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Central Intelligence Agency

Washington, D.C. 20505

25 February 2014

Reference: EOM-2013-02065

This is a final response to your letter of 29 July 2013 in which you requested an Executive Order 13526 mandatory declassification review of the following document:

"Speech given by Richard Bissell at CIA Headquarters, 12 October 1965."

We completed a thorough search of our records and located a document responsive to your request. We have determined that the document may be released in sanitized form with material withheld on the basis of Section 3.5(c) of the Order. Enclosed is a copy of the document showing our deletion and citing our exemption.

As the CIA Information and Privacy Coordinator, I am the CIA official responsible for this determination. You have the right to appeal this response to the Agency Release Panel, in my care, within 45 days from the date of this letter. Please include the basis of your appeal.

Sincerely,

Michele Meeks
Information and Privacy Coordinator

Enclosure

# Enclosure

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EO 13526 3.5(c)

~~SECRET~~

Speech given by Mr. Richard M. Bissell

12 October 1965

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S-E-C-R-E-T

RICHARD MERVIN BISSELL, JR.

Place of Birth: Hartford, Connecticut
Date of Birth : 18 September 1909

Married : 5 children

Education : Yale University; B.A. Economics, 1932
London School of Economics
Yale University; Ph.D., 1939

CIA Experience: Served as an Intermittent Consultant to the Office of National Estimates, 1952-54; Special Assistant in the Office of the DCI, 1955-59; Deputy Director (Plans), 1959-61.

Non-Agency Experience: Instructor and Assistant Professor, Yale University, 1934-42; served as Chief Economic Analyst with Bureau of Foreign and Domestic Commerce, Dept. of Commerce, 1941-42; Associate Professor and Professor of Economics, Massachusetts Institute of Technology, 1942-48; Assistant to the Deputy Director and other executive positions with the War Shipping Administration, 1942-45; Economic Adviser and Deputy Director Office of War Mobilization and Reconversion, 1945-46; President's Committee on Foreign Aid as Executive Secretary, 1947-48; Deputy Administrator, Acting Director with Economic Cooperation Administration and Mutual Security Agency, 1948-52; employed as Consultant concurrently with the Ford Foundation and Mutual Security Agency, as well as ONE/CIA, 1952-54. Also served as Consultant to FORTUNE Magazine, 1937-39, 1943-46; Economic Adviser to the Connecticut Public Utilities Commission, 1936-41; Staff Member of Committee on Employment for the Social Science Research Council, 1939-41; Consultant to Cosmopolitan Shipping Company, 1946; United States Steel Corp. of Delaware, 1948; Scudder Stevens & Clark, 1947-48; Coordinator of Exports, 1947; Brightwater PaperCo., 1947-48; Asiatic Petroleum Co., 1948; Gray and Rogers, 1948; President, Institute for Defense Analyses, 1961-64; Director of Marketing and Economics, United Aircraft Corporation, 1964-present. Author of "The Rate of Interest," "The Theory of Capital Under Static and Dynamic Conditions," "Price and Wage Policies" and the "Theory of Employment," "Price, Costs and Investment," The Anatomy of Public Spending," "The Impact of Rearmament on the Free World Economy," "European Recovery and the Problems Ahead," "Foreign Aid: What Sort? How Much? How Long?" and contributor and editor of report, "European Recovery and American Aid"(Report of President's Committee on Foreign Aid).

S-E-C-R-E-T

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Speech given by Mr. Richard Bissell on 12 October 1965

I think I will stand up so you can see me. You know this gathering is the clearest sort of evidence of a major and very important change for the better that has occurred since I left; and certainly not contrary to what I wanted and hoped and tried to do when I was here. Somehow it never got done, and I give Bud a lot of the credit for this, but the strength and every dimension of the inhouse staff of this part of the Agency is just vastly greater than what was here when I was active, and this greater strength fills one of the very real gaps. Quite a lot of you were old colleagues of mine and quite a number of you are new since my day, and I think that this is testimony to the fact that a most important and necessary evolution in the Agency has come about.

I have talked to the gentlemen, both to my right and to my left, about what subject to discuss this evening. I know this is a pleasantly small and informal gathering, and so I thought I would do a certain amount of rambling on to the tired old warriors of history. I thought that I would then, in a somewhat opinionated fashion, as usual with me and well known

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to those who have been my closest colleagues in the past, Jim Cunningham and John Parangosky, who well recognize my views about some of the lessons of the history. Then unencouraged by either the gentlemen who flank me at this moment, I thought I would probably offer a few obiter dicta about the future just to get off the reservation a little bit. You understand, gentlemen, it would be not only unnecessary but inappropriate for me to talk about the technology of the things I was concerned with. I want to talk a little bit about not only the U-2 but OXCART, and there are very few here who don't know at least as much as I about the technology and the technological and scientific accomplishments of these programs. The two have some sort of parallel lessons, and I am not going to say anything that couldn't be safely published in Isvestia. But I will start a lot of rambling history and some attempt to talk about accomplishments, lessons as it seemed to me that were to be learned and the like, and they won't be about the technology I don't think. This is quite widely known now and it is very knowable to any subscriber to Aviation Week, and what I can add to the history of these affairs is not about the technology at all. But about the origin in the government and who did what to whom and when and how and what were the issues of governmental relationships and what were the issues of organizational structure and what were the issues of inter-departmental relationships, the problems of management, and, in particular

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I think, what is interesting, I trust, to any group like this is where were the innovations, I mean the non-technical innovations, because I sometimes feel that in any group, (and this is not only a group of people like yourself, but any group of people like those I circulate and work with at United Aircraft and lots of others) any group whose primary orientation is technological, there is sometimes a temptation to think that an innovation is a term that applies primarily in the aerial technology. As I look back on these events, I'm inclined to think that the innovations of a non-technical sort are as important as those made, admittedly by others, contractors primarily in technology.

The U-2 program burst on my vision about Thanksgiving of [redacted]. 1954. I have been a little off on my years and I had to get John Parangosky to help me before dinner, but this particular mistake wasn't his. But it burst on my vision when Allen Dulles called me into his office and said a program, a project, has just been approved by the President. I think you ought to take charge of it for me but it is too secret for me to tell you what it is. We started from that and eventually he very reluctantly handed over some documentation he said I could keep for three days and which remained in my files until I left the Agency, and I believe they are still in those same inherited files.

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I didn't have time for quite a few weeks to study those files, but when I eventually got around to looking at them I learned as I had by then through conversation that in a manner of speaking the origin of the U-2 program was with something called the Surprise Attack Committee or panel or call it what you may. It was one of the Killian Committees with which recent history has been studded. This was one formed in late 1952 and had all of its meetings in 53. It was indeed one of the royal commissions that have become a feature of the U.S. Government to inquire into the danger of surprise attack against the U.S. and the means of guarding against it. I suppose most of its work was on indicators and the Indication Center but it had a panel on intelligence headed by Din Land. The panel included Ed Purcell and a Mr. Lathan, who still is with Arthur D. Little, and a Princeton mathematician and one or two others, and as this panel worked its way around the Executive Branch it came on a sort of rough proposal that had been submitted by Lockheed to the Air Force the preceding February for a high altitude reconnaissance aircraft that could be used for overflight of the Soviet Union. This was one of the proposals that the Air Force had turned down. Some four proposals were accepted of which later at least two were canceled. At least two of them were carried through under Air Force auspices to the point of building prototype aircraft. For

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reasons that I never have known to this day, people like Din Land, a specialist in optics and chemistry of the motions; and Ed Purcell, a fairly theoretical physicist, but quite capable of descending from the clouds; and a man called Kennedy, who was an organic chemist from St. Louis; and Lathan, who was a management expert; collectively decided they knew better than the Air Force. I believe they came in contact with Kelly Johnson, and his magnetism had something to do with this. And in any event they concluded that the U.S. Government should forthwith in great secrecy and with great speed build an aircraft of the sort proposed and should then overfly the Soviet Union, and this should be a solution of sorts to many problems relating in some way to surprise attack. I also learned from the file that a few other bureaucratic blessings had been bestowed on this enterprise.

What I gather was at a very late date indeed, Allen Dulles had had a meeting with the principals of the Intelligence Board, and they had drafted a stirring declaration that intelligence was a highly desirable thing and that overflight of the Soviet Union might produce intelligence. Armed with this document and with Messrs. Land and others, ~~lead~~ LED by Jim Killian, apparently these people waited on the President, and I think really quite a small group of them, and they induced President Eisenhower to endorse

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these views and to authorize the project. At this point, saving your reverence, the scientists felt that the problem had been solved and which in a sense it had because the Presidential approval had been obtained, and I must say it had been obtained on really quite a permanent basis. This approval endured for a good many years and it was ultimately good for something over $300 million and it would never have been given if this had been known in advance. Din Land, for whom I have both the greatest respect and the greatest affection, (as those of you who know me well know) is one who has always felt that once you had a "decision" within the U.S. Government and if it was at a high enough level the problem had then indeed been solved and there was nothing to do but carry out the decisions and little matters. Such questions as to who was to pay for something or who was to do it or what the relationships of A were to B and C, these were matters somewhat beneath science and therefore not meriting very much attention.

And Allen, as I said after the first interview, told me I was to run this enterprise but that it was too secret for me to know anything about it, relaxed as I have said. I read the files and the next two pieces of information I got: the first was that another man in the Agency, Herb Miller, thought he was running it; the third piece of information was that there

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was to be a meeting in the Pentagon the following day at which the project was to be launched. Herb and I had a meeting and we managed to patch up rather quickly, as these things go, a working alliance, and I went over to the Pentagon the following day. There was a table full of people and there was a gentleman who turned up to be a valued colleague of mine, Don Putt, who was then Air Force Assistant Chief of Staff for R&D. The moving spirit was Trevor Gardner then Assistant Secretary for R&D. He really had been in on the act at least to the degree that Allen Dulles had been. But we got around the table, about five or six of us, and first of all Trevor got on the phone, called up Los Angeles and said, "Kelly, the project's been approved. Go ahead." Well, I didn't know who Kelly was. The next thing he did was to get on the phone to East Hartford, my present place of work, and I don't know who he called there but anyhow, he said the project has been approved, go ahead. Then when he hung up the phone we began to consider such little matters as whose project really was this. So I was completely neophyte. I had absolutely no comprehension what technically was involved. Soon it became apparent there was no decision as to who was going to do what. We resumed along, a rather discursive meeting at which people talked about things they were going to do, but there seemed to me a certain indecisiveness about the meeting. I finally

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broke the ice and said I'm just new here, but who is paying for this airplane. The reaction was dead silence, and I was rather reminded of a wartime meeting (that's an anecdote I'll spare you until we have a drink in hand) but in any case, I looked to the right and found that everyone down the table was looking this way; so I looked to see who they were looking at, and everyone was looking this way. So I finally said I'll see what can be done about it, and we adjourned the meeting.

Well after a day of homework, I went to Allen and said I think if you really want this to move you better get some money out of the reserve, and you better authorize for me to say that we will pay for a major part of it, and this he did; so, what I assure you was not intended as a bureaucratically skillful use of a reserve, turned out to be just the same. I somehow found in a week or two that the shots were really being called by our then project office of, I think, three people. I apologize for these what you can call self-serving, but in any case, to me entertaining observations about the birth of the child. There are lots of dates and occasions one can anecdotedly recall in the early stages of this program. What has now become the august organization, which I see only that part of the iceberg that protudes above the dining room table, as it may, was for some weeks housed in my office and in a very small outer office which

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I then shared with Red White. We got up to about five or six active people by early spring. By this time I had a written charter from Allen Dulles which I had drafted and he had signed. It was a rather comprehensive document for its day; it was three or four pages. It was intended to run for three months but, in fact, it was never altered from that time on. In the meanwhile, the really important work was being done by the contractors and, with the real genius in this, Kelly Johnson.

He got that telephone call from Trevor Gardner on, I think it was, the fourth of December.[1955] If my recollection is correct, and Jim Cunningham's memory which I'm sure is better than mine, I think the first flight was on the sixth[1956] of August.[1955] This was really pretty good time for even a subsonic jet considering that up to the first of December there was nothing but sketches. There had been no engineering done. What started on the fourth of December was clearing out the hangar to build this beast and getting the engineering done. I think this is almost a unique accomplishment even in Kelly's career.

There had been other picturesque incidents before the U-2's first flight. I guess it was in March that I went out and flew around with Kelly Johnson in a small aircraft piloted by Tony LeVier. We had some maps that showed all the salt lakes in Nevada and we sampled about six.

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We finally settled on Watertown, a name which was dear to me and equally dear to Allen Dulles, as that was his birthplace in New York. The first flight came off on schedule except that LeVier, who made the first flight of the U-2, made absolutely a perfect landing six feet off the ground. The landing gear turned out to be tough.

By that time, shifting from anecdote to slightly more analytical discussion, I was engaged in the first jurisdictional battle which marked this part of the Agency. As the first flight approached, the question of who was running what part of this project had to be dealt with more head on that it had been to date. Up to that point, as I have tried to suggest, the power of the purse had perhaps been sufficient. I had paid a visit to Curtis LeMay a month or so before and he had mildly cursed me before his staff and said my ideas of operational dates and the rest were nonsense. He made it pretty clear that as soon as I had paid for it, he planned to take it over and he didn't expect that date to be very far removed from the date of our meeting. There followed a rather entertaining occasion in Colorado Springs where Allen happened to be getting a briefing at the Air Defense Command. Another equally distinguished Air Force General muttered to him in the corridor, "Don't let LeMay get his cotton picking fingers on that machine." But I went through a rather remarkably

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civilized and amiable battle in the course of which we hammered out a charter, which endured throughout the project. Under this charter, SAC set up a detachment to do the training at Watertown headed by Bill Yancey. They not only did an absolutely superb technical job, but they performed, as did every individual I think I can say in the Air Force, and there were a great many who participated in that project from beginning to end with complete loyalty to the purposes of and the enterprise.

The training went on through the autumn of 55. We were plagued by constant flame-outs at altitudes until we got a new version of the J-57 engine installed. Finally in April we had exercises at the end of which Bill Yancey's SAC organization was prepared to attest that our first detachment was combat ready. We had a number of negotiations in the meanwhile, this time overseas. Originally Anthony Eden agreed that we could base the first U-2 detachment in England, the first one was deployed there in early May or late April of 56. The Frog Man Episode occurred about that time and Prime Minister Eden had cold feet. So he decided that whereas he would be a reluctant host for a little longer, no overflights could be flown from British territory. General Cabell and I waited on Chancellor Adenauer, who even without interpretation, displayed quite shamelessly his evident relish for this

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plan and offered us the hospitality of the Federal Republic of which we promptly availed ourselves. So the detachment was moved over there and established early in June.

In the latter part of June we were in readiness; I guess we had four aircraft. I made the first of what were to be many trips to the White House to get authority to operate. I went with Allen Dulles, of course, Cabell, the Secretary of State was there, the Secretary of Defense, and I guess, the Chairman of the Joint Chiefs. This was discussed and, of course, the President was fully familiar with the program. Finally the meeting broke up and I was told that Andy Goodpaster would advise me in due time of the President's decision. Two days later I was called over to Goodpaster's office, and he said the President has authorized you to operate for two weeks. Well, I said, that is absolutely wonderful and I assume that for every day of bad weather I get you add one day on. In other words, I said, I assume this means 14 days of good weather operations. No, Andy said, it doesn't mean any such thing at all. Two weeks from today your bank account is closed out, period. Well, I went off a little disconsolate at this. I had hoped to get two months or more at this point. Later as you all know, we came to regard this as the best open account we had ever had.

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As I remember it for three days I sat through go-no-go briefings in the middle of the night and the weather was really not-- you couldn't even under this kind of a situation with a wasted asset of time, authorized. Finally we got a good forecast and the boys in ops had drawn up a mission, the first one to be flown over USSR, and it went right straight over Moscow. I said do you really think this is wise and they said why not. You better do it the first time, you may never have another. So I said okay, we'll go over Moscow. I went home and slept very soundly. When I came into the office in the morning, I sought out my boss Allen Dulles and said Allen, we've got a mission this morning. It is in progress now. I haven't heard anything from it yet. He said where is it going? I said right over Moscow and then it is going up to Leningrad just to make doubly sure. Well, he turned pale. He said, oh my God, maybe you should have asked me about this. I said no, no I shouldn't have. You remember the rules now. So we spent a nervous hour or so until we got a favorable report. The only thing was that there had been cloud cover over most of Moscow. We got a few pictures through the clouds that day, but that was all. Well, we ran either, I have forgotten, five or six operational missions including two on the same day on one occasion.

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Before my two weeks were up, we had a diplomatic protest from the USSR delivered to the Embassy here. It was all very private and very polite. One of the unpleasant discoveries we made after the very first operational mission was that this aircraft was tracked through most of its mission. We had hoped that Russian radar capabilities wouldn't permit consecutive tracking or anything like that extent. Most of those first operational missions were tracked. I don't know whether they realized on the first one what was hitting them, but they certainly did after a few. When the note came in, we were stood down and that was the beginning of course of the long standard phase of the U-2 operation.

I apologize for this anecdotal description and most of it has no relevance either to technology or anything else. I want to get back, and I want to finish what I have to say in a historical vein very quickly. We had after that first series of missions in the summer of 56 sporadic missions from that time on. The President personally was shown a map of every proposed mission every time. He quite often made specific changes in them himself, in short, if anyone need be told, one moral of this story is that if you're going to do anything this sensitive it is going to get its approval, if it gets it, at the very top. The scrutiny is going to be detailed and what you really can hope for

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bureaucratically, but rarely achieve, is to cut out the intermediate layers. Throughout Eisenhower's period in office these things always went to him. They were always taken to him by Dulles, Cabell, and myself. The Secretary of State or his Under Secretary was always there, the Chairman of the Joint Chiefs or his acting, the Secretary of Defense or his deputy and Andy Goodpaster. Goodpaster was sort of the liaison with the White House on this. This it seems to me was an eminently proper way to control a sensitive activity and indeed from the standpoint of anyone engaged in it, despite the frustrations of frequent negatives and the long standdowns, I suppose in retrospect, this looks like the Garden of Eden Lost it was so much better than the elaborate committee system that has gradually come to prevail.

Just a few other landmarks and chronology and then let me talk for a few minutes about some of the lessons and so on. That very first summer by late July with one operational month under our belts, before our second and third detachments were even deployed, (the second went to Adana that fall, and the next one to Japan the following spring), we started on the next technological chapter. The first one was what turned out ultimately to be a well, I guess, I'll call a wildly aborted effort to develop a radar absorptive coating which could be applied to the U-2.

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We did the basic work in an organization, in a loft in Cambridge, that later became SEI and they had really quite extraordinary and quick successes. What they did, in fact, develop was a thin material with a metallic pattern printed on plastic which on flat surfaces was extraordinarily absorptive with radar radiation. It was light enough with its substructual attachments to be used as a covering on large parts of the U-2 aircraft without too much effect on their aerodynamic characteristics. This development was done in the fall of 56. By the spring of 57 we were experimentally installing this on U-2's. We secured the use of a radar site IN THE south of Nevada; we flew the U-2 against it; we developed our own primitive instrumentation at Watertown to try to test the effects. And eventually we deployed U-2's so covered to the Adana base, and I'm inclined to think to this day that three or four of our more successful missions which went up on one side or other of the Caspians and weren't at all consecutively tracked, were attributable at least in part to the reduction in radar cross section by this method. Nevertheless by the following spring, I guess we are up to 57 now, it was quite apparent to all of us that this technique was inherently of extremely limited value. One reason being it was frequency specific. It was fairly narrow banded in its effect. And with the variety of radars

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that the Russians had, many of them old World War II devices, it was quite impossible to even conceive any kind of covering that would work.

And so that soon, I began casting about with my then deputy who had the job that Jack has now, then Col. now General Gibbs for a successor to the U-2. Our early approaches to this conceptual process were amusing. Jack got his flying time in, usually on a C-47, and I went with him. We visited various people, a few in industry, mostly in the Air Force. You know one-if-by-land and two-if-by-air kind of visits, simply to talk the proposition to them - what kind of a vehicle would you think about for this purpose. We ran into one idea that I remember that would sort of illustrate the extent of the spectrum. This was one that Northrup Aviation had been toying with. There was a gentlemen there who was something of a fanatic on boundary layer problems. They were talking about a perfectly gigantic beast that would fly very, very slowly at altitudes up to 80,000 or 90,000 feet, so they hoped, with the help of boundary layer control. I think this was the design that had the kind of a bridge truss on the upper side of the wing as the only way to give it necessary structural rigidity. I often felt it would not even achieve its design speed which was about as slowly as you can go and stay up in the air. I'm trying to say that we sampled the whole spectrum of ideas and

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power plants and aircraft but in a way that would make any really up-to-date Pentagon programmer howl with agony. It was all done by word of mouth and was done informally and we would go to contractors and wouldn't tell them who we were. We would say we were vaguely from the Pentagon. Some believed it and some didn't. And we said we might have some money, which was the most you could possibly say because we certainly didn't have any, and we would ask them for their ideas and we were quite clearly shopping for ideas. We weren't going to have a contract definition, and we weren't going to have a pre-contract definition phase, and we made it perfectly clear if one guy had a good idea we certainly weren't going to tell his competitor, let alone ask for a competitive design for that concept. We got a lot of good ideas, and it was quite remarkable some of the people who would unlimber to us ideas that they had been really quite unwilling to reveal to their closest colleagues. I felt this was an interesting and very constructive summer that we spent and then we began to zero in on the ideas that later became OXCART, and by I guess late 57, I felt sure this was the route we wanted to go on the successor. I will mention another historical incident because it was a gimmick and at the time it was one that worked awfully well.

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In the fall of 57 I told my boss that we had reached the stage where we now had to have some examination of our ideas by people whose authority would give them substance and lead hopefully if they had merit to their adoption. So there was established another one of these advisory committees headed by Din Land which was advisory jointly to the Secretary of Defense and the Director of Central Intelligence. Land was the Chairman, Purcell was on it, Al Donovan was on it, Perkins of Princeton was on it and quite a few others. And beginning in the fall of 57, over a period of a year and a half, they had about 7 or 8 meetings of this group. It always met in Din Land's office in Cambridge and from the beginning I would invite to these meetings the Assistant Secretary R&D from both Navy and Air Force, so that both of the then flying services were represented, and I also invited the Assistant Chiefs of Staff, R&D from those two services. This proceeded very much as I hoped. We had narrowed it down then to two contractors, which were Convair of Fort Wayne and Kelly Johnson of Lockheed. There was a tremendous evolution in their concepts from the first meeting through to the latter day of that group. And a year later just before November of, I think, 58, this group felt that they were ready strongly to espouse the project which later became OXCART. Well, they were about to

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break up our meeting shortly before Thanksgiving on this note. Again Din Land's and some of the other scientists view that if the scientists have decided, then things happen. I said it really isn't that simple. I haven't had you gentlemen to six meetings only for the pleasure of your company or even for the wisdom of your advice. You have got to have a written report. You gentlemen don't know quite how Washington works. Well, they said, we are not going to write any report. We aren't used to that sort of thing. Well, I said, you can goddamn well sign one. We'll write it. So we wrote our report in three or four days. It was a ringing endorsement of two pages and they did all dutifully sign it.

And shortly after Thanksgiving of that year, we had another meeting with the President and we presented this with the recommendation that there be approved, as I remember it, around $6 million, some such number as this, for a six-month study program. John Parangosky disagrees on some details here because he has another study program in mind. We weren't all that good. We too had successive ones but this was a crucial one. But the President's action was approval of the project in principle, and you are specifically authorized to spend this kind of money to see if it can be reduced to solid ground. So we went back to

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work. By the following June, and here John and I are back on the same track, we went back to the President still with two alternative airframe designs. At this point he gave his approval and a full go-ahead for the project. My recollection is that the tab we estimated then (and here we were almost as bad as some of the space characters) was $185 to $190 million. Well, this was approved. We still, however, had the job of source selection ahead; and the final historical episode was in late August of that year. A source selection board consisting of the Chief of Air Staff, General White, the Secretary of Defense, Joe Charyk as Assistant Secretary for Air R&D, Gen. Cabell and myself met; and we unanimously selected Lockheed and about the first or second of September, Kelly got his second go-ahead.

Well, my apologies to you all for an account that I suspect was more nostalgic and entertaining to me than to anyone else; but let me take a minute now to mention, you can call them, lessons of some of the things that I think are the accomplishments of this program as it unfolded or some of the implications that still have political.... The first is sort of the way it began. I don't think anyone could ever recreate this situation, but it is a scene I described to you in Trevor Gardner's office. There was absolutely nothing in writing. I had been told the

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President had approved this program and everyone sitting around the table seemed to have received the same information, but there wasn't any piece of paper that directed anybody to do any specific thing or to spend any particular money. If there is any key lesson that this Agency ought to draw, it is that the Agency's reserve is a potent weapon. If you want to be narrow about it, you can say it is a potent weapon for advancing the interests of the Agency. If you want, as I rather prefer, to speak as a citizen, it is a goddamn potent weapon for getting something moving fast if the national interests ever call for it. Now the Government may have changed more than I think since I left it; but I can only say that in that enlightened day in the middle 50's, there wasn't anybody else around Washington that could get anything moving fast. There were then, I think, rather more people around Washington, and Trevor Gardner was certainly one of them, who were willing to get things moving fast. There wasn't anybody else who could; and more than once, the Agency's reserve made exactly this kind of thing possible. You know old men always like to comment on retrogressions since they were active; but believe me, it is as a citizen that I regard it as major retrogression that I haven't heard in the last five years of any single case where the Agency's reserve has been used massively to get something of

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national interest moving fast without all the reviews and all the phases that they have to go through.

A second is not so much a lesson as a historical comment. In the negotiations with the Air Force that I had just about the time of the U-2's first flight, that I have also referred to, a concept emerged which really worked well for five years. The U-2 project was quite explicitly set up as a joint Air Force/CIA project. The document which is in your files, and maybe all of you have read it, says, "There shall be a Project Director appointed by the DCI and a Deputy appointed by the Chief of Air Staff and they shall be responsible to the DCI and the Chief of Air Staff for their conduct of a joint project." In other words, throughout, especially the U-2 phase, the Air Force wasn't just in on this as a supporting element, and to a major degree he wasn't in on it just supplying about half the government personnel; but the Air Force held, if you want to be precise, 49% of the common stock. Quite aside from interdepartmental clearance obligations of the normal sort, I had to clear every major policy decision with two bosses. It was done, and it did work, and it worked extremely smoothly and well. Whether it ever could again is something I won't comment on because I don't know.

The third historical comment--I have already mentioned the hideous

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figure about OXCART, hideous because it shows we were as badly optimistic in our estimating, in all due respect, as the space boys. But it is worth noting that the original contract with Lockheed for 20 U-2 airframes and I think 21 million dollars was under-run by somewhere between 2 or 3 million dollars. Those happy days will never come again.

Next a problem, and I think an accomplishment--Jim Cunningham from one vantage point and John Parangosky from a very different one. One from the Washington end and the other from the deserts of south eastern Turkey will both well remember that we suddenly found we had to put into the field detachments of approximately 200 strength each, which were roughly 1/3 CIA civilian personnel, 1/3 Air Force uniform personnel, and 1/3 contractor personnel. These people had to preserve the tightest kind of security. They were expected to achieve a standard of maintenance that three successive SAC Colonels fresh to the project admitted were above any they had seen achieved in a 100% military operation. To do these things, they had to be disciplined and a damn hard working organization. Yet you had three different kinds of animals in them. There were, of course, the usual incidents with drunks and fights with local citizenry and the like. But more than that, what we had to cope with, somehow or other, was that all three pay systems

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were different, all sorts of standard arrangements for fringe benefits, including most notably R&R leave, was totally different and so as James Cunningham used to remark we ended up running a narrow-gauge airline from Adana to West Germany. We sort of averaged the regulations up until each of the three components was getting all the privileges it was used to under its union contract, plus all the privileges that both of the other union contracts afforded. Remarkably, this was, needless to say, an expensive operation for the U.S. Government but I'm here to say it really did work. I think it worked as measured by maintenance standards achieved and maintained, obviously I think, by accomplishment. But, I think, it worked in terms of human relationships and morale. Most of the time these were very good outfits. In every case there was a commander who was an Air Force Colonel, and I would say that we were extraordinarily lucky in the men that were assigned to the Agency for this purpose.

We had one gentleman, if I may relax with an anecdote, and I presume I still correspond in great affection, Col. Ed Perry, who is no longer in the Air Force. When Col. Ed got to Adana, I think John over-lapped him in the work at that time, he was a hard driver and a very

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effective guy. But he disliked logistics and I mean he disliked it in the sense that he thought all supporting elements were out to get him and his outfit and anyway were scum to be spat upon. I sometimes wanted to collect our exchange of cables and publish it as an example in, I don't know whether it is, how to do it or how not to do it. But in any case, it was quite eloquent. As I remember, we got into the routine where Ed's cables would come in with their complaints Mondays, Wednesdays, and Fridays; and my answers would go out Tuesdays, Thursdays, and Saturdays. I think in the course of six months time, by which time we gradually got straightened out, I must have dictated fifty pages of cables explaining how things were to be done and why they weren't to be done this way. The climax came, I may say Jack may appreciate this, when I of all people had to make the finding on medical advice that Ed was grounded. Well, this produced the nearest thing to a volcanic eruption there has been in eastern Turkey for I think 200,000 years or something of this kind. It was felt on the seismographs all the way back here. But even that we managed to get over.

Two or three other quick ones. I think that one of our very greatest accomplishments, I take very little or no credit for this, in that period

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was the attitudes developed and relationships developed, attitudes towards, relationships with, contractors and suppliers. When this project started, as I told you, we had about five people in the government working on it. It was at least two years before, as a government organization, as a procurement office, we were in any significant degree staffed to the point where we could perform the minimum functions, by my personal standards, of what a procurement office ought to do. Obviously, therefore, in the early phases, this was wholly a contractors job. We funded; we helped with security. A certain number of major decisions were fed up, not nearly enough, but it has to be said that our collective contribution, out of all of us in the government, for what happened was pretty small. I think years later we settled down to approach a pattern that was very much more what I had wanted to achieve. It was one in which we still placed extremely heavy reliance on the suppliers. We expected them to do all the engineering. We expected them to solve, what I will call, the unambiguous technical problems that were there to be solved, but by that time I was in a position with enough help to insist that they bring what I call customers problems to the customer. My definition of a customers problem was this: that, when there is a choice to be made, if the choice

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can be and is translated into a choice of trade offs; performance versus cost; performance versus time; cost versus time; one kind of performance against another kind of performance; performance against risk; time against risk; but when a choice could be and was put in those terms, it then became a customer's choice. But I was equally clear that it wasn't the customers business to translate it into those terms. At least as we were then staffed, the customer was not the guy to try to say that shifting to the J-75 engine and adding 3,000 feet of performance would cost so much in range, or so much in money, or so much time, or so much in weight. The customer had to have profound confidence in the contractors to make these technical translations honestly and accurately. The contractors had to have enough intellectual honesty to do the translating and to bring the choices, when they became real choices, back to the customer. Well, this is an ideal as you all know. Maybe it isn't your ideal, but it is mine; but even as mine, it was an ideal. Nobody has ever made Kelly Johnson willingly bring any choice to any customer. I tried for years and I made some headway but not a hell of a lot. Indeed the only way to make this sort of concept of a relationship work, the customer has to do things honorably to keep the contractor honest. I think my ideal example of this is the job that [redacted] has done in the OXCART

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program where he has lived with the engine builder. He has known and seen the problems. He has reported on them, and I call this keeping the contractor honest and this can be done. I think he, for instance, in this particular case, did it in such a way that far from being resented by the contractor it was, on the whole, appreciated. But it was an enormously important help. It provided an intimacy of communication that alone makes this kind of a relationship pay off. I do think we built a very solid edifice of mutual confidence in the U-2 program, and I think it carried over.

One more and I'm through on this. One of the great strengths of the Development Projects Staff, at the moment when it was still that (when it was operating the U-2's, and developing the OXCART system, and before its functions had widened), was the degree to which it was a self-contained organization. I touch on one of the oldest organizational dichotomies known to man. I strongly suspect that long before Adam and Eve died there were enough people on earth so they had committees and jurisdictional disputes. And in all organizations, and I could document this in some way, there is one school of thought that says lets organize things functionally. If it is a private company you have a great big sales department that sells everything, and you have an engineering department

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that may design everything, and a manufacturing department that manufactures everything. The other principle of organization, there are many others but the one I wish to contrast, is the project principle, where you put a slice of engineering and manufacturing and sales and in, of course, a government office contract management and security and various other things, and you take a slice of each of these, and you put them in an office that is devoted to one or to very few end objectives. Well, DPS was one of the most extreme examples I have ever known in the government or anywhere else of a project organization. I am not going to try to erect some Parkinsonian general principle on the basis of one experience, but I will say to you that for its purpose and in its time this self-sufficiency paid off handsomely. Again to make a remark that I wouldn't make in the same tone or at all outside of this group of people, it was particularly valuable that we got the essential procurement functions of contracting into the project. And now, I will really defend to the death the way we performed that function and the way, to the best of my knowledge, it has performed ever since. I have never heard any substantive criticism; I have never heard anybody who said you were too easy, or you were too tough, or you were too fast, or you were too slow. But the fact that it was in there made a great deal of difference.

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Then take another function--finance. We had in the early days a gentleman called Douglas Ogan, known to a few of you rather affectionately. Ogan was regarded as something, well he was regarded as what shall I say, as slightly irascible by his colleagues in the project; but mercifully, this was as nothing to his reputation with other people. Ogan wouldn't show the finance officers of the Agency . . . let alone any other financial type. He wouldn't write down that two plus two makes four for them without an explicit authorization from me, which I rarely gave, and which he resented if given. Ogan took the view, which I shared, that there ought to be a twelve-month open season on budget officers. They should be shot when encountered and there should be a bounty for two ears. I would say that the finances of the project were sternly and admirably run and they were run without assistance if it is to be called that, from outside edifice. I think Jim Cunningham could attest to the fact that the budgetary process was not exactly a smooth one. He used to come over with a number of colleagues from the budget and pencil on yellow paper in my office. We would have explosive meeting after explosive meeting, but at least they were in the family and we didn't constantly have to go outside.

But aside from these in part entertaining examples, one of the

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most important lessons that I draw from the self-containment of this project organization was that we had it tightly bound up here. There were roughly 300 people overseas and 150 people in Washington in the whole organization or something of this sort. The number that sticks in my mind is around 500 for the whole of DPS, of whom at least 300 were out of the city. Here we have bound up in this comparatively small group everything that was being done in the way of development and, be it said, training of people and procurement and the actual operations in the field itself. And this contributed to a speed of feed back in the whole operational phase which I think was one of the successes and accomplishments of this era. It meant specifically that the contractors were supplying equipment. (I really think a closer and more effective relationship with the operating units in the field than is typical). It meant that senior contractors, senior engineers would go to the field, not just to do trouble shooting on the equipment there, but to come back with ideas for modification. This whole process of feed back, from the operational experience to the modification and to the flight testing of the modification, was very rapid.

Well, I think gentlemen, I have said too much about all of this.

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I'm afraid that it has probably sounded to you that it was too self-serving. I suppose to balance this off I ought to have a number of lessons to set forth as to things that were done wrong and what one shouldn't repeat. There is one major lesson of that time, but it is implicit in what I have already said and what I said at the very start. If you are setting out to do this, you wouldn't start in with only five people. The greatest single weakness of this program, which was still not overcome for either the U-2 or the OXCART program, (even when I left the Agency, in fact, it was quite far from being overcome) was that we didn't have the minimum technical staff in the government to do things the way I felt they ought to be done. Now mind you, my notions of a minimum would be in terms of numbers very, very small indeed, very obscure. But we would have been better off, and this project would have gone better if we had had a somewhat larger and highly competent, but still modest, technical staff in the procurement office. So in a sense the main deficiency, that I would point to, is one that has been overcome.

I have taken much too much of your time. I feel apologetic about it, but, nevertheless, let me entrench on it for two more remarks. They are no longer history and are, what I said, a few obiter dicta

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on the future. And I'll make just two of these and they really don't have anything to do with the examples, especially the U-2 examples, that I have been talking about. The first is that no one, certainly no one around this table, can look back to the beginning of the anecdotes that I have been telling from December of 54 to October of 65 without being aware of the really incredible advances in the intelligence community resources in overhead reconnaissance that has taken place. And this has gone from practically zero to being, I suppose, the most important single source of intelligence in the whole intelligence community. It has gone from a program which in the first full year of the U-2 operation involved the obligation by the Agency plus the Air Force of maybe 30 million dollars to a program, I don't know how it's carried on the books today, but I am perfectly sure that people like McNamara regard it as responsible for at least a billion and a half of expenditures in an annual, or obligations at an annual rate. Along with that has gone more or less a comparable change in capabilities.

Now a comment I want to make particularly to you gentlemen. I worry least we now begin to slip into the fallacy of overdoing something we know how to do, or if not overdoing it, over-emphasizing and over-investing in an activity because we have learned how to do it damn well. We still see large opportunities for the technical improvement

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of that activity and for making it still more valuable. I submit that it's quite possible that five years from now the things that the intelligence community should be most vigorously trying to discover or trying to find out are things that just plain aren't seeable from overhead at any resolution and are not even inferable from anything you can see from overhead. There are friends of mine who argue that the next strategic revolution that hits us is going to be the result of MIREVing our own ballistic, strategic, offensive weapons and more particularly assuming, believing, or proceeding on the assumption that the Russians have MIREVed theirs or are about to. What I mean by MIREV, of course, is Multiple Independent Re-Entry Vehicle, and the argument that has been presented to me is that when the single large payload missile is given multiple re-entry vehicles with independent targeting that then in effect two things have happened. First, whichever side does this has greatly multiplied the number of targets he can attack. But, secondly, he has done so in a way that can't be photographed from above the ground, because the damn things are in a silo and when the lid is on you don't know whether it is a single war head or a multiple. In fact, even if the lid is off you don't know that. So you can't just go around and count silos and know that you have got enough Minutemen

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to do the job. You have to find out some things that you can't find out that way any more. Then there is the whole question of perhaps a higher priority on intelligence concerning ground forces. And with due respect to Lundahl and his associates, I very much doubt whether any amount of overhead photography will enable them either accurately to estimate the number of Soviet ground forces currently, or, still less, the mobilization reserve of weaponry in being that would permit an expansion of those numbers. Then there is the question of whether someday somebody can develop a system for mid-course or exo-atmospheric discrimination of an incoming missile ... probably employing laser techniques for this purpose. Now whether this can be done is just a question of this point or whether and when the Russians start trying to do something of this kind is not going to be revealed by overhead photography until it's pretty well along. And there are others that one could mention.

Now I submit that there is a very real possibility that in a few years information bearing on matters such as this will come to be recognized as the highest priority of intelligence requirements, and people will suddenly realize for reasons quite inherent in the nature of the sensing techniques, overhead photography is not going to

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provide the answer. Now this doesn't lead me to suggest, even as a remote possibility, that somebody is going to say that we've had it with reconnaissance. We'll call it off and do it with something else. That isn't the point. I'm talking to a group of R&D'rs and I'm saying that I think it would be a great mistake if all of our forward thinking were focused on this. And then this problem of, A, can you get three feet instead of eight feet and, B, how much is it worth. I think quite a lot of the things that you get, I hope, will be thinking about the requirements that perhaps can only be met by wholly different techniques. And may I say, and this leads me to my next and last comment, for God's sakes, as a group of developers and researchers don't wait for somebody else to tell you what the requirements are going to be, because I think you all know why that system doesn't work.

My last comment is an organizational one and if my count is correct the third that I wouldn't dare make outside of this room and perhaps I shouldn't even make this inside this room. As I think Bud Wheelon knows, my last act before leaving the Agency was to recommend to them directly that his job should not be created. You understand he wasn't the candidate so this isn't quite as personal as it sounds. Now what I'm talking about here is what happened and this is theory, I don't know about this

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you perhaps do. When you have in the Agency a DD/S&T and quite separately a DDP, and my hunch is that what happens absolutely inevitably is that one group of people called scientific and technical people, researchers and developers become as it were spiritually somewhat separated from another group of people who are operators. And I feel that this is almost always retrogression when and if and to the extent to which it occurs. At least it may not be if we are talking about one of the national laboratories or university laboratories or something of this kind. In a government organization, I think, this is almost always a retrogressive step, except for something that is set apart as a laboratory and is supposed to be something of that kind. I think the reasons are fairly obvious and there is no particular reason to go into them. But I have to take one more dig at one of my favorite conceptual hates which is the requirements concept as it tends to be used. Because I have always felt that the reductio ad absurdum of this separation of development and research on the one hand and operators on the other is that sooner or later someone says we must, of course, impose order on this inherently disorderly world and so since we want our researchers and developers to be usefully employed and not wandering off inventing things that happen to interest them, but have

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no relevance, we are going to have a requirement system. Now the operators have a job to do and they know the sort of tools they want to do the job and so the operators are going to generate requirements and it is the job of the technical people to meet the requirements. It is all very tidy and orderly and, of course, you then develop hierarchies of development, the preliminary requirements, tentative requirements, specific operational requirements, unfunded requirements, funded requirements, and so you go down the line. Now this has always seemed to me to imply that you ask a man who's busy with his day-to-day job, and who wasn't hired to invent anything, for Christ's sake; he was hired to fly an airplane, or he was hired to be a case officer for an agent, or he was hired to write propoganda, or he was hired to write a national estimate, anything but to invent - you ask this man to write a requirement for something that isn't invented yet. If you don't invite him to write requirements for the things that haven't yet been invented, then all you have done is to tell your technical people that there is a law against inventing anything which is presumably what some of them hopefully think they were hired to do. Mercifully, requirement systems in practice don't work. Although I contend that we are currently in a phase, certainly in the Pentagon and I strongly suspect in this building,

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where the requirements system is working more effectively which means it is suppressing original thought more effectively than it normally does. A great deal more effectively than it did in the middle 50's.

So I will close with this plea to all of you. This Agency still has a certain degree of intimacy. It is small compared to the Pentagon; it still has a certain degree of shelter from the public and Congressional eye. Why, there are still more degrees of freedom here on matters of this kind than there are, or probably ever will be, in the Pentagon. For God's sake gentlemen, use that to frustrate this tyranny of the conventional requirement. Don't wait for anybody else to tell you what needs to be invented because they will try to make you if they can, but it is a most important part of your job to struggle against that. Now I'm not here talking about any previous state of grace. I used to battle with these things in the days when the nucleus, the original embryo of this organization was part of the DDP and therefore was under my authority along with the rest of the DDP. There was a dreary piece of machinery then, it was a committee that met once a month. It had operators on it and it had some representatives from the technical side, and I'm hoping I'm not stepping on the toes

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of anyone here, but it was a dreary piece of machinery. I used to read the minutes of this and I occasionally went to its meetings, and operators would behave exactly the way you would expect operators to do. They said we've got a pocket recorder. It weighs three pounds and it is four inches long and two inches wide. Now what is to be the development objectives for the 1960's? A pocket recorder that is two inches long and one inch wide and weighs one pound. Or they would say we've got a pocket recorder but the goddamn thing has to be plugged into another unit to play back. The development objective is to have one machine that will also play back but is no bigger or heavier. Well, operators in the aviation business, all they have to say is go higher, faster, we want to carry more, and we want more range, and we want it to cost less. Well, it doesn't take any imagination to come up with this kind of stuff. This is not intended as any criticism of the operators as operators. It is a way of saying that operators are not the guys to dream about the future and, I think, collectively you people are hired for that and this is one of the few places left in Washington where there is some chance to do it. I hope for God's sake that is what you people do and not wait around for anybody in this building or anywhere else to tell you what is going to be needed in another ten years from now.

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