments. So I came in. He brought me right in, and he said, "You ought to take this job." Of course, we knew the background of it and so on. I thanked him very much and I thanked the President and I said, "I must tell you, Mr. Vice President, I am not the person for the job. You've got to have somebody who can reflect the public, political, and popular attitudes in this business much better than I and who can respect the technology, but this is a large enough mission so that it must be done on a different level than I as a scientist or technologist can do."

Well, Johnson was very cordial about it. He was apparently taken aback—he thought he had had that settled, and he was a little bit disappointed or something—but while he thought about it, I said, "I know some of the people you've been thinking of and James Webb is one of them." We knew him with respect to his work out in Oklahoma, you know, in the petroleum business. And I said, "He [Webb] is great. He has had all kinds of background, public and private." Webb and I had formed a committee on the future of cities, two or three years before the Kennedy administration. Together we had made a very detailed report. I must say we did foresee a lot of the problems of the inner cities, but we worked very intimately, and I knew Webb could do this sort of thing very well.

So they went back and Jim was glad, I think, to have the job. And Jim was entirely sympathetic to good technical judgment, but obviously Jim properly felt that he had a political and populist mission as well. I mean, what we said earlier about the moon shot, you see, was not Jim's doing; he was perfectly responsive, but he was doing what the President wanted.

Now, a particularly poignant feature of this is that—I shouldn't be quoted on this, but on the other hand, it is exactly the detail. There were people there who saw it, and Jim is gone and the others, too; but the Telstar trial demonstration came after we had gotten the thing working all right, and it was going to be the first video transmission and a detailed one. They invited the President. The President later told me that he was sorry he didn't go, but he had the Vice President go.

And I sat there to represent and explain events if it didn't work. This was in Carnegie Hall, the one in Washington. The chairman of our company was up in Maine, I think, with an antenna up there trying to be sure that worked. But I was down there in Washington—to explain what happened if it didn't work.

Well, mercifully, it worked, and Senator Margaret Chase Smith distinguished herself by saying the most graceful thing when the Stars and Stripes appeared and the French Tricolor appeared on the first transoceanic video transmission. But everybody got in a very jolly mood, and we had a reception, and the Vice President was feeling very good, after he had a liberal amount of a certain refreshment.

Webb was there with people around him, and Johnson came up and said, "That was very good, and we are so glad it happened." And he turned to me and said "It worked all right," and then he turned to Webb and he said, "We tried to give Baker your job." About 10 people

standing there heard this, you see. And Webb had a very quick response. He said, "Why didn't you?" which I thought put the Vice President very much in his place. But nobody ever followed that up, you see, and it was done in the spirit of alcohol stimulus, so I think everybody respected that. But I thought you'd be a little amused that Johnson played his role all the way along, but there was a great respect for Jim the way that went.

H: The Telstar's success precipitated the whole COMSAT Corporation activity.

B: Yes.

H: The legislation followed to set up how we would handle this; now we've got this communications tool.

B: Oh, yes. That triggered political affairs—I'll mention a case of that in a moment, but in the meantime Webb handled this so well, and the whole thing was so systematic, it created tremendous values around the world for Kennedy and others. But when the moon shot came, they based a lot of their thoughts on that, and that's why I think Bellcom got tagged to participate. Anyway, one day I got a call direct from the New York Times—what was that fellow's name—the reporter . . .

H: Sullivan?

B: This is before Sullivan. Sullivan was the science man. This fellow was the defense politics man. We'll think of it in a minute. Anyway, he called me, and he said, "You were seen leaving the White House office the other day. I suppose this means that you were lobbying for telecommunications satellites like Telstar." Well, I had nothing to do with that. I was leaving PFIAB. But he pushed on and put something in print that we were pushing for domination of the Telstar or the COMSAT deal, which I think probably cost us, and others, too, perhaps, a certain amount of political capital because we had no such intentions. And, of course, as you know, the way the thing was set up, it was probably as good a compromise as they could have had.

The irony there—I'm a little sorry to see COMSAT now coasting along and going into the entertainment business—is that photonics have done so well that they are pushing the satellites down a bit. On the other hand, you probably know that we have this thing called the Marconi Council. Did you ever hear of that? Marconi's daughter, Goia, she was a remarkable character, decided 20 years ago that he was being forgotten and that she was going to change that, and, by golly, she has changed it. She got two or three of us together like a council to give a

prize, a very modest prize in the best context for Marconi, the Marconi Fellowship. Killian was the first winner, by the way. And this included some people who recognized the value of communications and advanced technology and the like.

H: Is it an annual, semi-annual prize?

B: Yes. She has organized the thing beautifully. It has been given around the world: Tokyo, Rome. The next one, I think, is in Denmark, but the point I'm getting to is that one of the particular people we chose was Pal, an Indian physicist who is very good. This reflects simply on the point that satellites shouldn't be written off—I mean, communication satellites. This fellow recognized that India could never afford large antennas and dishes for people, but he found that you could take chicken wire with an appropriate mold form and make an antenna that was good enough so that people with rather primitive television sets around India could get direct satellite inputs. And we gave him the prize because there are thousands of these things that are literally made of chicken wire.

H: And they are used now in India?

B: Yes, for individual village communications satellite reception. So there is still a lot of life in that sort of thing, and I think, will be felt in other continents as well, even though the photonics have really taken over the transoceanic work, which I'm proud of, and I think that's going to continue to grow very, very fast. We're going to put a circle of photonics cable around Africa which will open up Africa, you see, because what you can do with photonics cable is tap off it anywhere, whereas you can't do that with an ordinary electronics cable, coaxial cable. You have to have a terminal, and then you have to distribute out of the terminal.

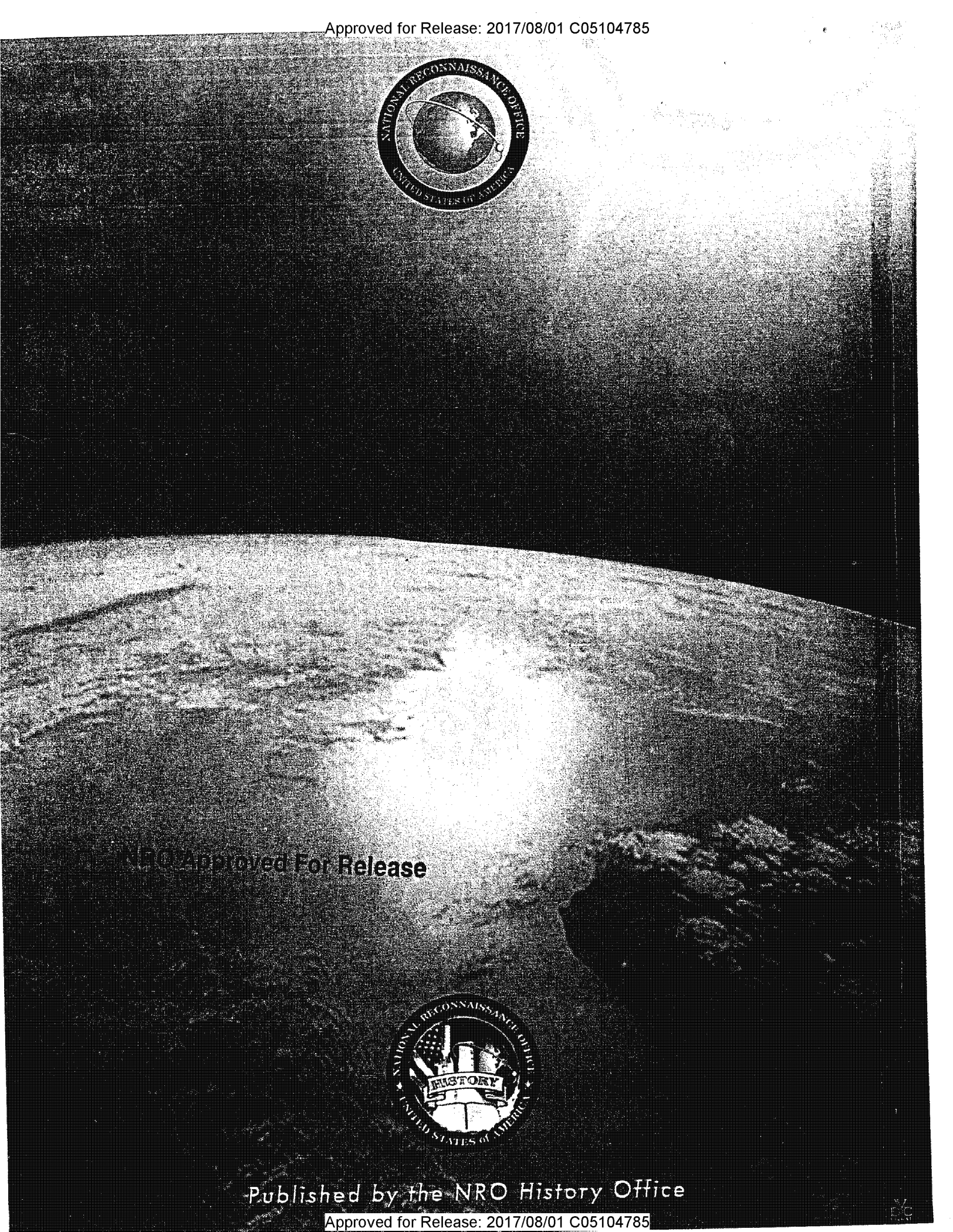
We can tap off from these photonics cables at an enormous bandwidth, even bigger than the satellite bandwidth, and so we're going to have a situation where these countries in Africa will be forced to recognize each other. They will have some knowledge of what's happening to them and what kind of commerce they have and so on. But in spite of that, which I think is going to be a very dramatic change, satellites are still going to have a big role.

H: Arthur Clarke would be pleased.

B: I think so.

H: Thank you for your time.

- END -



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