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Blasingame describes first his education, engineering work in the Army Air Forces during World War II, and his post-war graduate study in instrumentation at MIT sponsored by the USAF. He next discusses his career with the USAF beginning in 1952, including working on the Development Planning Objective (DPO) for strategic bombers while at the Office of the Assistant for Development Planning; the input of RAND, Cornell, SAC, and others into this DPO; and the relationship between the Office of the Assistant for Development Planning and other offices in DCS/Development. Blasingame then reviews his move to the USAF ballistic missile program, the input of RAND and others into this program, and eventually being placed in charge of the Titan program.

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Interviewee: Paul Blasingame

Interviewer: Martin Collins

Date: November 14, 1990

Place: Mr. Blasingame's residence,
Santa Barbara, California

TAPE 1, SIDE 1

Mr. Collins: I wanted to start out by getting a little bit of your background, and how it is you came to be associated with the Air Force and with then Colonel [Bernard] Schriever's operation in the deputy chief of staff for development. I see from your Who's Who that you graduated from MIT in 1950 with an Sc.D. in aeronautical engineering. Is that equivalent to a Ph.D.? I'm unclear on that.

Mr. Blasingame: Yes, that's what the the notation for doctor of science. It doesn't, I guess, carry all the weight of a Ph.D., but has most of the same basic requirements. It doesn't involve any of the arts or literature that a Ph.D. might.

I think I better change your impression there just a little bit. I went to the graduate school at MIT from 47 to 50, under sponsorship of the [United States] Air Force. I went to Pennsylvania State University, where I graduated in mechanical engineering and took some electrical engineering work at the time. I was there from '36 to '40, graduated, and worked briefly for Dupont. Not too long before the war, Dupont had committed themselves to making explosives for England at the time, so I went to an explosives plant.

In the spring of 1941, I was called to active duty because I had been a ROTC [Reserve Officers' Training Corps] student. I was called to active duty, I guess, in the Air Force. My memory isn't too clear on that. I was stationed initially at Tallahasse Air Force Base, Florida. In December 1941, I was transferred to Panama. After some years in the Air Force I read a bulletin, (I believe when I was in Panama) that said I could apply to become a regular officer, which I did. Then I became a second lieutenant or first lieutenant or something in the Signal Corps. It was a long time before I got back into the Air Force proper.

After coming back from Panama, I was fortunate enough to get an assignment in the Armament Laboratory at Wright Field. This was in the development activity, and it was specifically

associated with aerial gunnery, air-to-air gunnery, both fixed gunnery systems like in a fighter airplane, for example, and flexible gunnery, systems like a turret in a bomber. I was there I believe four years, but it might have just been three, I don't remember exactly.

I was fortunate there to work for then Colonel Lee Davis, later General Lee Davis, who was a very scholarly person. He studied and worked very extensively on scientific and technical matters as they applied to the Air Force. He and Dr. [Stark] Draper at MIT had worked together. I believe that Lee Davis perhaps was a student at MIT, a graduate student probably, with Draper, and together they worked on a computing gun sight for a fighter airplane that has been called the Draper/Davis sight in its early years. I think it more recently was called the A-1 computing gun sight.

Well, that set the stage for a number of things. My several years in the Armament Laboratory happened to coincide with the Air Force's beginning to realize that they had a lot of people in the service that would stay on, and they wanted to see them better educated, if you will, better prepared for their post-war assignments. So they started a very extensive educational program, which included sponsoring people to go to undergraduate schools, and more importantly to me, the opportunity to go to graduate school. It was a natural connection working for Lee Davis that I came to know Dr. Draper, and that MIT was taking students under this Air Force program specifically in Dr. Draper's department. So everything was going my way at that time. This was an opportunity to do something that I had, for many years, very much wanted to do, but of course, it had been seven years since I had been in school.

MIT, probably influenced by Draper, because we had been associated in work at the Armament Lab, accepted me for graduate work in his department. He sponsored a program called "Instrumentation," which was run by a faculty group consisting of the aeronautics department, the physics department, the math department, the EE [electrical engineering] department, and the mechanical engineering department. I think that engineering was part of it to make sure it included Den Hartog in our instruction series.

Collins: So you started there then about 1947 or '48?

Blasingame: I started there at the start of the summer of '47.

Collins: Who else do you recall as being in your class at that time? I know there were many Air Force people, and people who later came to be associated with the Air Force who passed through that program.

Blasingame: As far as I know, just one other in the MIT program stayed for the length of time that I did, and that was [G.C.] Clementson. We were there together for three years. There were

a number of navy officers there for shorter periods of time. A lot of our laboratory work was done jointly with some of the navy officers. We shared a study area in Draper's laboratory with them.

Collins: Now, this was a full-time program, and you were essentially stationed at MIT during this period. Did you know Bob Seamans during that time?

Blasingame: Oh, very well. Yes, not only were we students of his, but we worked together on a number of things. We collaborated on a few things. Clementon, who was an Air Force pilot, conceived and accomplished an experimental project using a B-25 on which we collaborated in the analytical work. Bob Seamans, as our faculty advisor, joined us in publication of our results in the paper, "The Dynamic Performance," Journal of the Institute of Aeronautical Science, Vol 17, No. 1, January 1950. I came to know Bob quite well.

Collins: Were you in this instrumentation program?

Blasingame: Yes. I couldn't just start in there, because I had been out of school too long. So Draper urged me, and very wisely so, to spend my summer taking a lot of preparatory courses, differential analysis and a whole lot of things that would be important to the program. Vector analysis, I remember, and two or three other things that he felt were background that I would need and that I had not had as an undergraduate. Then I was accepted into the program in the fall, as I recall.

Now here again the Air Force was not sponsoring people for doctorate degrees. They were sponsoring people for undergraduate and master's degrees. On the one hand, Draper was kind of encouraging me to stay for a doctorate, and I didn't know whether I'd be able to stay that long or not. So I just decided to take the chance that my assignment could be extended and planned my course as though I were going to have the third year there. Fortunately, the Air Force continued to sponsor both Clemenston and myself for a complete program.

Collins: What was the Air Force thinking there? Was it just a question of being able to spare somebody for three years, or was there a sense that only a certain level of technical knowledge was needed?

Blasingame: Thinking in respect to what? Sponsoring for the master's, and not the doctorate level?

Collins: Yes.

Blasingame: Oh, I just think that was kind of a practical matter, that they were making quite an extraordinary effort to sponsor people to go after master's degrees, and I don't think they anticipated people being out of the service, out of connection as long as three years as we were, and I guess it was

kind of a little bit of a special dispensation. It was undoubtedly helped by Lee Davis' sponsoring us and Draper's influence. I think it comes back to that. I think it was probably an exceptional thing.

Collins: Do you recall what your thesis topic was as part of your effort in this program?

Blasingame: Yes, it was entitled "Optimum Parameters for Automatic Airborne Navigation." It was an effort to establish what kind of navigation errors you would accumulate in long flights such as halfway around the earth, relying solely upon inertial navigation, and the disturbing factors being principally the wind shifts that would occur. I got basic data for several years' time from the local weather stations, global maps of weather, and was able to generate wind charts for selected flights or trajectories, if you will, over major distances and then work from that back, knowing the transfer function of the navigation system, which at that time was under active development in Draper's laboratory. We were interested in a navigation system that had a substantial amount of damping in it. So it was tuned to an eighty-four minute period, but had a substantial amount of damping. And the more damping you have, the more exposure you have to these external forces like wind shifts. So that was why there was interest in determining what those errors might be for different levels of damping ratio in the system. Is that approximately understandable?

Collins: Yes.

Blasingame: There was another school of thought. In the long run that school of thought prevailed, and that is that you could make instruments good enough--we couldn't at that time--and determine the initial conditions well enough, that you could go with essentially no damping. And, of course, that's the way it's done today--for example the Carousel IV airline automatic internal system. The systems operate without damping, so they don't have this external problem. But at that time it was very, very difficult to imagine making a practical system without damping, though some systems were being developed without it.

Collins: Were you working with a specific inertial guidance configuration that you were testing under various conditions, or was this more theoretical?

Blasingame: Yes and no. My work was entirely theoretical, but I used the configuration of a system that they were working on in the Draper Laboratory. Thus I had something from which I could determine the actual dynamics and its response to these disturbances. So it was based on that much reality of a system that I could actually determine how it would be affected by external disturbance.

Collins: This was a topic that was suggested by Dr. Draper as something that would kind of add to the line of investigation of the laboratory?

Blasingame: Much of our laboratory work was in the same laboratory where all this development work was being done, so we had a lot of exposure just by making acquaintances and friendships with the people working there and being interested ourselves in the equipment that was being designed and built. I came forward with this as a proposal for the thesis after listening and being caught up in the arguments of how much damping the system should have, and having been exposed to Norbert Wiener's theories of minimizing errors in systems and his mathematical formulations that let him specify the ideal system depending on certain noise levels, and I could see the connection between the two, and that's the basis on which it occurred to me to make this investigation, so I proposed it.

Collins: What then happened after you completed this program at MIT? What was your assignment at that point?

Blasingame: Well, I went directly to the Pentagon from MIT. I can't explain how I got there, quite honestly. I know that there was one influential person in the Air Force what wanted me very much to go back to the Armament Lab where I had come from. On the other hand, for some reason, there was interest in my being in the Pentagon, and that suited me just fine. In the long run I was just kind of a lost soul when I walked in the Pentagon for the first time. I worked on some armament problems there initially, but later got tied up with General Schriever in the early development planning efforts.

Collins: Can you recall roughly when it was when you began to work with him?

Blasingame: When it was? I'd be guessing a little bit but it must have been early in 1952.

Collins: I think that roughly coincides with when he assumed--

Blasingame: The whole thing was a very vague concept, not vague in Benny's [General Schriever's] mind I guess--but the concept of development planning was not very well fleshed out or understood or well-established at all. So when do you say when did you start, well, the start-up was kind of hard to determine.

Collins: What do you recall, if you can, of your initial responsibilities and general direction of the development planning effort as it was construed at that point?

Blasingame: Let me try making a little statement, and maybe then you can focus in from that. My principal concern, of course, was aircraft instrumentation, the navigation equipment, and the bombing equipment that would go with it. Now all that was being done in the context of what should the next strategic bomber be.

I'm sure you've read enough to know that a very heavy concern of the Air Force in those days was strategic air power. The Air Force had a very effective and experienced individual in the person of General [Curtis] LeMay, head of the Strategic Air Command [SAC].

There was a pretty well established pattern of what the Strategic Air Command wanted in the way of an airplane. By now they had the B-50, and people were talking about jets, and in fact, the B-47 was coming on stream, but what they really wanted was that great big B-52 that was going to come along next. They were interested in and completely committed to doing their mission at the highest possible speed at the highest altitude possible, as near as I can tell. There are many reasons for that. They felt that was the safest route in.

Others of us--and this kind of started around Schriever--wanted to make a careful investigation and see, really, is that the kind of an airplane that would give us a powerful strategic arm in the long run, or shouldn't we really look at the whole picture of an air defense that you're going to fly into, and what kind of an airplane might give you the best probability of delivering your weapon and surviving. So, my recollection is that that was the largest debate going on in the development planning office initially.

We later also got to looking at tactical airplanes, but initially the big press was on strategic bombers and what should the next generation be. Well, that kind of is what development planning is all about: what should the next generational weapon be? And the more we studied this--and a lot of this was very much with the assistance from RAND--the more we came to wonder if you should think in terms of making your penetration at very low altitude that you might be a lot better off. And the more analysis we did the more convincing it became to us that you should explore and see what you can do about a strategic mission done at low altitude, as fast as you can go at low altitude.

And this was in pretty sharp conflict with the Strategic Air Command's concept of what they wanted for an airplane and also with the people at Wright Field, and the general aircraft industry, I might say. And in particular with the aircraft engine business. They were completely committed to the high-speed jet airplane. Jet airplanes were just coming into their own then, and so it was a little nervy, I guess, for us to start talking about, what do you do to fly at low altitudes. In the long run, as I look back over it--I tried to think through when we talked earlier, what was the big thing that came out of all this development planning effort. Well, in my mind, the biggest thing that came out of the effort was the turbofan engine. You just couldn't make a low-altitude strategic run with a plain jet engine.

Now Cornell Aeronautical Laboratory was doing some work for us. They were doing sort of preliminary design of different

airplanes that we were looking at, and they were helping us determine what kind of range, what kind of payloads could you handle at low altitude. They came in to us and said, look, as long as you're going to use jet engines, you're not going to do it. You're not going to make a low-altitude airplane. So they got to talking about the turbofan, and as we explored that more, and more it became very clear that the turbofan engine would open up all kinds of possibilities for the Air Force, not just in strategic airplanes but tactical, equally importantly, maybe more importantly.

Then we went out looking for interested engine companies--we wanted to get industry into making some studies for us of what it took to make a turbofan engine. There weren't any then. We traveled to all the big engine companies, to Pratt-Whitney and the whole circuit, and only at General Electric [GE] Company did we find that they had any equipment big enough in diameter that they could even begin to make a turbofan engine. They had some huge turning machines that I guess had come out of their steam turbine work. They came from somewhere, but they could make up a pretty good-size diameter "turbine fan," much bigger than anybody else. And so we did interest GE in doing some study work on what they could build in the way of a turbofan, and what its performance would be, and therefore, then we could feed that back into the studies at Cornell as to what kind of an airplane you could have. So the engine is the all determinant thing in what you're going to have for an airplane.

Well, it turned out the turbofan let you go high subsonic at low altitude, and automatically at high altitude you went supersonic. I think I said that wrong. You go high subsonic at low altitude, and automatically, you had enough thrust that you could go supersonic at high altitude, maybe not as fast as a pure jet, but you still had a great range of capability with the engine.

And as I think back over it, I think getting that started--the turbofan would have come some day--but I suspect it was started five to ten years earlier, because of this effort, than it ever would have been. Of course nobody uses anything but a turbofan today. The original 707 was a pure jet; it had to have two-mile long runways to handle the thing as did the B-47 and B-52. Today nobody builds anything but a turbofan, so that the commercial airlines can get in and out of reasonable size airfields, and the SAC force can be more widely disbursed. It had enormous impact on tactical airplanes. It meant you could really get down to small forward-based airfields.

So I think the major impact of the office--Benny Schriever might disagree with me--but I think the major impact was getting this head start on the turbofan engine. It wasn't my area of skill at all. Well, in a sense this is deflating for me, because engines weren't directly my business, and yet I think the biggest impact that the office had was in that field, and I think that made almost everything else possible.

In addition, we looked at and explored some of these other matters. Of course, payload was all important. That also is a determinant on the airplane. The power plant and the payload pretty much determine what the airplane is going to be, and whether you can do it or not. I think you've already recorded in your interview with General Schriever that the other major impact of the office was in the payload area. The payload, of course, is just all important, and this is the other reason why SAC was adamant about the huge airplanes. They envisioned the payload as being a very heavy, very large weapon, and that's another reason why we found ourselves at odds with SAC. They were really glued in on that focus.

So an enormous part of our effort, including my own, probably more of my own time on that than on the kind of systems that you might put in the airplane, was involved in trying to get to the root of what would come along in the way of weapons. The people developing the A-Bomb were very, very reluctant to forecast what they could do. They always wound up doing many times better than they would quote themselves as able to do, but even when it was so important as to what kind of an airplane you're going to design, there were very few people in that business that would risk an opinion about it or have a scientifically derived opinion, as to what kind of weights, sizes and yields you were going to have in the future.

There were a few of these. Sam Cohen of RAND was one, and he did a lot of work with our office. He had the general impression that probably there were possibilities of getting a much more reasonable-sized, much more reasonable weight of warheads. His influence was pretty heavy on us, and it led us into looking very deeply into that aspect of our effort. This effort put us in touch with John Von Neumann and others who had a lot of insight into this, and who could give us some more reasonable forecasts. I guess a major part of my time in the last several years there was trying to put together and--not that I had any particular knowledge to bring to it, I did not--trying to understand some of the forecasts that were made and helping us form some kind of a fixed viewpoint of what would be available, and therefore, what did you have to provide for in an airplane.

Collins: You describe an interesting situation here where you weren't getting direct information from the AEC [Atomic Energy Commission] but where relying on the advice of people who had some contact with it, and because of their scientific standing could also make some kind of judgment about it. But obviously you had to be able to convince somebody like General LeMay that your reasoning on this was sound and would still allow him to do the job that he felt he had to do. How did that kind of thing play out when you were dealing with something like this, where the scientific backing was not as strong as it might have been?

Blasingame: It played out in kind of a strange way, kind of a surprising way. It took us from looking at strategic airplanes to looking at the ballistic missile. Suddenly, as we began to

see it, the warhead weights were going to drop dramatically, and this put back in focus the possibility of a long-range intercontinental rocket that could handle a very, very effective payload. In other words, it would reduce everything to a size and weight that made the ballistic missile practical.

In fact, that's the way it played out with us, and we had extensive support from people like Von Neumann. I think if you go back through Schriever's interview, you'll find a list of a number of people there that--we did have some people from the AEC that were in agreement, and certainly [Edward] Teller was. Teller was quite convinced that they would not be doing us a real service unless they were really more ambitious in their forecasts. I don't think Teller could ever be accused of being modest in his forecasts. [Laughter] And especially he was egged on a little bit by von Neumann, that's the way that played out.

Collins: Okay.

Blasingame: You see the strategic bomber that we were so concerned about, and so sold on, didn't get built, but the turbofan engine did, and it went into everything. The warhead thing played itself out in quite a different way by suggesting that maybe you had less need for the bomber, or at least you certainly could cover the worst, most difficult parts of a bombing mission with a missile.

Collins: There are a number of threads here I'd like to follow so let me just begin to go after them to follow through on this line of evolution of the implications of reducing the size of the warhead. Were you involved in the committees that Schriever established to look into this issue more critically? On the Scientific Advisory Board [SAB] there was a nuclear panel that von Neumann headed up and did the first study, and then subsequent to that was the so-called Teapot Committee. Were you associated with either one of those efforts?

Blasingame: Not on any kind of a full-time basis. I did get involved in some of their activities and meetings, but not as a member of the committee. No, not as an influential person in it, not at all.

Collins: What were your impressions of those exercises from your involvement?

Blasingame: There were personalities really, more than--I remember one time I was called upon to present to one of these study groups the projections we were using in our studies. There was somebody there from Sandia, but von Neumann was also there, and I got up and gave our projection, and one of these persons in the audience was just aghast that we would have such a forecast as that and challenged the view. I can recall it to this day, von Neumann said, "Now I really believe that what this young man has said--me, who knew very little about the weapons--is probably more accurate than anything else that been said here." He was, I

guess, defending his own position on it. That's about as close as I ever came to any of it. I didn't contribute anything directly to it. I was trying to put it together.

Collins: But that suggests even in these discussions there was still some difference of opinion.

Blasingame: Oh, yes. Enormous differences. There was a set of curves of projected weight that somehow had gotten exposure. It came from somewhere inside the Sandia Corporation. And they showed the projections not too different than we were using. It was unusual that those got into our hands somehow.

Collins: What were the nature of SAC's connections, LeMay's connections with this community that was looking at the possibilities of the downsizing of the weapons? Did they have their own contacts to the SAB unit of the weapons laboratories, or were they fairly removed from that community of researchers?

Blasingame: I cannot answer that question with any certainty. It seemed that they must be heavily influenced by somebody, because they maintained this insistence on these enormous payloads. I guess they [SAC] figured, well, if they get lighter, we'll take two instead of one. We'll still go out for it in a big way. These people all came fresh from the big effort in Europe, you know, and that was a difficult, hard fought thing, and they figured if you're going to go into the teeth of this scale, you better go with everything you've got. That's really not a bad philosophy, and I'm sure they figured if the payloads got lighter why, that would be just great; they'd put some in both bomb bays, not just one. [Laughter] That's speculation on my part. But these were hard-nosed guys, hardened in combat. They were running Strategic Air Command at that time and very powerful people and rightly so. I don't know where they got the information. I think probably they extrapolated everything, I imagine, from the World War II experience with the bombs that they took over Japan.

Collins: One thing I wonder, whether at this period, as you were looking into these potentially different paths of development for a strategic bomber, the issue of high versus low, and supersonic and subsonic, and the size of the payload, whether the issues about what technologies were appropriate to what war aims, what were your strategic objectives when you went over to the Soviet Union? Did it make any difference about what targets you were going after, whether you went in low or you went in high? How did that kind of consideration play out? There must have been some differences there. Did that enter into the discussions?

Blasingame: I wasn't involved with any of the target selection or anything like that that would have influenced how you went after the targets. That's all done at SAC. This is all a high-level classified kind of thing. I can remember that a lot of us debated about what kind of targets, but we weren't influential in

any way, and it didn't enter into our talk about what the next airplane should be.

We probably got into a more controversial position with SAC over an interim recommendation we made. We were suggesting that a modification of the B-47, I think it was called a B-47X, it would be refueled, that we commit ourselves in a strategic bombing mission to in-flight refueling, and that this was the most economical, most cost-effective airplane to buy, and committed to a fleet of in-flight refueling airplanes. KC-97, I guess it was.

We ran headlong into a difference of viewpoint with LeMay on that. This was another aspect of their big airplane business. They did not like the idea of refueling. They were doing it only because they couldn't get there any other way. They were refueling simply because they couldn't get the range any other way. So it comes back in, they wanted a great big airplane with lots of range, lots of speed and lots of payload. That would be a SAC man's dream come true. You just couldn't get there with the technology of those days. But the B-52 is about as close as you could get to that and here we were, a bunch of upstarts, saying, "Look you can achieve this objective with a little extension of the B-47 using the lesser payload weights that you'll have and committing yourself to refueling." And that was not a popular view with SAC. It ran completely against their concept of what would be the best machine for their idea of SAC.

TAPE 1, SIDE 2

Collins: I wanted to talk a little bit about the internal organization of Schriever's organization for carrying out one of these development planning studies. How would the case of the assessment of what the next generation of strategic bombers ought to be--how was that organized? You seem to have suggested perhaps that it was broken down into components that looked at different subsystems and the overall character of the airplane, or something along those lines.

Blasingame: Well, as near as I can recall, reflecting back over the years, we had nothing in the way of rigid organization like you'd expect to see in a normal organization chart or something like that. Realize that we were a very young group. Schriever and his group had left Washington a mere two or two and a half years after we started the whole thing, so we didn't get all the time to build up all this neat little organization.

Instead of that, Schriever thought more along the lines, I believe, of bringing in specialists. So that's why we went to someone like Cornell Aeronautical Labs to look at what could you have in the way of airplanes, why we would go to RAND for special studies. As near as I can tell, Schriever more or less was the head of the SAC examination, and he brought in a Colonel McGowan-

-then colonel, I guess now general--to head up the Tactical Air Command [TAC] study. McGowan, I believe, had come from the Tactical Air Command. I'm not too certain of that but I believe so. And he brought in a civilian by the name of Mike Chaffee, who had a lot of experience in the ground electronics that you use in forward operations in directing a fighter aircraft from a large ground-based radar. And that's kind of the way he operated. There was more emphasis on bringing in somebody that had a lot of experience in a particular area, than it was any kind of a rigid organization.

Collins: Perhaps the report that we've both had a chance to look at, this study of the Deputy Chief of Staff Development, conducted by the Office of the Secretary of the Air Force, dated 15 July 1956. They go back and suggest an organization for the very beginning of the DCS Development and the system for development planning, and you get a sense there's a little bit of organization there, but that may just be organization charts.

Blasingame: Could you tell me where that is?

Collins: Let's see. That should be fairly close into the beginning of this thing here. Yes, just after page 12, there's a series of organization charts.

Blasingame: You mean where it says the Director of Requirements. Oh, I'm familiar with those organizations. Yes, they were very much in place and had been a long time in the headquarters.

Collins: Well since 1950, when that DCS was established.

Blasingame: Oh, now I had the impression it was longer than that.

Collins: The precursor to Schriever's operation is the one entitled Assistant for Evaluation, which was then changed to the Assistant for Development Planning. That's several pages into it. But that's how it existed in 1950 when this organization was first set up.

Blasingame: I'm embarrassed to say that I don't recall the formalities of that. That title of Assistant for Evaluation, is not an unfamiliar one, however.

Collins: I think Ivan Getting first served in that role, and Benny Schriever was his assistant, and then the name of the office changed, and Colonel Schriever became the Assistant for Development Planning. So what you're suggesting is that it was a failry loose kind of arrangement. During this period, RAND was fairly active in assessing a lot of different elements of the evolving technologies of aircraft, both bombers and fighters. What do you recall about their involvement in the things that you were working on?

Blasingame: Well, they were extremely helpful. They actually put people on extended assignment in our office. Gene Root, for example, who was a high-level person from RAND, I beleive from the aircraft design aspect of it, actually spent a better part of a year in our office, as I recall, to assist us in putting a lot of this together. Jesse Marcum and Sam Cohen both were given desks in our office space there, and they were there a good deal of the time. Whether they had some kind of a fixed assignment there, I just don't recall, but they were there a good bit of the time, very often. So RAND support of us was really quite extensive.

I can't see the name of the guy who's head of Northrop, Tom--

Collins: Tom Jones.

Blasingame: Yes, Tom Jones. We were also interested in large transport aircraft, and Tom Jones was working for RAND at that time, as I recall. He was put in there to lay down what could be done in the way of a very large aircraft, and I believe that that had ultimately some influence on committing to an airplane as big as the C-5, for example. I can't trace that for you because that sort of came after my time. I think I was in the office with Tom there for about a year, but I believe he was put there by RAND. In addition to all of that of course, RAND here in Santa Monica did many studies, on penetrability and the kinds of targets you ought to be going after. I can't recall all those studies, but they were deep into study of a lot of the aspects of this, and we could have available to us their results. They were very helpful in meeting with us, or having us meet with them in Santa Monica and understand their studies better.

Collins: One of the interesting organizational and intellectual problems here, seems to me you've got a lot of input coming from a lot of different directions. You've got different RAND people doing studies. You've got Cornell doing work for you. Some industry people. With your ultimate goal, as you want to pull together something that will allow you to say, this is the way we ought to go in this area of technology, say for a strategic bomber, who or how was all these disparate inputs pulled together, and some coherent position established?

Blasingame: Well, let me jump to the end of that cycle, and that is how do you get out a report, and I trust you must have looked at the Strategic Development Planning Book.

Collins: I haven't seen that, no.

Blasingame: Someone ought to make one available to you. I got Jackson and Moreland, I think was the name of the company, that was doing this kind of work for Draper at MIT, to come in and help us pull all this together and get it presented in the way that would be useful and to give us some discipline about getting the material together. They they put quite a high-level

gentleman in our office to do this. He wasn't an expert in any of these areas, he was just a superb technical writer and editor, perhaps is the best way to describe him, and he was a very good disciplinary force on us to get our thoughts together and give him chapter after chapter that he needed to put this thing together. I suspect that that had more influence on how it all got put together than a chart or something might have had. He would call upon each of us with, "Will you get this piece of it done," or he'd be complaining about it. "I don't know how to finish this chapter. It doesn't make any sense the way I've got it." I'm sure that that was an important part of how you got it put together?

Collins: Did you have some administrative responsibility for helping to see this part of it through?

Blasingame: I spent a lot of my time working with him on that, writing and trying to get it assembled.

Collins: I guess another way of phrasing that, was there somebody who served as a coordinator or administrative focal point for overseeing all the work that was going on, in a sense, or just keeping track of it?

Blasingame: Well, to the extent of getting organized so we could put out a report, and to being the internal driving force behind getting that put together, yes, that was one of my responsibilities. But I in no way was organizing the office or anything like that. It was a kind of indirect way of saying, "Look you've got to ultimately have a product. You can't just go around and talk about these pieces; you've got to put it together." And that's the way it was finally put together.

Collins: Okay. You read through this document again that I've referred to, the study of the Deputy Chief of Staff Development, and one of the things that at least comes through in this prose is the importance of connecting up the development planning activity with strategic plans and operational plans. What was the nature of your contacts, or the contacts you are aware of with these other elements of the Air Force, to try to bring some kind of coordination between these different elements of planning?

Blasingame: In the time that I was involved--I don't know what happened later--but in the time that I was involved I would say that we didn't do very well in this field. Like I tried to say, we didn't influence SAC to support the bomber that we thought they should have. They went in an entirely different direction. They went their own direction, which had already been cast long before we were in existence, so we did not have that kind of a direct impact on them. Our impact was totally different. We did, however, gain some support from ARDC [Air Research and Development Command] in Baltimore on the engine development. Our impact is only seen after some years, and that's the turbofan engines and the small bombs, and that made a lot of the rest of

this happen. But I guess I just have to say that I don't see that we changed the Strategic Command's viewpoint in the short run--we did not win their support.

Collins: I wasn't so much trying to direct you in that way, as to just look at what the nature of the interaction and coordination was. I mean clearly there had to be some sharing of information, some meeting of the minds, and just a general sense that we're both working at least roughly towards the same kind of goal here.

Blasingame: I'm going to have to ask you to stop the machine just a moment.

Collins: Sure.

Blasingame: Now let's ask me the question again. This is how coordination is done with the actual commands?

Collins: What the degree of coordination there was, as you recall it.

Blasingame: It was not for lack of effort. Schriever made a number of presentations to the Strategic Air Command staff that I'm aware of. But we generally were--I felt generally we were rebuffed by SAC. Lemay and the entire staff just could not bring themselves to a viewpoint parallel with ours at all. As I say, I presume that it was born of their own experience, of when you go in there and get all beat up trying to make a bombing attack, you better take everything you've got and see if you can't get over the first time and not have to come back and go in there big and strong, and they just couldn't get in tune with the idea of a different way of doing it.

In fact, their reaction to the ballistic missile was the same. They were not supporters of the ballistic missile in its early development. It was a long time before they saw in the ballistic missile a real possibility of doing a major part, not all of, but a major part of SAC's job. I think it probably wasn't until the Minuteman came along, when they could have these things in enormous numbers, that they felt that it was appropriate to say, this is a major part of the strategic command or strategic capability.

I can cite some very amusing incidents that happened in some of those briefings, but I don't know whether you'd want them retold or not. Well maybe I'll tell one, and I hope you'll scratch it if it makes trouble.

On the refueling aspect of it, General Jewell Maxwell, Colonel Maxwell then I guess he was, great big guy, bomber pilot in World War II, and he looked like he was eight feet tall to me. Jewell Maxwell, I'll never forget it. He had become a champion of the "probe and drogue" refueling mechanism, rather than the big boom. There were two different refueling systems, if you

recall, and he thought there was flexibility in this one, particularly if you were going to talk about smaller airplanes, that this was the way to do it.

So we were sent out to brief SAC on probe and drogue refueling, and Maxwell, in his big, booming voice and his heavyweight presence, almost as heavyweight as LeMay himself--heavyweight presence, I mean not necessarily size and scale reading, but domineering presence. Jewell went through this whole logical presentation of how well it worked and pretty soon came to the end of the briefing, and General LeMay said, "Well, I don't want anything to do with that kind of a system. Anybody in this room want anything to do with that kind of a system, believe in that?" Silence. [Laughter] And that was the end of that. I mean that's about how we were received on many occasions.

I didn't finish the story. LeMay turned to Maxwell, and he said, "I don't understand where in the hell all this support for a probe and drogue system comes from in the Air Force. Where does it come from?" And Maxwell stepped across the stage, looked him right straight in the eye, and he still had a pointer in his hand, he said, "General, I believe I can answer that question. Every pilot that's ever flown it, is supporting it." [Laughter] But it was after that that LeMay turned around and said, "Anybody else think there's anything in this?" Silence. [Laughter] So that's kind of the way we got along. That sums up my impression of how we got along with SAC. We just didn't influence people directly and maybe we shouldn't have expected to.

In the long term, we had a fantastic impact on them with just warhead weight and size and the turbofan engine, and the next go-around with the ballistic missile. The impact was just complete and total, but it wasn't at the moment. It took a long time. You had to be there with the equipment before it meant very much. Isn't that strange? You'd think the user, and the guy that's got to face all this, all the flack, literally flack and everything, you'd think he'd be very sensitive and concerned about finding the best way to do it, or what he thought would produce the least losses, but it didn't turn out so.

Collins: Within the DCS development, there was this Directorate of R & E and a Directorate of Requirements. What were the nature of your contacts with those offices?

Blasingame: Well, I had some contact with both of them, and certainly our activity was an open book to them. My own experience was that the DCS requirements was an extension of SAC staff in strategic matters. They were very completely and directly aligned in viewpoint with the SAC staff. I would imagine that the directorate of requirements for strategic operations, probably most of them had a lot of experience in SAC, and I think it would be reasonable to expect that. They were sort of the command's representative on your doorstep, their Washington lobby. Very influential.

Collins: Since I didn't see the document, was that the end of this process of deciding what this strategic DPO was? In the DPO, did you end up recommending the low-altitude subsonic bomber?

Blasingame: Yes, that was our recommendation. I'll tell you the truth, I haven't read or seen one in all these years and I don't know precisely how it was phrased. But we never changed our conclusions on our SAC studies.

Collins: Which raises it seems, an interesting question. You've got, over in the directorate of requirements, people who broadly, in framing the requirements, should be fitting into the pattern of the development planning objective, and clearly there are almost two different cultures at work there.

Blasingame: Yes, and I'm sure the newness of the development planning activity on the scene was probably part of this. They weren't seen as terribly important to the man on the street there in the office. I think the Chief of Staff was more attentive than the man on the street was. That's kind of getting into speculation; that's an impression I have.

Collins: How about contact with the Research and Development Command? That was also another kind of potential point of friction. Was there active contact with the people in that organization, in terms of their understanding of the outlook for particular technologies that you were concerned with? This would I guess be primarily the people at Wright Field.

Blasingame: I don't recall being very much involved with Wright Field. We were involved with a group in Baltimore, ARDC [Air Research Development Command] headquarters. I think our effect there was somewhat delayed again. Somebody there had to pick up the development of that engine, so somebody was influenced by it. Most of our contract with ARDC was by General Schriever directly and personally.

Collins: One of the interesting issues that was raised in this study report we've referred to a couple of times, is the issue of costing out the particular technologies or approaches that were to be recommended. Do you recall this as a consideration as you were sorting through the options of which way to go? Was the relative cost of these things and how that would affect Air Force budgets in future years?

Blasingame: We did not have that expertise. That was an important aspect of the RAND work, looking at systems costs, and to the extent that we had those available to us, they came to us from RAND. You know, to quote system costs for the low-altitude airplane we were suggesting, versus the high-altitude. I don't think we ever got sophisticated numbers on that. But in terms of the B-47 upgrade versus the B-52 we did--somewhere we got some pretty extensive system cost studies on those. I presume that those came from RAND.

Collins: I would expect so. We touched on it a little bit earlier, but I just wanted to phrase it in a slightly different way. You had the RAND advisory input there available to you. Apart from the ballistic missile activity, did you have much contact with the Scientific Advisory Board? Were they an active presence in what you were trying to do, say on aircraft problems?

Blasingame: I didn't have a lot of, certainly no prolonged contact. I would occasionally sit in on one of the meetings but I didn't have that direct involvement with them. I would have a relation sometimes with some of the individuals that were on those panels, but not when the panel was meeting as a whole. Any work that I got involved in like that came somewhat later.

I was not quite as far out on the fringes when we got to the ballistic missile program. I was a little more directly involved there. But even then, the Scientific Advisory Boards would have their presentations from us, and they would not have but a few of the operating officers. Schriever would be there, and maybe one or two others, but they were usually not attended by a lot of the Air Force people.

Collins: What about contact with the Research and Development Board? Do you recall this as a presence at all in your planning activities?

Blasingame: No, and I can't explain to you why that might have been.

Collins: Okay. Did you have a position title during this period?

Blasingame: I don't think so. If I did, I certainly don't remember it. I kind of doubt that I did.

Collins: From our discussion, my impression of that organization was clearly fairly small, and as you characterized it, fairly youthful.

Blasingame: You ran after an assignment that was given to you.[Laughter]

Collins: Okay. The organization just seems fairly fluid at this period that you were there.

Blasingame: It was anything but a rigid, determined structure.

Collins: I'd be interested in understanding a little bit about your transition from this activity to the ballistic missile program work.

Blasingame: There had been some early studies of the feasibility of an ICBM with which I was not familiar. These and intelligence reports, I presume, caused Trevor Gardner, Undersecretary of the Air Force, to ask General Schriever to plan for a major (crash)

effort to develop an ICBM. I knew Si Ramo casually, and I knew Dean Wooldridge by reputation and I knew that they were leaving Hughes to set up their own business. I urged Schriever to contact them on his planned west coast trip to explore their interest in helping organize an accelerated development of the ICBM. He called me the next day from the west coast to recall our discussion wherupon he made contact with Ramo. That I guess, marked the actual start of the program run by Schriever. General Schriever had developed some confidence in me, and he took me to the ballistic missile program and put me in charge of the development of guidance equipment, which was my training at MIT. I don't know that we knew just how we were going to organize the setup when we did set up the ballistic missile office.

But before long, it became apparent that the first cut at it at least, you'd probably set up your major staffs along technological discipline lines--a propulsion group, a guidance and control group, and so forth. Our counterparts, TRW, came with that organization in the back of their minds, too. So we initially organized along these technological disciplines, but we only had one missile then. We were just talking about the Atlas and what should be done to redesign it. You see, it had been designed and conceived around this enormous payload, and it was an enormous machine. The first step was to get that scaled down to a reasonable and practical size, and how you would stage it, and so forth. And that's where we finally adopted the Convair philosophy of the stage-and-a-half design, in which all stages were running on the ground and you drop the booster engine cluster but no tankage while the core, high-altitude engines continue to operate rather than worry about an air start, which was completely unknown territory at the time.

So all our initial concentration was on selecting the right setup for that missile. The propulsion people more or less kept to their game, but they began looking very soon about what might be available other than just a plain liquid oxygen and kerosene engine, because we could all see some of the basing problems and security problems of having missiles exposed on the ground, and as soon as we started talking about underground storage, and so forth, you just had to talk about other kinds of fuels.

The guidance field, which was mine, was in a very embryonic stage. It was not clear to me that we should commit this major effort to one of the early inertial guidance system, but I felt for a future generation, we better get started sponsoring inertial guidance because it was clear to me and to many of us, in the long range that was going to be the ideal system. But we went along with and supported the initial electronic system which Convair had more or less created or sponsored. I believe it was called the Azusa system, which was a ground-based radar electronics type of system, and initiated alongside of this, some far-reaching development work without any idea to production.

Then we began to think about an intermediate-range missile. RAND felt very strongly that we should be going for intermediate-

range missiles, strangely enough. The AT&T Bell Labs group, who were just fresh from the enormously successful development of the Nike program, also was pressing us to add an intermediate-range missile.[Laughter]

I think I was talking about the initial organization along technology lines.

Collins: Evolution of an intermediate-range concept.

Blasingame: As we finally became convinced that we should attempt or add to the program an intermediate-range missile, that gave us the opportunity to commit to an inertial guidance system, because at that range we could undoubtedly achieve the accuracies that we needed. So we decided that it would be an inertially guided missile. Along about the same time, or certainly not very far apart, it was decided to get a second back-up competitive intercontinental ballistic range missile, and that was going to be the Titan, which turned out to be a full two-stage device, and then I was moved over. General Schriever had me take on the assignment as director of the Titan program.

Collins: That would include all aspects of the development of Titan, not just the guidance.

Blasingame: We still had a guidance group but the program director was sort of the authority--he used them but he could make his own decisions. He better be pretty careful and not be in conflict with those people. By that I mean not discarding good quality technical information and advice. I think I'm messing that all up the way I'm trying to say it, but for instance, the man in charge of the guidance program did not report to me as the Titan program director. But I could make my own choice as to guidance system. I guess like the president can nominate a Supreme Court justice, but it's at the pleasure of the senate as to whether he gets appointed or not.

Collins: You're talking about the intermingling of a matrix and program organization. So you became the program director for the Titan program on the Air Force side. I can't recall who your counterpart was in Ramo-Wooldridge who had responsibility--

Blasingame: The first one was Will Duke. Oh my goodness, I can't say the man that followed Will Duke and I know him so well. Oh my, that's terrible. I can see the man's face in front of me, but I can't--that's as far as I can get at the moment.

Collins: That wouldn't have been Louis Dunn or Rube Mettler?

Blasingame: No, they were both at the head of the organization. This chap had come, I believe, from Cornell.

Now the Titan was originally configured--this was before there was a program office--was originally bid on the same liquid oxygen and kerosene type engines as the Atlas, and it was also

like the Atlas. The launch pads were to be open and exposed above ground.

I guess the first real change that I made in the program was, I went in to Schriever and I said, "You know we studied so long and hard the necessity of hardening our strategic bases"--that's another thing that were very hep on in our strategic planning. One reason we wanted the B-47 was to have a lot of them scattered all around. I said, "You know, it's hard for me to live with the idea of an aboveground intercontinental missile. It just doesn't fit what I believe in." And he agreed that it didn't, either. I said, "Well, I've got to have plans get started to make some kind of an underground storage of the missile."

And that led us into the original elevator design of storing the Titan I, just the way it was being designed in a silo. It would be on an elevator that would raise it up to fire, just because you couldn't get enough people to agree with firing it from underground. Then of course later, after I left--we were talking about it then, but got committed to storable fuels, which made more sense.

TAPE 2, SIDE 1

Collins: I'm interested in a good deal of the ballistic missile history but I think just for the purposes of our discussion, I'd be most interested in whatever continuing contact you had with RAND during your various positions in the ballistic missile organization. You raised a couple of points that we may want to follow. One is, I think it was part of RAND's orientation to encourage the hardening of missile sites, and they also were very active in trying to cost out what these various missile programs were going to involve. Did you come into contact or discussion with them on any of these kinds of points?

Blasingame: You know, it's hard for me to be specific about that. I don't think there is any question in my mind but what they were very influential on all of us in Schriever's office in the Pentagon, particularly about being very concerned about the hardness and dispersing of our strategic force. I'm sure that they had a very major input. I can't trace it to a particular incident or anything like that, but I think some of their major studies were aimed at that.

That really immediately confronted me when I got into the ballistic missile business and to realize that all our efforts were soft. I was very uneasy with that. One reason for pushing the inertial guidance was it was something that was amenable to storage. If you could store the missile, here was a system that you could store and have ready. The system itself would be just

as hard as the missile was, whereas if you had a radar-base system you'd have to worry about trying to harden that whole thing, too, to very high levels, and it didn't seem possible to do that with the radar guidance, hence the emphasis on inertial from my guidance-hat standpoint.

Again, let me remind you that in those days, inertial was very much in its infancy, and it was a big stretch to try to commit to ICBM performance with an inertial system, and we did have enough agreement, and with the Scientific Advisory Board and with RAND, that we probably could handle the intermediate-range missile with a commitment to inertial guidance, which I had pressed particularly hard.

Collins: In this case, were these decisions about the inertial or noninertial guidance efforts Schriever's call, or was it somebody else who would make the case for that?

Blasingame: Well, let me turn you back again. You know, he had this powerful and very capable technological staff there supplied by STL, so they were of overwhelming influence on something like that and to make a commitment say, of a Thor to inertial system, you had to have them come along with you. The background of the people in their guidance area of course came out of JPL [Jet Propulsion Laboratory] and they were all ground-based radar people, so their background was that and their outlook was that. The idea of an inertial system was kind of quite new to most of them, and they were more comfortable when it was there as kind of a research effort than they were when it was to be made into part of a missile. But they allowed as to how they could handle it in an intermediate-range missile and certainly supported it very well and staffed up with people that worked in the field.

Collins: I'm wondering about a couple of technical problems that may be associated with that. One is reliability, and the other is what do you have to do to get an inertial guidance system ready to fire with the rocket when you know you need to make a launch. If I recall, there is some amount of preparatory time required to get the guidance system ready to operate as you would like it, or at least at that period.

Blasingame: In that period, the scheme was to be solved by having the system run continuously and be ready at any time. You might have units that would go on alert, and they would be up and ready and aligned, and everything ready to go. Because in those days, of course, the alignment was from the outside with a theodolite. The systems were not self-aligning the way they are today. The gyroscopic art just wasn't well enough advanced that you could get azimuth by determining the earth rotation vector that well on board. Even the Minuteman had theodolites in the silo, but the idea was to have them run and run constantly. In those days, all the gyros had to be kept at operating temperature. So it was quite a commitment to have that stuff running all the time.

Collins: So then reliability became an important issue.

Blasingame: Interesting. Reliability of equipment like that, which we thought was going to be so severe in the missile, and we instituted a lot of elaborate environmental testing of stuff that went into the missile. It turns out that it's a much more benign environment than an airplane environment. An airplane environment, particularly say a commercial airline, is so different because your airplane may be parked in Minneapolis tonight and get down to minus forty, and you can't have power on that thing all night. You want to start up in the morning and drive it to the gate and be ready in twenty or thirty minutes to go, and nothing that was built for any of the missiles could respond to that kind of an environment. So that was why it was quite a different set of equipment that was designed for that role, and one of my major efforts was in making a commercial version of an inertial system a practical thing and sponsoring the development of the inertial navigator for the 747.

Collins: So that would have been when you were at General Motors, you mean.

Blasingame: Yes. Then that system has now been applied--it's in most of the Titan IV's, and Titan II's now for that matter. It's a system derived from the commercial, and its all self-aligning and adequately so, even for a missile flight. It turns out that the longer the time you have to get it aligned, the better the alignment is, so you can make it about as accurate as you want it, granted enough time.

Collins: I don't have any more questions. My primary interest was looking back at this period of development planning function.

Blasingame: I'm concerned that I haven't given you what you wanted about the relationship with RAND or the extent of that contact. I had substantial contact with RAND and I found them very helpful, often on very specific questions.

Collins: Maybe one question to spur that a little bit further. Did you travel out to Santa Monica and meet with RAND people in Santa Monica?

Blasingame: Oh yes, with some frequency. I'm sure that six months didn't go by but I was out there, probably every quarter during this stay in the Pentagon.

Collins: And this would be primarily to talk about their research in aircraft technology?

Blasingame: Yes, and they often had reports coming out that would bear directly on what we were doing, and we would often be privileged to see those, probably on a very early release, particularly if I were out here, whereas it might be a long time before you'd get it on the circuit.

Collins: Right. This interview will be transcribed, and perhaps in the interim some additional things will come to mind, and you can always add them to the transcript when I send it along to you.

Blasingame: Another person you mentioned the other day. Ed Barlow was in our office for an extended period. I used to see a great deal of Ed. Very extraordinary person. How is he, incidentally?

Collins: He seems to be doing pretty well. He still bears the aftereffects of his but with polio which I think he must have contracted just before he came to Washington, but he seems to be doing pretty well.

Blasingame: He's retired now, I presume.

Collins: Oh, yes. He would have been there I think primarily concerned with air defense issues. Was that something that you had any involvement with?

Blasingame: Well, yes, directly with the penetration problem. My memory doesn't serve me well, but I'm sure that Schriever probably had in the back of his mind that the planning should be somewhere involved in the air defense field, too. Maybe that's why Ed was brought into Washington. I really don't recall.

Collins: Why don't we call it quits there? Thank you very much.