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Annette Stein Interview (MORS)

Stein, Annette

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Military Operations Research Society (MORS) Oral History Project Interview of Mr. Arthur Stein and Dr. Annette Stein

Eugene P. Visco

Retired from the Office of the Deputy Under Secretary of the Army (Operations Research)

MILITARY OPS RESEARCH HERITAGE ARTICLE

CLARIFYING NOTE

The Preface, below, was written in 1997 when the intent was to publish the then seven histories developed under the Army Oral History Project. Subsequently, a decision was made to delay publication. By the time I retired from the Office of the Deputy Under Secretary of the Army (Operations Research), ODUSA (OR), I found that Walt Hollis, FS, had originally commissioned Dr. Wilbur Payne, a well-known and highly regarded Army operations analyst, on Wilbur's retirement from his last Army position as Director, TRADOC Operations Research Activity (TORA). Unbeknown to any of us in the ODUSA (OR), Wilbur had completed two interviews before passing away in 1990. The subjects were Dr. Hugh Cole and Floyd Hill. The tapes of those interviews were found by Dr. Dan Willard when he was asked to go through Wilbur's papers and files by Mary Farley, Wilbur's widow. I had the tapes transcribed and the transcriptions reviewed by the two subjects who approved their publication. The added interviews brought the total number of histories under the Army project to 10. The interviewees were Hugh Cole, Margaret Emerson, Abe Golub, Frank Grubbs, Floyd Hill, Hugh Miser, George Schecter, Nick Smith, Annette Stein, and Art Stein. As of now, the following who have died are Hugh Cole, Abe Golub, Frank Grubbs, Hugh Miser, George Schecter, Nick Smith, and Art Stein.

After my retirement from federal service, I recommended that MORS pick up the ''baton'' and perform additional oral history interviews concentrating on pioneer MORSians and early MORS Fellows of the Society. MORS established the Oral History Program in 1998 and has since captured more than 50 oral histories of distinguished MORSians and prominent members of the military OR community.

This volume of *MOR* presents a Foreword written by Walt Hollis and two of the above-mentioned oral histories: Art Stein and Annette Stein. Mr. Arthur ''Art'' Stein, FS, was the third President of MORS, serving from 1967 to 1968. Dr. Annette Stein, Art's wife and one of the human ''computers'' during World War II, worked for Dr. John von Neumann. Dr. Bob Sheldon, FS, whom I mentored in the MORS Oral History Program starting in 1998, helped edit these oral histories.

—*Eugene P. Visco*

PREFACE

The oral history project was assigned to me by Mr. Walter W. Hollis, Deputy Under Secretary of the Army (Operations Research) in the winter-spring of 1992. His initial guidance to me was to seek out support from other Army elements, most specifically the Center of Military History in Washington, DC, and the Military History Institute at the Army War College, Carlisle Barracks, Pennsylvania. Together, Walt and I developed a provisional list of candidates for interview. Our criteria were simple: the people we were looking for would have begun their military analytic careers no later than World War II and they should now be ''vertical, ambulatory, and cogent.'' Up to two of the last three, somewhat lighthearted, criteria could be waived. As Walt pointed out in his Foreword, we had already lost many of the stalwart founders of the practice of Army operations research. There are still, fortunately, many who still meet the criteria. So many, in fact, that the oral history project has a rich trove of potential contributors. Because of time and money limitations only seven people were interviewed for this book. We hope that this little book will be but the first and will open the door to many more like it; time is of the essence if further oral histories are to be gathered from the Army founders.

Following Walt's guidance, I met with BG Harold Nelson, then Chief of Military History, COL Thomas Sweeney, Chief, Military History Institute, and Dr. James Williams, then Director of the Oral History Program, Military History Institute, and now Curator, Armor Museum at Fort Knox, Kentucky, to develop a plan and methods for the oral history project. Subsequently, Jim Williams collaborated with me on development of the Army OR Oral History project. Together we developed a short information paper used to introduce the project to interviewees and help get the dialogue moving. We conducted some of the interviews jointly, with Jim taking the lead at the outset, while I learned from his considerable experience.

Six of the seven histories are included in this volume; the review of the seventh by the interviewee was not completed because of complexities in the interview and subsequent family problems. Maybe some day that interview will be available for publication. The six histories come from

Drs. Annette Stein, Hugh Miser, and Nicholas Smith; Ms. Margaret Emerson; and Messrs. George Schecter and Arthur Stein. The last two gentlemen are now, sad to say, deceased, resulting in great gaps in the military analysis community. Hugh Miser and Nick Smith both began their careers during World War II as members of Army Air Forces operations analysis sections. Later, both served with distinction, Hugh with the new US Air Force and Nick with the Johns Hopkins University Operations Research Office, supporting the US Army. Annette and Art Stein began their professional careers during World War II at Aberdeen Proving Ground. Margaret Emerson worked on the Army staff in the Pentagon during the War. George Schecter spent the first half of his distinguished research and analysis practice in the Army's arsenal system. Jim and I jointly interviewed the Steins and George Schecter; I interviewed Margaret Emerson and Nick Smith; and Hugh Miser was interviewed by LTC Lawrence S. Epstein, an Army Reserve officer working under our direction for this project.

The lives presented in this book span a wide range of Army analysis activity and are a good and interesting introduction to the origins of Army operations research.

FOREWORD

From the time I was appointed Deputy Under Secretary of the Army (Operations Research) in 1980, I have been concerned with the heritage of the practice of operations research in the United States Army and in the larger defense community. When Dr. Wilbur P. Payne, a premier Army operations analyst and the founder and first occupant of this office, retired from full-time service as a Senior Executive Service official, I saw an opportunity to contribute to the recording of our heritage. I prevailed upon Wilbur to undertake interviews with distinguished analysts whose careers began during and just following World War II, recognized as the beginning of the modern practice we call operations research. Sad to say, Wilbur became quite ill shortly after our discussions and soon passed away, leaving a major gap in our community. When Mr. Eugene P. Visco, once a colleague of Wilbur's at the Johns Hopkins University Operations Research Office, the Army central operations research organization from 1948 to 1961, came under my direction in 1987, I saw fit to assign the oral history task to him.

We realized that it was already too late to capture the thoughts and personal recollections of many of the early leaders and contributors to Army analysis. Dr. Ellis A. Johnson, founder and only Director of the ORO, was no longer with us. Dr. Dorothy K. Clark, a fine military historian and analyst who made major inroads in the relationship between casualties and success on the battlefield and who worked in the field in Korea, Vietnam and Thailand, had passed away. No longer with us were James W. Johnson, one of the architects of pentagonal division, Dr. George Pettee and Lynn Rumbaugh (the latter two were main stays in helping Ellis Johnson design and develop the ORO), and many other distinguished practitioners. Nevertheless, there are many analysts still with us whose words need to be preserved.

This little book represents the beginning to that task of preservation. Although the Army may not be able to continue the oral history project, it has set in motion a process that I fervently hope will be continued by others. The Military Operations Research Society, for which I am the Army Sponsor, is an admirable choice to pick up the torch. With Gene Visco now retired but still active with the Society, that may come to pass. If so, I am sure the oral history will benefit by expansion to include analysts from the other services. I note that two of the analysts whose history is included in this volume got their start with the Army Air Forces (now the US Air Force) during World War II.

I commend these pages to the younger and even the middle-aged analysts now practicing. There are important lessons to be learned about searching for operational data, understanding the operations being analyzed, and presenting results to decision makers. I recall the advice given by P. M. S. Blackett, one of the principals in the founding of operational research in the United Kingdom during World War II: "I think the essential prerequisite of sound military advice is that the giver must convince himself that if he were responsible for action he would himself act so.'' Good words for analysts to keep in mind.

Read and enjoy!

Walter W. Hollis, Deputy Under Secretary of the Army (OR), The Pentagon
April 1997

OPERATIONS RESEARCH ORAL HISTORY PROJECT INTERVIEW OF MR. ARTHUR STEIN, FS

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INTRODUCTION

Mr. Arthur ''Art'' Stein was the first Secretary Treasurer of MORS in 1966. He then served as Vice President of MORS and later as the third MORS President, from 1967 to 1968. He was elected a MORS Fellow of the Society (FS) in 1991. Mr. Stein earned a bachelor's degree in mathematics from City College of New York (CCNY) in 1938. Art began his early career in operations research at the Army Ballistics Research Laboratory. He was named head of Operations Research at the Cornell Aeronautical Laboratory and remained in this position until his retirement in 1974. After his retirement, Art worked at Falcon Research and Development Center until December 1983. After a second retirement, he became a consultant at the Institute for Defense Analyses (IDA). Mr. Stein passed away on June 24, 1995. The interview was conducted at IDA, Alexandria, Virginia, on April 2, 1992 and at Mr. Stein's home in Williamsville (near Buffalo), New York, on June 5, 1992.

Gene Visco: Would you begin by giving your name, position, and address?

Art Stein: My full name is Arthur Stein—no middle initial. I am presently a consultant here at IDA and have been since 1984. I retired twice. After about 15 years' service, I retired at the beginning of 1974 from the Cornell Aeronautical Laboratory, which had been sold and named CALSPAN Corporation. I then opened a branch of the Falcon Research and Development Company, whose headquarters was in Denver. Falcon was wholly owned by the Whittaker Corporation, a conglomerate. I retired from Falcon in December 1983. I turned it over to one of the other fellows in the group. That company is now an independent company; they bought themselves out. They are now called ANSIM (Analysis and Simulation) Corporation. Until two years ago I maintained a separate office in the Buffalo, New York, area. Now I have my office in my home in Williamsville, New York.

Gene Visco: I'd like to ask you some basic questions about your background as it might pertain to operations research. Did you have any military, family, or educational background that was relevant?

Art Stein: With respect to military experience, the highest rank I held was staff sergeant. My Army specialty was mathematician. However, I had two main activities during the time I was in the Army. One, I served at the Ballistics Research Laboratory (BRL), at Aberdeen Proving Ground, Maryland. I came back to that laboratory as a private, after having been a civilian there for three years. Also, half of my time in service was on a ballistics team. This team, which was a 12-man squad, had the responsibility for calibrating artillery weapons in the field. So, I did that. About two or three months before I was discharged and left service, a new project had started. I'll tell you something about that later. As a result of that new project, I was asked if I would assist. They needed a statistician—somebody to do some test planning. It sounded quite interesting. That's how I got into the aircraft vulnerability business, because the project was associated with obtaining information from war-weary aircraft. That was very shortly after World War II was over. My Army service was from July 14—Bastille Day—1944 through June 1946. My childhood and education: I was born August 5, 1918, in New York City, where I was raised and educated. I received my bachelor's degree from City College of New York (CCNY) in 1938, with a major in mathematics. This may be of interest to people in operations research: At that time the possible fields for people in mathematics were very restricted compared to today. All we knew—and perhaps we were a bit provincial—was that you could teach it or you could become an actuary. There were applied areas, such as astronomy and engineering. However, they would require that I go out of town to some university and that was out of the question in my particular circumstances. It was my intention to become an actuary eventually. It was very difficult and time consuming to go through that actuary sequence, because first you had to be working in an insurance company and then take seven sequential examinations, normally spaced over almost 10 years. There was a depression on then, and openings in insurance companies weren't there. I tried. As a backup, I took some undergraduate education courses, as well, and then did some practice teaching in mathematics at a now-defunct and very unusual high school in New York—Townsend Harris High School. This was a school for bright young boys. Upon successful completion of Townsend Harris, which they could do in three years, they could enter CCNY without having to take any examinations or have any particular grade level. Others going into CCNY had to have a very high average—93 or 94—to get into the college. I entered CCNY based on my grade average. Incidentally I graduated from CCNY at age 20, which wasn't unusual in those days. In fact, I would venture to say that, in that particular college, almost half of the students graduating were probably on the order of 20 years old. After that, I went to Columbia University. When I graduated from CCNY, there were no positions available because of the Depression. I had taken a number of civil service examinations and was waiting to hear from them. One was for meteorologist. I studied and learned something about meteorology. I took some local civil service examinations for clerical positions. Lo and behold, one came along. In September 1939, I went to Columbia University to major in mathematical statistics, which was in the School of Economics. What turned me toward that particular area was a conversation with a second cousin. Families were very close-knit in those days. This second cousin was really the fair-haired boy in the extended family. He was going into medical research and had studied two years of mathematical statistics to equip himself to do that kind of work. When he heard about me, he asked to see me and talked about the opportunities that would exist in mathematical statistics. So that opened up some new possibilities, and I thought that made good sense. I knew I wasn't interested in theoretical mathematics, per se. I was interested already in problem solving, and mathematical statistics seemed to be a way to do that. I went to Columbia, which he had suggested and which was fortunate because it was in New York. I could continue working in the clerical position with the New York City Department of Welfare, which was the largest department in the city in those days. Professor Harold Hotelling, who was one of the best-known academics in mathematical statistics, was at Columbia. (We may come back to him later in connection with some papers for the National Defense Research Council, which he was involved with.) I was fortunate, as well, in that all of the courses offered in mathematical statistics could be taken starting at 5:10 p.m. That is, half the courses would be during the day and half would be during the evening. The next year they inverted the two. So it was possible in two years to get your master's degree. That was what I did. Columbia did that to accommodate people who were working. Many of the people in my classes were working in various insurance companies. It was interesting. In fact, some were actuaries. During that period, the thing that influenced me most toward what we now know as operations research was a course I took just to fill out the required number of credits. I had taken all the courses they had in mathematical statistics. This other course was given in the School of Business. It was called internal statistics; it related to statistics of the firm or company. The first week consisted of the very simple statistics that you might see in business statistics. After that, it turned out that what this man was presenting was solving various problems—scheduling, queuing, and optimization—and much of this was analytical or trial-and-error, because we had no computers and no theory to fall back on. It was trying to think through the process—how you would arrive at the best way to proceed. That I found to be one of the most interesting courses I had taken. The course was given by Dr. R. Parker Eastwood.

Shortly after I had started at Columbia, only about three or four weeks into Hotelling's course, he introduced a little man who came down from the back of the room. Hotelling said he was going to India to teach for a year. The course would be taken over by this little man, Dr. Abraham Wald, who was a Hungarian refugee. Wald eventually became the leading mathematical statistician in this country. Unfortunately he had an untimely accidental death in an aircraft accident in India. I guess he was in his 40s when that happened. He was responsible for the development of sequential analysis. It's interesting that when he first started to lecture, we were so disappointed because we couldn't understand his English. But he did much writing on the blackboard. After the third lecture we didn't care particularly if Hotelling didn't return. Hotelling was a very nice guy but rambled. Wald presented everything in so logical a fashion and made the difficult seem simple that it was really inspiring. He was very, very modest—an unassuming person and the kind of professor people really grew to love. He was my professor for perhaps half of the course work I had at Columbia.

Gene Visco: When and how did you first start doing work that became associated with operations research?

Art Stein: As I was nearing the end of the two-year period at Columbia, I could not get my master's degree because there was a required seminar that was not given in the evenings. It was the one exception to the ease of scheduling all those courses. It was early 1941. Things were beginning to heat up. The war in Europe had already started and things were already starting in this country in terms of supplying the allied forces. Hotelling suggested that the only man he knew who was really doing any work in applied mathematical statistics was Captain Leslie E. Simon. Hotelling thought maybe he was a major by then. Also, applied work was being done by people at the Bell Telephone Company. They were working principally in probability, and Walter Shewhart had just started some of his work in quality control. Anyway, Hotelling thought I should write to Simon at the Aberdeen Proving Ground. So I wrote to Major Simon. It turned out Major Simon was Colonel Simon by then, and he had just become the director of the BRL. He was a very remarkable man. He had written a book, *An Engineer's Manual of Statistical Methods*, which was a very forward-looking, interesting book on sampling and quality control, while he was a lieutenant at Picatinny Arsenal, New Jersey. Incidentally, I have a copy of that at home, along with some other things, such as the comments of a team that went to Europe after the end of the war.

Gene Visco: Was that connected with Alsos? That team was not associated with the Alsos Project, which was strictly nuclear and was led by a Dutch physicist who could not be cleared for the Manhattan Project. He had a team of military police, whose mission was to capture the French and German physicists they could find. That way, if there was a German nuclear weapons program, it would be stopped, so that there couldn't be a surge at the end. Those guys were all incarcerated in a nice facility in England. There's some interesting stuff connected with their reaction to the bomb—a separate part of history.

Art Stein: There was another group, known as the Lehigh Project, that was concerned with assessing bomb damage. It eventually became known as the Strategic Bombing Survey. Well, I went to work at BRL on October 2, 1941. I was assigned to a group called the Mathematics Unit.

Gene Visco: What were some of your early projects or tasks? What do you consider your most interesting or important early work?

Art Stein: The principal work of the Mathematics Unit was the generation of bombing trajectories to make bombing tables. At that time the bomb trajectories, where you had large curvatures, were done by finite-difference methods—quadratures or numerical integration. The study of finite differences had been restricted to actuaries and astronomers. There was no such course when I went to school. The only places they taught that, I believe, were at the University of Michigan and the University of California. The need for me, then, was to study. The only really good book was by a Frenchman, Jordan, which had been translated into English.

Gene Visco: I took that course in 1949 at Boston University, and there was no textbook then.

Art Stein: At any rate, about half of what they did in that unit was the start of these trajectories. That's where they were the most difficult to apply. Once you got through the first 15 or so lines—we'd have a little template, marked quadrille pads, and Marchant or Monroe calculators—and you'd start this trajectory for a particular bomb and particular altitude and speed. These were for standard conditions. At that time, if you had nonstandard conditions, you applied corrections using Galois equations to get the differential effects for winds, errors in altitude, tipping, air density variations, and yawing on release. That also turned out to be half of what I did. Most of the people there worked full time on starting these trajectories.

Gene Visco: What do you consider your most important or interesting work in these early years?

Art Stein: I got into somewhat more interesting work because I was new and did not have the knowledge of all the various bombs and their numbers that others did from having worked on all these trajectories. About two weeks after I arrived, the man who was in charge of this unit gave an examination to the people who'd been coming on board. In the examination he asked very specific questions: For a specific bomb, whose nomenclature was Mark or M so-and-so, dropped from an aircraft travelling at a specified velocity at a specified height, the trail of the bomb would be such-and-such. Or he would ask what the range would be. I didn't know one bomb from any other bomb, so all I did was calculate what the vacuum trajectory would be. Because the answers were multiple choice, I always took a little bit less. I apparently got a good grade on his examination. As a result I was given some very interesting assignments.

Some of these assignments, other than what turned out to be quite boring in starting these various trajectories, were very interesting and might have been called operations research, if that discipline existed. For example, an ordnance officer at the Ordnance School, a lieutenant, had devised a method of firing a rapid-fire 40 millimeter gun wherein the tube would move in a circle. He had a cam set for this so that he could change the diameter of that circle that the muzzle would be moving in. The purpose was to induce dispersion. What caused his becoming interested in this particular device was that, at that time, there was a great fear of an invasion by Germany. The war had started. It was thought that there would be some sort of very fast landing craft, doing a lot of maneuvering on the way in. If you tried to track them with the linear tracking systems we had, you would have very large dispersions. If you had very high precision, you would precisely miss, as it were. So he thought of this as a way to induce dispersion. The question was what is the probability that you'll hit one of these things? And how should you design the diameter as a function of range and some subjective estimate of how much maneuvering was going on? I was handed the problem of determining the probability of hitting with this device. From then on, I got various coverage-type problems related to bombing accuracy and effects, how many you wanted in a stick of bombs, how to straddle targets, and varieties of problems of that type. They were assigned to this Mathematics Unit at BRL. I even got to get my hands on things. We designed, and had the machine shop at BRL make, a copper plate with all kinds of moving arms for doing vector additions to put in the effects of winds aloft that a bomb would have. That would get corrections to determine the true ballistic wind. We made that gadget. They had a problem calibrating the timing device to determine the time of flight of the bomb. They did bomb testing to get the ballistic coefficients of various types of bombs. They would drop a bomb and obtain a signal of when the bomb left. Then they would take pictures of where the splash area would be. These pictures were taken with a Western Electric camera clock that was originally developed for horse races. It had one lens on a rapidly-moving dial and the other saw the scene. The clock started when the bomb was released, and the picture told you when the bomb landed. Meantime you had telemetry that had altitude readings and so on. That clock, which was good to a thousandth of a second, was much more precise than the chronograph that had been used, which was good to a hundredth of a second. But every once in a while it would be off by a second or two. So I was told, "Fix it." I couldn't see, with the gears meshing the way they were in that clock, why such an error would occur. However, just to see whether it would help, I remember putting in an electric bulb to dry out the entire mechanism. We said, a couple hours before we were ready to go, we'll turn on the bulb. We did that and it took care of the problems. But I still don't really understand why. I'm merely saying we were given a variety of tasks at that time; they went from analytical to hands-on work, such as with the ballistic winds calculator and doing something about camera clocks.

Now something else happened in the first week that I was at BRL that would be of great interest, from the point of view of operations research. All that existed in the way of electronic machines were accounting machines. IBM had accounting machines that were over in headquarters. They were used for payroll and such. However, at BRL the man who was in charge of that unit thought that it should be possible to adapt these accounting machines to deal with these finite differences and to do that job. They were merely a series of additions—actually, taking differences, then taking them in another column, and more. So he went and talked with the man who ran the accounting machines. This man was Mr. Gillespie. I'm not sure whether he ever finished high school. He was almost a caricature of the Tennessee mountain man—tall, lanky, chewing tobacco—but he had an insight into the operation of that machine that was really fantastic. They had written to IBM, when BRL wanted to know whether it could be used for this purpose of doing quadratures. IBM said, no—that was impossible with those machines. Mr. Gillespie wired up a machine. He took the board apart, despite instructions not to do this and that. Sure enough, he was doing it. The BRL unit had a lady over there who was doing the roughing of the cards and actually going through the operation. It was now an ongoing operation, using the accounting machines, to do some of the calculations. This lady got sick; and, during my first week there, I was sent to continue the work. Gillespie showed me what to do. About the third day I was there, a delegation came from IBM to see how this thing was happening, despite the fact that their people told them it couldn't. The men from IBM were all obviously VIPs (very important persons), because they all had striped pants and homburgs [Editor's note: a homburg is a formal felt hat]. There was a large number—must have been about a dozen of them. They all stood around while this man from Tennessee showed them how to wire the machine to get these differences. At any rate, to my knowledge, outside of the normal calculating machines like the Marchants and Monroes that were in use, that was the first time that the machines were used. That preceded the high-speed computers at Aberdeen by several years. Before that, all of the calculations had been done manually in a room filled with girls working with large quadrille pads—ruled paper. They still had to, because the machine wasn't that fast. They had these quadrille pads and had three shifts going. The night shift, because there was no air conditioning, had the windows open. Tiny gnats would come in through the screens, fall on the quadrille pads, and look like decimal points. So, if any of the bombs went astray, blame it on the gnats! After I was there about a year, it turned out there was another problem. One group had been doing surveillance testing of ammunition to see how the ammunition would stand up in storage and whether it was still good to use. At the initiative of Colonel Simon, that group teamed with a group from Bell Telephone Laboratory. The Bell people included Harold Dodge and other people who had been working in the area of quality control. Walter Shewhart was the chief of the group. The mission was to generate sampling inspection tables for the very large increase in acquisition, procurement of munitions, and other things. There were no formal procedures for our inspectors, who were now going out to all the plants. Toward the end of 1942, I transferred into that group. For the next two or three years I was involved in developing these tables. As a result, when they had that seminar at Columbia University and I could finally attend, my master's thesis was on most-powerful double sampling inspection plans, because you could take different ratios in the two samples. We wanted to get the best sampling plans to put into those tables. During that same time, Wald had started sequential analysis, which gave the most-powerful multiple sampling plans. Initially, you just took one sample, tested it, and then saw whether your moving mean went above or below these lines to accept or reject the lot of ammunition or whatever you were sampling. I used that before it was really available and published. Because I was talking to them, I got some of their initial notes and used it, since it was the optimum. It was impractical, though, to take one at a time and go back for another. We were working in batches. So we were using group sequential plans. Later Wald actually wrote about group sequential sampling as part of the book on sequential analysis. Incidentally, Walter Shewhart was the father of statistical quality control. He was still at Bell Telephone. They had developed double sampling inspection plans. Because they were within the same company, one section would do a detailed inspection of a rejected lot. Their outgoing qualities were very different from the government inspection, because they were involved in this sort of screening operation. With the government inspection, if it was unacceptable, it would go back to the manufacturer, who had to fix it, change the process or something, and resubmit. So both the average outgoing quality of those procedures and the average amount of inspection were different. The latter type was more typical of the type of procurement the government would be doing. That became the basis for the Ordnance Sampling Inspection Tables. Later, with slight modifications, they became MIL-STD-105, which is still used all over the world for attribute-type sampling. In addition to the work on this type of sampling, because this surveillance group had the statisticians within BRL, it also received other tasks during the war. For example, I became the fellow who had to answer queries from the artillery. The question would come in: Why don't we get the same results you see in the firing tables? There would be some kind of mismatch. They would describe what they were doing and thought they should get a result they weren't getting. I would have to prepare the responses to that and give it to Dr. Theodore B. Sterne, head of the Terminal Ballistics Laboratory, or to Colonel Simon to send out. Usually those were all quick responses. They needed it within a day or two. From the South Pacific the type of question was often something like, how many shots should we expect to have to fire to breach a cave or bunker on the islands, realizing that we have to get succeeding shots into the same hole? No one impact would be sufficient to cause the breach. That would have been a simple problem if each one were independent. Unfortunately, they weren't. It appeared to us to be a much more complicated problem. All during that time at BRL, in the early days of the war, there existed a very unusual, multidisciplinary growth in the attack on problems—very different from what was usual before. Simon had induced the eminent mathematician, Dr. Oswald Veblen, as his "recruiter"—one of two well-known brothers. Veblen, as the recruiter, obtained almost the entire University of California math faculty to come to work at BRL. Edward McShane, who was the head of the math department at Virginia, was a famous man in integration theory. He had [Edwin Powell] Hubble, the astronomer, on the Scientific Advisory Committee. Part time, he had [John] von Neumann, who was there two days a week. [Isador Isaac] Rabi, the physicist; [George] Kistiakowski; Dr. [Bernard] Lewis from the Bureau of Mines. It was quite an assemblage. Many of these people did what I would today call operations research, but it wasn't called that then, at all. They all had problems within their spheres that were types of optimization problems, although they were dealing with very hot, current problems. For example, in about two hours, von Neumann was given and solved the problem of how to correct for spin of a shaped charge: What was the optimum configuration for a liner to compensate for spin? The cross section was the spiral of Archimedes. It would compensate, because a spinning projectile normally doesn't give you the same kind of penetration that a nonspinning projectile would with a shaped charge. The jet is moving around in a spinning projectile, instead of staying in one place. It was fantastic to see these minds at work. During that same time he was very much involved with the concepts for the original computer—the ENIAC. I didn't know it then that he was spending the rest of his time on developing the atomic bomb. Many of these men who came and went were contributing in different ways. At BRL there were problems in optimizing fragmentation of projectiles: What kind of fragments should you have? What sizes? What kinds of errors could you stand on time fuzes? I worked on the variations you would get from a powder-train time fuze: What were they due to, and how could you get rid of them? That was very interesting. There was a lot of that type of work. BRL then was very small. In fact my badge number, when I got there, was 78. Many of the people with lower numbers had left.

So, initially, it was quite small and started in one of the wings of the administration building. It was called the Ballistics Section. All that growth took place in the early years of the war. They got an air-conditioned addition to the building by claiming that the Bush differential analyzer required conditioned air. They were able to get the whole addition that they built air-conditioned. I should correct something I said earlier. I said the only machines being used were the calculators—the Marchants and Monroes. For artillery projectiles, where you had these simple and relatively flat types of trajectories, they could make use of Sciacchi equations. You didn't have to use the numerical methods that were used for the bomb trajectories. They made use of the Bush differential analyzer, which was a nonelectronic machine. It would occupy an area about 40 feet by 70 or 80 feet. It was about waist high, with long rods and gears at various stations. People would stand at these stations. As the calculations were going on, somebody would read off a time of flight; somebody else was getting the range. They would take it off at various times, then they would match up these different readings and do interpolations to get them all for the same times. That was used for artillery, flatter-trajectory firing. The name came from Vannevar Bush, who had one of these at Harvard. This was the second one.

One of the reasons I mention the activities of the academics at BRL was that their general behavior was very focused on the solving of applied problems. Many of these involved optimization of procedures. To my knowledge, there had been no work on the kinds of operations research that dealt with large forces. With [Dr. Oskar] Morgenstern, von Neumann had published a book on the theory of games and economic behavior. We very much wanted to talk to him about things that he had in the book, because the book was not applied to military-type problems. We wanted to see how we could adapt his game theory to military problems. But it was difficult at work to get him to do that. Von Neumann was the type of fellow who never wanted to talk about what he had done. He felt somebody else could come along and sort of mop up. He was always interested in talking about something new. However, we attended the meetings of the Scientific Advisory Committee, which met in the evening. Preceding dinner there would be a cocktail party, and he loved Manhattans. So those of us who were interested in the theory of games would gather around him. One fellow would come back with two Manhattans. He'd take one and give one to von Neumann: "Dr. von Neumann, a toast!" Von Neumann would enjoy a Manhattan. Then another. I must admit we did this with malice aforethought. Soon he would talk about his book. Those were the only times we could really get his attention to those types of problems. Incidentally, this was the BRL Scientific Advisory Committee. It had a number of Nobel Prize winners on it and was very powerful.

Gene Visco: Do you know how and where Colonel Simon got his training?

Art Stein: I don't know where he got his training after West Point. It would be interesting to see. One man who might know is Dr. Frank Grubbs, whom I was going to suggest you contact. He's one of the very few people I know who's still technically active and who was around during that period. He was an officer—a captain, then a major for most of the time I was there. Dr. Grubbs is a mathematical statistician who authored many BRL reports and later the Army Engineering manual on systems analysis. I think he still lives in the vicinity of BRL. Another man now works on the West Coast; his name is Dr. Alexander Charters. He was involved with the spark photography range for getting drag coefficients and behavior of projectiles, and with many of the scaling studies that were done. Dr. von Neumann would consult also for some of that work. My wife actually was one of the *computers*. In those days people were called computers. My wife had been a journalism major and we were married in December after I started to work at Aberdeen. She came down to Aberdeen and after three months or so she was stir crazy and wanted to get a job. She thought she could work on the post paper. She went into the civilian personnel office there. They said, "You're a college graduate? Did you have college math?" She said, "All I had was this one required course." He said, "But it was called College Math?" She said, "Yes." "Well, then, you should go to BRL." So he sent her to BRL. Dr. Louis S. Dederick, the associate director, said, ''You know, you really haven't had much mathematics, have you?'' She said, ''Well, that's what I tried to tell them. But they said I should come here because I had this course called College Math.'' Dr. Dederick looked at her and her paperwork. Then he said, ''Stein? Are you related to Arthur Stein?'' She said, ''Yes, he's my husband.'' ''Oh, that's all right, then. He can help you at night!'' So that's how my wife became a computer. She first worked on the second parts of those bombing tables. There were a very large number of people, all called computers, doing the rest of the bombing tables. They were actually putting them together. Later she was loaned out to assist various other people. When von Neumann came, he used to ask for her to help him. She would tell me what a real gentleman he was.

Gene Visco: Would Chester Clark be the person whose name you couldn't recall before? You had given me some names earlier.

Art Stein: No, but Chester Clark would be another person who would be good to speak with. He was the executive officer. I think he's still alive. Mr. Robert (Bob) Kent was an associate director of BRL and head of the Exterior Ballistics Laboratory. He served as the technical director. Kent worked in many of the exterior ballistics and artillery fragmentation problems, among others. He was a brilliant man. Floyd Hill came there around the end of the war, in early 1946. He could tell you some of the stuff from a different viewpoint. Floyd preceded Dave Hardison, looking at tank problems. Floyd was in the Proving Ground Detachment with me. When he got into it, I'm not sure. He's a very close friend, lives in this area, and is currently a consultant at IDA. Walter Hollis, FS, knows him because Floyd used to be at the Army's OTEA [Operational Test and Evaluation Agency]. At any rate, one of the reasons I've spouted perhaps too long about these men, rather than what they may have done specifically in operations research, is that I think it would be very desirable and useful for these archives to obtain at least two different kinds of things. One, the bibliography of BRL reports. That would trigger the memories of the people with whom you'd speak, including myself, on some of the activities and those activities that might be considered pre-OR. Two, the correspondence files on the different projects. There were many letters and reports that came in from the field. There had to be responses to these. In those days, unlike the Navy, I don't know of any people who were with the troops doing operations research or who were stationed at various commands doing that. At least I don't know of that being done in the Army. The Navy did, and the Army Air Force had something going on in England. We would later receive large three-view drawings of a B-17 (Boeing B-17 Flying Fortress) with holes marked, where they came back with fragment holes. From that we would try indirectly to deduce that those areas where there were no holes were more vulnerable, so those planes didn't come back. So, in addition to seeing what the distribution might be over an aircraft, we tried this type of indirect estimation of vulnerabilities of aircraft, by looking at the damage to the returned planes. I didn't have any opportunity after the war to verify the basic premise. I never thought of myself as doing operations research then or of how broad the term would become. I remember when the journal (*Journal of the Operations Research Society of America* [JORSA]) first came out. The early practitioners seemed mostly to be mathematical statisticians and a few physical scientists. Some of the early papers had to do with discrete distributions and Poissons and binomials, and various types of applications of this sort. The early journal had a much narrower focus than was true later. I would guess it was about 1946 before the phrase, operations research, first came to my attention. It was used earlier. There was a question, are you doing operations analysis or operations research? That was kicked around. When I get into the last part, that deals with the whole question of aircraft vulnerability, optimization of when you should open or close fire, air-to-air duels and the things that I was talking to Shephard [Professor Ronnie Shephard] about having gone on in England. I don't want to get into that because it'll take a little longer. In these other areas, until I got into that aircraft vulnerability bit, I personally was involved primarily with ballistics, and sampling inspection and quality control. By virtue of some of the earlier work, I was also asked to look into malfunctions because they were involved with tests and sampling. Something would malfunction and I would have to go see why it was happening. What kind of a test could we run to find out? I became familiar, particularly with fuzes; they were the most amenable to malfunction or at least to give results that were somewhat unexpected at times. Later we were visited, once a year, by classes from West Point. There would be a series of lectures on different subjects, and I was asked to talk about ammunition malfunctions—what kinds of things could or might happen. That was a kind of test-design work. We set up experiments and tests to determine what the causes would be. I think you would still call that statistics, rather than operations research, but I don't know. It is very important that we obtain the reports of Herbert K. Weiss, who came to BRL toward the end of the war—about 1945 or 1946. I would say he was the principal man doing operations research at BRL. I believe he left BRL about 1953 or 1954 to go to Northrop. The last I knew he lived in Palos Verdes, south of Los Angeles. Prior to coming to BRL he worked at the Army's Air Defense School at Fort Bliss, Texas. He has an antiaircraft gun sight, the Weiss Sight, named after him. I knew him to be brilliant, imaginative, and a prolific writer on many diverse military OR topics. Dr. Paul Deitz, head of the Vulnerability Laboratory at BRL, has promised to send me a listing of Weiss's BRL reports. If he is still alive, he would be an excellent man to interview. Floyd Hill may have his address and phone numbers.

Jim Williams: Art, what is your perspective in this oral history?

Art Stein: What I have been describing is either work that I did or was involved with, or my perceptions of work being done and in which I had some interest and awareness. However, back then there were Black Programs, just as there are today, and I was quite junior in the organization. As a result, there's much I didn't know about. In addition, there were some sections that I just never interacted with, such as Interior Ballistics. We didn't do very much with them during the time I was there. I had much more to do with that area later on. On the other hand, I think that there is very little of what we might term operations research or operations research-related activities going on in a section or branch such as Interior Ballistics. They started out as sections and later as the staff grew at BRL, they were made branches and later in fact were called laboratories all on their own. Well, what I have related so far essentially concerned the work in ballistics, per se. I was originally with a mathematics unit and some of the problems I described were during that first year at BRL when I was with that unit. Then I transferred into the surveillance unit, which was much more statistical in nature and that's where a good deal of the sampling inspection, quality assurance, and ammunition malfunction work was done.

I went into the Army in 1944 from that section and returned as a private. I believe I have described some of that. Initially when I returned it was back to the same desk. I think I mentioned that and I had a number of WACs (Women's Army Corps) and even a couple of lieutenants who worked for me during the day; but before we started work and after we finished, then we reversed roles.

The next thing I did was go to a ballistics team known as the T-6 Chronograph teams. They were developed in 12-man squads to calibrate artillery weapons in the field. We used Doppler radar, but the entire operation wasn't that well mechanized. It was necessary to have a mathematician on the team, because the radar was not located right behind the gun itself or close to it, but was offset. Therefore, you had to make corrections for the amount of offset when you were determining muzzle velocity.

You couldn't take chronograph screens as you would use at a Proving Ground where you fire through an initial screen and then a later screen in order to get velocity and then work back to the muzzle. These were Doppler radars, very mobile, and you could use them to calibrate guns in actual combat.

We had three port calls to go to various theaters and each time they were canceled because needs changed. The war was starting to wind down. At the end there wasn't very much for this team to do. We had finished putting everything back in Cosmoline [*Editor's note: Cosmoline is a rust preventative*] again and actually we were trying to find useful things to do.

It was just at that time—in late 1945—that a letter from the Office of the Chief of Ordnance came to the Development and Proof Services at the Proving Ground. It said there were 20 to

30 obsolete aircraft there at the Proving Ground, and it was desired to fire a very large number of types of small arms and small caliber machine gun types of ammunition at them at different velocities and angles. The types would include ball or shot, high explosive, high explosive incendiary, armored piercing, armored piercing incendiary. The purpose was to determine which caliber would be optimum for firing against the aircraft and to obtain some notion of the effects.

It used to be that people would be mostly concerned with the probability of hit and once the projectile or a round hit whatever it was designed to or supposed to hit, it was assumed it would do the job. This was true earlier with our tank munitions and also with the anti-aircraft munitions. But obviously this wasn't true now. This program was requested by the Chief of Ordnance. Well, there was some concern. This had not been done before. There was some concern on the part of Colonel Carr at the Development and Proof Services as to whether he could actually accomplish all the different tasks and angles and types with so few aircraft there. They wanted to do tests with gasoline in tanks and in a sense this was a forerunner of the current live fire testing that was asked for by Congress for new types of aircraft.

Actually it was Joe Sperrazza and another chief Proof officer working for Colonel Carr at that time in Development and Proof Services who were going to conduct the testing. They asked me if I would be able to help them by examining the statistical meaningfulness of doing different shots under the various conditions. They had to get a reply quickly. I really didn't have more than a day to look at that problem.

At that time, as I may have mentioned, I was a staff sergeant. The problem was one that required some assessment of the likelihood of different types of results and an assumption regarding what might be a permissible error in the answers. I did some calculations and made some estimates of what kinds of variations there might be among a group of projectile types. On that basis it appeared to me that if we wanted to get the answer to the questions raised by the Chief of Ordnance within 10 percent, we would need more than three or four thousand aircraft. Assumptions would have to be made regarding how many shots we might be able to take against an airplane, even with fixing it up, before we would no longer have it to use.

But if you wanted to get answers within 20 percent, you would need something like 1,200 airplanes. That's what I told them. I never expected that anything like that would happen, but they asked me to look at that problem and that's what I did and those are the answers I got.

Well, I guess before Colonel Carr sent his letter back to the Chief of Ordnance with those numbers in it, he called around a couple of places and saw nothing that suggested there was anything wrong with what I had written. So he sent it in.

At this point we switch to the Office of the Chief of Ordnance and the letter is on the Chief's desk. He's first seeing it. I knew some of the people there. Later I got to know them quite well, one woman real well. She was a very, very fine analyst in her own right. Melvin Miller was there and they all told me that when the General saw this letter he just about hit the ceiling *[Laughter]* and he put a call into the Proving Ground to Colonel Carr. He wanted a conference call and he wanted Colonel Simon from BRL, who was a scientist in the Army Officer Corps at that time, and the Director of the BRL to be in on the call.

Colonel Simon came to Colonel Carr's office and there was a group of people in Colonel Carr's office when the call came in. I was sitting there as well. The General asked Colonel Carr, "And who made the analysis? Who came up with this number of 1,200 airplanes?" Colonel Carr said, "Why Sergeant Stein, sir." *[Laughter]*

Then he wanted to speak to Colonel Simon. The General said, "Les, what do you think about that estimate? Do you know who this Sergeant Stein is?" And Colonel Simon—I loved him forever afterward—said, "Sir, if Sergeant Stein says you need 1,200 aircraft, that's how many aircraft you need." *[Laughter]*

Jim Williams: Do you remember the name of the Chief of Ordnance at that time?

Art Stein: No, I probably could dig it up somewhere. Believe it or not, they started shipping in 1,200 airplanes. They all came flying into Aberdeen Proving Ground and that was the start of the largest systematic investigation of aircraft vulnerability. The cost of the testing was provided by the air people, not by ordnance, because they wanted to know how to make aircraft less vulnerable. The ordnance people wanted to know how to make better munitions for shooting against aircraft. So there were those two interests.

I believe the amount of money provided by the Air Force was on the order of two million dollars a year for that program. We had about eight ranges going at the same time and we also had quite a number of supplementary tests.

Let me say that very shortly after this program started we scrapped that original plan of testing all these many different types because we knew that if we didn't get a particular type of damage with a larger and faster projectile, we weren't apt to get it with a slower one. We started to do our test designs incorporating sequential types of subplans, you might say.

In addition, we felt that we could get a great deal of information from the aircraft without interacting with the loaded fuel tank. If we first downloaded the fuel and did some structures testing, we looked at component damage for power plants, fuel, fuel tanks, probability of getting fires, personnel, damage, casualties, and structures.

When I left the Army I came back to BRL, because I had been a civilian there to begin with. And the program I was responsible for was the aircraft vulnerability program.

Gene Visco: Did you mention the date when you made that estimate?

Art Stein: The actual date?

Gene Visco: The approximate year.

Art Stein: 1945.

Now organizationally this program then was transferred away from Development and Proof Services to BRL. The responsibility was put there and I was responsible for that program. It was embedded within the Terminal Ballistics Laboratory headed by Dr. Ted Stern.

Gene Visco: I had forgotten about Ted.

Art Stein: He passed away.

I was put together with a new employee at BRL for whom I worked directly, namely Herb Weiss. That's when I first met him. Our original staff consisted of the two of us, and a computer.

Gene Visco: This was a human computer?

Art Stein: A human computer person who was Francis Hill—Frankie Hill—who was Floyd Hill's wife. Floyd Hill was also an enlisted man in the BRL Detachment. And a Lieutenant Britton.

It wasn't long before the requirements of the staff grew so large so that we had to make significant additions. Now the test planning was done by BRL. The test execution was by Development and Proof Services. So there would be a Proof officer. They would get the gun crews, the ammunition together and the like. The test analysis and test report were done at BRL.

Each time there was a test conducted by Development and Proof Services there would be a firing record written by them that was in consonance with their procedures for all other types of tests.

These firing records constituted the raw data that we had to work with for vulnerability. We were starting in this area with a blank piece of paper. We appreciated that there would be different types of damage, immediate kills some would be interested in, attrition kills to enemy aircraft, types of kills that would interfere with missions but not necessarily be killed, types of damage that might cause extensive requirements for repair before the aircraft could be used again although they weren't actually killed, and the like.

We had 5-minute kills to represent relatively quick damage that might prevent a bomber from releasing on target at the time it was intercepted. These were notions we had for organizing the description of the damage. They were not entirely independently generated by us. We, I think, had a big part but there also had been other work going on for other reasons at the New Mexico School of Mining and Technology located at that time in Albuquerque, New Mexico.

They were operating under a contract for the Navy to examine the effectiveness of antiaircraft shells. I believe they were 5 inch 38s [*Editor's note: this was the Mark 12 5"/38 caliber gun*] with Variable Time (VT) fuzes. To do that they would raise aircraft between two towers suspended by cable, fire the rounds, have their airbursts, and quite precisely map the locations of bursts around the airplane. That was their prime purpose, to investigate the VT fuze functioning.

However, and quite incidentally, when the shell detonated, as a secondary objective they saw where the fragments went. They wanted to know whether you would get hits or not with the various locations of bursts and at the different remaining velocities that these projectiles had at the time they burst.

They actually then recorded where on the airplanes the fragments would hit and they would divide the hits as to whether they were very serious or critical types of hits or not so critical. In other words, they had already started to think about some procedure for this.

We were quite interested in finding out what they were doing and to borrow or use whatever concepts they had that would apply and be useful for us.

Before I continue with what the effect of our visit there was, I should describe how we did our assessments because we wanted our damage assessors to go to the New Mexico Institute and see how they were doing damage assessment there and what could be borrowed.

We had Naval and Air Force officers assigned to the Proving Ground who did the damage assessments for us. They would see the damage, which cables were cut, what happened to an engine and the like, and we would have some assumptions regarding the scenario for the combat aircraft. They would then make their assessment as to what the damage signified and they would write it up. They also traced the damage, and described what it was that was cut. They traced the path and wrote perhaps a half page on the average description of what was observed.

They would then categorize the damages to whether it was an immediate kill, a 5-minute kill, attrition kill, requiring air type. However, I appreciated the fact that damage to that airplane, in that scenario, either was a kill or it was not. It wasn't a probabilistic matter either way.

Nevertheless, if I insisted that they tell me whether it was a kill or not, yes or no, and they had entertained some doubts as to whether it was a kill, I might be forcing a very large error to get into the assessment procedure.

I had a system set up whereby they would use fractional numbers as well, reflecting the degree of confidence they had—how they leaned. They didn't have to say they were certain this was a kill, which would be 100 percent, but they could say 80 percent, which meant that they were fairly sure it was a kill, but it might not be. They weren't certain, or 10 percent, for some particular damage they observed.

The underlying assumption for these fractional kills was that the expectation over many such assessments would be the expected number of kills you would have had if indeed some all-knowing person had put 1s and 0s. That's how that assessment procedure was established throughout all of the firing tests against the aircraft vulnerability program.

Jim Williams: You were talking about how it was that you came to be able to get essentially a pretty high confidence level in the reliability of different types of test results without forcing people into making the categorical distinctions between kills and non-kills.

Art Stein: Yes. Now our principle interest was in the damage itself, true enough. However, we were working with an assessment plan which would require that we put all of our information together and obtain a probability of kill on that airplane — estimate of probability of kill based on the empirical results and assessments.

To that end we had to divide the types of firings we did into two types; one was the random hits where we could do that collection—random hits over the presented area of the components that were being examined. But then we had selected tests where we were attempting to determine the causes of damage and the means that would enable us to generalize from the damage we observed to other aircraft. That became a very important issue later.

We had some problems obtaining random hits on aircraft, for example, shortly after we first started shooting against engines, which might have a presented area of about six to eight square feet. I observed that just before lunch or quitting time the engine would be killed and I felt this was happening more often than one would expect just by random hits over the presented area of the engine.

I went down to observe the procedures that the gunner was using and, as we had gone through previously, he would correct the windage and then he would do a number of shots towards the 1,500 yard range or whatever and quite far in order for us to get the proper slow down.

But before lunch is when he recalibrated, taking extra care, and used his Mann barrel (which is putting the barrel in a V-slot cradle and is used for very accurate shooting as compared with conventional combat). He was able to place the round, for example, right into a P-47 (Republic P-47 Thunderbolt) oil cooler. The inlet of that would be something like a 6-inch diameter circle and he could do that from considerable range. That would cause oil starvation and freezing of the engine and we knew that by then, but it always happened before lunch or before quitting time.

We had to devise something to correct and get around that problem. So we took essentially diagrams of the presented area of the engine at the angles that were being employed, the off angles in the particular series of shots, and we divided that presented area into boxes of areas equivalent to that of this oil cooler, and we labeled each box A, B, C, D, and so on. Then from a table of random numbers we got the order with which we were going to challenge the gunner to shoot at this engine.

Now he might shoot H first, then B, et cetera, and he actually aimed and we weren't depending on accidental dispersion now at these far ranges. But he aimed to get the round into the presented area as on the diagram he had, that part of the engine and the order in which we had picked from a table of random numbers and that was the challenge. We complimented him on his ability to do that throughout the series and we got over this problem of the biasing results before quitting and lunch.

Jim Williams: Did you ever find out what it was that motivated the extra care at that particular point in time, right before lunch and quitting time?

Art Stein: Oh, they didn't want to drag over. You couldn't stop the task and let things hang. Suppose he started a fire? People would have to go down and they'd have to assess it and put it out. I mean there's a lot of work to be done. So that nothing interfered with the orderliness of life, if you went and finished it off and killed it, that was it. You could make things a lot neater.

There are many small things of that kind that arose and we were learning as we went along how to properly conduct the tests.

The assessors, as I mentioned, were Naval and Air Force officers, and I went with them to Albuquerque to speak with the people there. The people we met were extremely capable and even though they were concentrating on a somewhat different problem than the one we were facing, they were very, very helpful and after that we were able to continue to cooperate a great deal. We had established a good relationship with them. It was an experience for the assessors and for me to see an experimental program somewhat like ours that had been going on for a period of perhaps two to three years and where some of the procedures had matured and they had also observed some of the things we had started to see.

For example, we also did tests not just firing these rounds that the Office of the Chief of Ordnance had talked about, but we wanted to get information that was important for air bursts, anti-aircraft shells, and missile warheads.

To do this for our purposes we developed a controlled fragmentation shell. We had nine shells, three different initial velocities by controlling the charge-to-metal ratios with these experimental warheads, and three different fragment sizes. These were obtained by notching the warhead.

The men responsible for the design of these controlled fragmentation shells to give us the initial velocities and fragmentations that we wanted were Mr. Shaw and Noah Tolch. They were both very capable and fine people and we worked quite well together with them.

They were able to obtain very good control. The shell was made in the machine shop at BRL and when we fired them, we fired them statically. They would sit in the cradle and we'd detonate them. They might either be horizontal or vertical depending upon what we had in an arena. We used an arena with several aircraft with engines running. We had components out on fuel tanks on stands. We had collected boxes to show us where the fragments were that were coming off from these warheads. Each warhead detonation had done a tremendous amount of damage.

One of the things that we observed was that we had masses or fragment sizes up to the order of something like three-eighths inch square—I think 700 grains was the largest mass we had. We had about 8,000 feet per second. That would be a very large test of warheads.

In thousands of such fragment impacts, we never obtained a kill that was due to structural damage alone. That was a rather robust finding we felt. So that there's no misunderstanding, that did not include damage in control cables or any portion of the control system, per se.

Jim Williams: You're talking about the basic airframe.

Art Stein: The basic airframe and skin.

There were many other interesting factors. We found that you could put an average of 20 50-caliber projectiles into the R-2800 piston engine, which was on the P-47, on the B-26 (Martin B-26 Marauder) light bomber, and on the Navy's F-4, before you'd stop the input of the engine. These were extremely robust and in fact the chief way you obtained the kill at all was by a shot into the oil cooler. To know what that would signify, we ran off-line tests where we shut off the oil to an engine and found that it would run about 15 minutes before it froze. We found in our firing tests that this kept happening with not that much dispersion around the 15-minute freeze point.

We did the same side tests with hydraulics and we got to understand that an in-line liquid-cooled airplane like the P-51 (North American Aviation P-51 Mustang) was extremely vulnerable compared to the P-47. The P-51's engine was up there in the front where it had two circulatory systems. It had the oil and the hydraulics and a hole in either one would cause a kill, an attrition kill on that airplane. And because the engine is jacketed, you couldn't miss them if you hit that anywhere in the presented area of the engine.

The P-51 may have been more maneuverable and had an advantage that way, but the P-47 certainly was a lot more robust. It was the most robust airplane I've seen. When you look at the presented area and what you could hit with just those two small circles for the oil cooler underneath and the top of the pilot's head if he was coming ahead.

The early versions didn't even have any fuel in the wings. The fuel was behind the engine in an L-shaped tank coming down and then below the pilot. All in all it was a tough airplane, but those were all findings that we would never have expected. As far as hits with high explosive shells were concerned, we did that. We cut off sequentially different amounts of propellers to see when we would get sufficient imbalance for there to be kills.

We detonated small blocks of explosives to take out cylinders and in different sequences on the engine to see what they would signify. When you had a running engine and you put a bullet into it, it seemed to be as though it were alive. You'd hear the roar of the engine and then it seemed to grind out any pebbling or damage that the bullet may have caused. It would have grounded that cylinder that might be ineffective, but it sounded as though the engine spit the projectile back out again when you listened to it.

We measured the ability and the power of the engine by pulling a dynamometer that we had in the airplanes. We did a great many investigations offline to obtain incendiary functioning distributions for probabilities of fire to see what the delay times were for fuel spray to come out from tanks after they were hit. We even did some tests just before I left Aberdeen in the stratosphere chamber just to see whether we could get any ignition at extremely high altitudes and we did, although the kind of fire we obtained was very different from anything I had seen. It was an orange transparent fire, very thin, which made me think that we should have been doing an altitude chamber examination to get a better feel for what could occur at altitude.

Unfortunately, they still haven't done that today after all these years. All in all we were, as I said, learning as we went along. The big problem, of course, was we were going to know a great deal about these obsolete aircraft from World War II. How does that help us? That was the subject of an inquiry. A small board was set up by the Air Force to see whether the $2 million per year they were spending at Aberdeen was worthwhile. That question was just one of the questions raised then. How does this shooting against obsolete airplanes help you for the future?

The members of the committee that came to BRL were Colonel Charles Lindbergh, Jimmy Doolittle, and a Navy officer whose name I can't remember.

Jim Williams: Was that committee given a particular name?

Art Stein: I don't know. Lindbergh was the most outspoken of the three. He asked very sharp, good questions. We got to this principal question of how can you estimate new airplanes that we were shooting at.

This was essentially what we did and how I described it to them. We didn't look at aircraft the way they did. To us, the aircraft was a convenient collection of components. Some of the components of the new aircraft will still exist, although perhaps in different sizes, but physically comparable to the components in these older airplanes. Some of the skin or particular type of aluminum may not be in the same place on the new airplane, but how it affects the functioning of the ignition of the fuze and of the incendiary, we learned from the older ones.

Aside from the assessment of the kill of those older airplanes themselves, the descriptions of the damage of what happened is what was so important—what was going on physically. I looked at the airplane as having perhaps 50 red bricks and 200 brown bricks where the bricks represented different componentry or material and the numbers represent their relative size.

The new airplane doesn't have 50 red bricks, but it has 20 red bricks. It has 600 brown bricks, but also has some purple bricks that the old one didn't have. Now we have to see what fraction of the total presented area is represented by purple bricks and what difference they would make. In other words, the physical things that we don't understand, that we couldn't predict, what difference would they make in the overall assessment? If the difference was not large, we would estimate it as best we could from what we knew physically. But if it could be significant and important and if we were looking to say something about one of our own airplane's vulnerability, I would generally go visit the chief designer. Say on a new jet engine, we didn't have any modern jet engines to shoot at. But we had these World War II airplanes. We had some P-59s (Bell P-59 Airacomet, the first American jet fighter aircraft) with centrifugal flow engines. But we had no actual flow jet engines left to shoot at. I went to the chief designer for these engines. They were very cooperative and we'd have detailed drawings of the engine and I would start in the front and I'd ask, ''Well, what if we had a pin hole here? What would happen?''

He would say ''Oh, nothing.'' I said, ''Well, suppose it were a half inch hole?'' Well, he didn't know. I said, ''Suppose it were a three inch hole.'' ''Oh, it would be terrible.'' I would close in from the two ends to try to find where the zone of mixed response might be on his part. Where was he certain and sure and where was he not?

Then I would proceed backward through the engine, station by station, looking for holes, chips off of particular turbines, holes in the tailpipe, and obtain his best overview of what the significance would be.

Now we knew from tests on the materials how big the holes were that we had made, but we didn't know what the significance of making them would be for the new components. In some cases, we didn't know what the holes or damage would be if the material was quite different. For example, we had metalite type of control surfaces. Metalite was really a type of plastic. We did these tests involving aircraft that were sitting on the ground, but in actual fact they'd be in an air stream if they were flying. One question was, ''How much difference would that make?'' We would cut notches in ring materials and put them in a wind tunnel to see whether we would get any increase in damage. We found that the raised petal material would flutter and then break where it was bent and wouldn't cause any increase in damage.

But we weren't sure that would happen in metalite, and indeed, metalite did rip and it was much worse when you did that kind of testing.

We had to rely on many supplementary tests to obtain this information. It's interesting that today in terms of live-fire testing programs that various models have been set up for predictions. The important thing with respect to the modeling of the damage that we learned was that we could not have a universal model which would exist for all the new aircraft to come and merely require that we modify geometry and deal with the damage estimation that way. The introduction of new materials, new control systems, much larger power plants all meant that there had to be a continuing surveillance and vigilance that the models would be current for the new types of aircraft emerging.

It was for that reason that we wanted to emphasize obtaining an understanding of the phenomenology of damage. For example, given the distribution of length and duration of incendiary flash on hitting skin at different obliquities and different thicknesses; then coupling that with the delay times for fuel sprays to emerge from a fuel tank that was hit either below the liquid level in the tank or in some cases from above and going through. We determined that where we had an intersection of the flash and the emerging spray we would have ignition and fire, but where the distances might be large and the flash located very close to the skin, it might not be available any longer. It would have gone out by the time the fuel spray reached it or the fuel air mixture that is caused by the fuel spray would reach it.

Those were principles that enabled us to go to a different type of aircraft and take what differences existed for it, but apply the principles we had learned regarding ignition and be able to estimate what we would expect to get on this other airplane.

At all times we wanted to obtain the feeling for the physical principles involved that we could use in order to make those predictions. We saw that it would be very dangerous just to rely on the empirical data themselves.

Now a number of things would occur. Sometimes we didn't have all the aircraft on hand when we wanted to do the firing and we had some close calls as to being able to continue our efforts. We were engaged in these R-2800 engine tests, and as I mentioned, they're on a number of different aircraft. We had essentially expended all that we had there at the Proving Ground, but we couldn't complete the tests without the Navy F-4s arriving and they were due to come in, but they hadn't arrived. I remember we were going to have to let go of the gun crews and the teams we had assembled to work together and it was a matter of great concern. The planes would be arriving at Phillips Field. I asked the man at the tower, ''Let me know if any of those F-4s come in today because then we can hold the group together.'' I told him that the Proof officer would set it up at whatever the attitude was to be such as 13 degrees elevation, 20 degrees off an azimuth, for example.

Sure enough, late in the morning we get the call that an F-4 is coming in and I tell him to call the Proof officer and get it down there at that particular range. I forgot about it and went back to my other work.

Well, later in the afternoon there was a call for me from the tower. ''Are you Mr. Stein?'' ''Yes.'' Well, the fellow on the other side sounded just like Donald Duck. I mean he was mad. This was a Navy liaison pilot who had flown that F-4 in and he went over to the administration building and was doing his business and when he came back to get his airplane it was down at the end of this range sitting 20 degrees up, 13 degrees elevation. *[Laughter]* Luckily we hadn't started to shoot at it yet, but he was one mad fellow.

Gene Visco: Art, just a quick question. It seems to me at this time there were two different F-4s. Was this an F-4U (Vought F-4U Corsair) or an F-4F (Grumman F-4F Wildcat)?

Art Stein: F-4F.

Gene Visco: Do you remember if the wings were straight and not gull?

Art Stein: Yes. And it had the same Pratt and Whitney R-2800 engine. Yes, I'm pretty sure. In fact, I better check now—F-6F. [*Editor's note: The Grumman F-6F Hellcat replaced the earlier F-4F Wildcat.*] Well, then we had many occasions where we just barely didn't commit terrible, terrible errors in the conduct of the tests.

Despite all the variations in our testing and our goals, the fact that a good number of aircraft were devoted to obtaining information for external blast tests, we were making good use of the 1,200 airplanes that had come in.

I must say this and you can see what often might happen in describing modern scientific statistical design of experiments. When I had that estimate for 1,200 airplanes, there was a meeting of the Scientific Advisory Committee with many of the people from the Laboratory. Colonel Simon at that point was talking about the aircraft vulnerability program. He was talking about how this was not a hodge-podge program, but was scientifically designed from the very beginning and we predicted that we would need 1,200 airplanes.

Then he turns to me and I'm sitting in the audience and, ''Mr. Stein, how many airplanes did we actually use?'' ''Oh, we've used 1,190.'' And he turns to the audience and says to them, ''And that's what you get with statistical design.'' *[Laughter]*

As I said, earlier, I really love him. He was very good at what I would call scientific sales. The tests we did were completely different from the ones that we based an estimate on and we used all we had and if we had 1,500, then we would have used 1,500.

Jim Williams: You had mentioned earlier that you did some kind of comparison of these tests against actual data from combat.

Art Stein: We had received from the Eighth Air Force pictures or drawings of B-17s with holes, the idea being to see where the holes were and the inference being that if you found certain sections where there were no holes, that meant that those particular places were very, very critical. It was an indirect way of estimating what parts were critical because no airplanes came back with hits on those spots.

That was for the B-17s. We had very poor information except gross information from combat. For example, that over 80 percent of the airplanes that went down were aflame and those were in consonance with the sort of things we were doing.

However, there was a much more meaningful prediction not made during that time, but at a later time during the Korean War. At that time I was at the Cornell Aeronautical Laboratory and I had become interested in the total aspect of survivability or evasive maneuvers, countermeasures, total weapons effectiveness, and the like. We had estimated what the relative losses would be in fights between the F-86 (North American F-86 Sabre) and the MiG-15.

This was before any data was available. We assessed the fact that there were essentially no losses with dog fights, per se. If one of the aircraft wanted to not be hit, he could do so—he could get away. They didn't have the ability to really go after somebody who was trying to get away and be able to bring him down.

That seemed to have been borne out and, in fact, most of the kills would occur owing to surprise from a formation of one type by the others diving in on him, going through them, and then going away.

When I was at Cornell Aeronautical Laboratory we had conducted this fuel analysis that I have just been describing. To make the estimate, we assumed that half the time the F-86 would see the MiG-15 first and half the time the MiG would see the F-86 first. The tactic that was most important insofar as losses were concerned would be to dive through the formation firing and then after passing the formation, to keep going. We didn't have the complexities of the dog fight to gain more estimates.

I was told by a number of officers that that was an appropriate assumption. Then the things that made a difference would be the rate of fire of the 50 caliber weapons of the F-86, the vulnerable area of the MiG-15 to those projectiles, where the opening and the closing range was, and how much they carried or could carry and might use in their pass.

Similarly, the 23-mm weapon on the MiG-15 was a higher caliber than the 50 caliber for sure. But this early version of the 23-mm had a very low rate of fire and a very poor ballistic coefficient and the net effect was that during the two types of attacks by the MiG-15 and the F-86 and vice versa, we estimated an advantage of the F-86 of about 8 to 1. It surprised a lot of people that it was that much. The results were presented during a keynote speech I gave at the first Aircraft Passive Defense Conference at Wright-Patterson. I think it was held around 1950 when I presented the results of that study.

Well, lo and behold, when the war ended, the losses were totaled up of air-to-air combat between the F-86 and MiG-15, the ratio was so much closer to what I had predicted than any of the errors would be. I mean it was something like 8.1 instead of the round 8 that we had. The actual number was extremely close, closer than they said in the errors. It could have been 8.5 or even 9, and I would have been very happy. But 8.1 was so close!

So, insofar as that kind of prediction was concerned it worked out well. You've got to appreciate that these were two airplanes that we knew something about and that we had made these estimates for.

Jim Williams: I was reading some material yesterday flying up here. I ran across a reference that the Germans had installed combat cameras on their fighters and that they had about a thousand sorties worth of combat film footage that, after the war, apparently found its way here to Aberdeen, and Herb Weiss had done some analysis of that. Did you folks use that as well?

Art Stein: Well, it was used to illustrate a different point that I think you would appreciate. The combat films were matched against the debriefings of officers after they had returned from combat. The officer in his debriefing, the pilot, would typically say, ''I opened fire at say 400 meters and pulled away at 150 meters.'' Then they would compare it with the camera film to see what it actually was.

Well, yes indeed, in the first couple of years or so if he said, ''I opened fire at 400 and pulled away at 150,'' maybe it actually was 430 or 320 that he opened and when he pulled away at 150 it might have been 200 or something like that, but it was in the ballpark.

These first ones occurred when there was no tail turret on the B-17. Then a tail turret was put on the B-17. With the addition of the tail turret, the B-17 apparently seemed much closer to the fighter coming in. Now when he was debriefed and he said, ''I opened fire at 400 and pulled away at 150,'' what the camera film showed was that in fact he had opened fire at over 2,000 and pulled away perhaps at 1,200 or 1,500 because somebody was shooting at him. That, coupled with the fact that some of the pilots toward the latter stages of the war were less experienced in Europe, caused a very wide variance between the debriefings and the camera film in contrast to those that were observed earlier during the war.

We've often talked, for various types of weapon delivery, about the comparisons between testing at Proving Grounds and combat data, about what the Pucker Factor might be. In that case you could express the Pucker Factor in terms of the opening and closing ranges. We used to use a rule of thumb of something in the order of 3. That is, the standard deviation for fire would be degraded to three times what it would be in Proving Ground testing.

That was your question that I sort of jumped ahead to. I was waiting to talk about Herb Weiss and his part of the work. I might as well do so now.

Let me interject something just to complete the aspect of the vulnerability studies. The blast work, external blasts particularly, were done by other people at the laboratory. These included Bill (Wilfred) Baker, Jim (James) Sarmousakis, and Joe Sperrazza; and one of the things we did in order to get more modern aircraft was to act like ghouls and try to obtain any crashed airplane which would be more modern than the ones we had, anything where a functioning portion of the airplane could be useful for testing.

That's how we were able to get information on the vulnerability of integral fuel tanks to internal bursts by explosive shells. A B-36 (Convair B-36 Peacemaker) went into the lake there at Dallas/Fort Worth and it was essentially being junked. When it was pulled out, there were very large sections that were intact and good. So we immediately had called to try to see what we could get of that. There was no way we could move what was there elsewhere. So we went there with our explosives and we detonated increasing sizes, from quarter-pound, half-pound, and so on going up inside within the very large integral fuel tanks on the B-36. We got a great deal of information. It was the only information we had for a long time and that only came about because of the fact that unfortunately there had been that accident. That also was a source of information and data for more modern aircraft when they had an accident of that type.

Jim Williams: Did you get pretty good cooperation?

Art Stein: Oh, yes. In fact, everybody there felt so bad that this happened and there's such a loss. The fact that you could retrieve some good from it and get some information made them feel a little bit better. Yes, we got good cooperation.

Now the Air Force was concerned about this money they were giving to our team. I think what really stuck in their craw was the fact that after we were finished with the airplane, we had it taken away by a scrap contractor, located on the field, who was paying the Army about another million dollars a year just to dispose of and to take the scraps.

The people at Eglin then attempted to get a transfer of our group to Eglin to do this work. None of the group wanted to leave, but eventually an additional group was set up at Eglin for vulnerability work. That was quite a while after

I had left Aberdeen, but the first attempt was aborted.

Before leaving the discussion of the aircraft vulnerability efforts at Aberdeen, I should mention that since the initial start of the group that I had described with Herb Weiss and two other folks, the group had grown to where it included, as one of the key people, Mr. Morgan Smith, who had come from the New Mexico School of Mines to work at the BRL. He was essentially in charge of all of the fieldwork that was conducted. Mr. Harry Kostiak's general support served somewhat as Executive Officer within the group itself.

Then I had asked a number of fellows, who were all mathematicians, what their choice might be. Who wanted to become our expert on power plants and who wanted fuel systems and the like because I would then be expecting that any time I had questions related to those, they'd be able to come up with the answers. So we had Roland Bernier in the area of fuel systems and so on throughout all the subsystems of the airplane.

Interest was not only on the matter of the vulnerability of the aircraft being hit, but on the entire question of weapons selection. In the Aircraft Weapons Branch the aspects of duels between aircraft, the questions related to hits on aircraft, the questions related to the optimum time to open fire and close fire, were all dealt with directly by Herb Weiss. He had one or two other people who would come on board to assist him. Even though he was the manager of the entire operation, he nevertheless was very prolific in writing papers of his own in those areas.

In this sense, much of what we know today as being OR has greater similarity to the work that Herb Weiss did than to what we were doing in the aircraft vulnerability part of the group. He left BRL in 1952 or 1953 to go to Norton and subsequently served on the Army Science Board, on the Scientific Committee for BRL.

It's interesting the way in which Weiss operated. He "felt" the correct solutions, initially intuitively, and then he would proceed to demonstrate them. Often his demonstrations might not be mathematically rigorous and there were a number of people around the country who made it their business to make rigorous the things that Herb Weiss had observed and had heuristically demonstrated. This was true in the Operations Evaluation Group (OEG) in the Navy and other places around the country.

During this period, the needs of various organizations multiplied insofar as aircraft weapons effectiveness was concerned and the whole question of games and theory.

The Air Force initiated a contract at the Institute for Air Weapons Research at the University of Chicago and these folks were located in the Museum of Science and Industry in Chicago. That's where I first had the opportunity for meeting Clayton Thomas, FS, Tom Caywood, Walt Strauss, Ed Schiller, and the like. They came to Aberdeen to get orientation with respect to some of the terminology we were using, e.g., what we had found out insofar as the vulnerability of aircraft was concerned and weapons effectiveness. They then were to go back and go on from there pursuing various Air Force-specific problem areas.

I'll assume that at some point in the archives Clayton Thomas will be asked to describe the activities of that group themselves.

With the expansion of the effort after the end of World War II, all efforts then were directed toward what the future might be like. When we got into the Korean War, a number of other types of problems surfaced. One of these had to do with the ballistic quality of the artillery ammunition being used in Korea.

Apparently 40 percent of the 105-mm Howitzer rounds fired were fired for observation rather than for effect. This was extremely high and was due to the fact that there were so many small lots at the gun positions. Each time you changed from one lot number to another you had to reregister and refire because of the differences in ballistics behavior from one lot to another.

I was asked to go to Joliet and head a group of ordnance people to serve with people from Bell Telephone Laboratories who were called in to attack this problem. It was one of the most rewarding technical experiences I had.

Rarely does one have the opportunity to participate in the examination of requirements; into R&D activities to determine differential effects and which variables really drive dispersion; into matters related to procurement, inspection, testing, and evaluation for ballistic tests, production, and production control; and one of the most important aspects—distribution. One of the problems that existed is every time there would be a distribution point where ammunition lots were divided among freight cars and among trucks. At the end there were very few large lots that would arrive at any one position.

The average ammunition lot size in those days was on the order of 9,000 rounds. Yet, one powder lot alone could serve 150,000 complete rounds of 105-mm Howitzer ammunition. Within the space of less than two years it was possible to produce very large lots. The first large lot was JAA-1. I still remember the lot number and giving the go ahead for loading that lot out. In that lot, the dispersion was less than that which existed in the smaller lots previously.

Another activity that existed at Joliet, and in which I found myself being involved more and more frequently, had to do with the investigation of various problems in the munitions procurement, aside from this question of the 105-mm Howitzer.

The Korean War period was one of rapid enlargement of the production facilities, and the transfer of people among plants into positions where they had to learn not only characteristics for the new munitions themselves, but also new methods of manufacturing process control. It was during the time when there was increasing emphasis on statistical process control and its use in the munitions industry.

I recall being asked to go to Kingsbury Ordnance Plant in Indiana due to a problem with the 20-mm ammunition scheduled for use on the B-36 bomber. There was not one lot accepted and the causes were several. On the one hand, lots would be rejected or suspended because they did not meet bullet pull tests. The round would be tested in a tensile machine wherein the cartridge would be pulled one way and the projectile the other to see how much bullet pull is required to break the crimp. The bullet pull had to be controlled within fairly good limits because if it was too hard you would rupture the case and get erratic muzzle velocities and also foreign particles in the chamber of the weapon. If it was not crimped hard enough, you might get too low striking velocities or variable striking velocities. So there had to be good control on bullet pull.

The typical manufacturing process involved one machine wherein the shell and projectiles would come in along two different belts. They were weighed. Propellant would drop down into the cartridge case. The projectile would sink into the neck of the cartridge case, pass a crimp bar, and eventually be the complete round.

When I visited the plant I saw a very conscientious man hovering around this machine like a mother hen. Every few minutes he would pull from several projectiles, go to his tensile machine and see what it took to pull it. If the pull was above the nominal value, even though it was within the specification on this projectile that he had collected, he would try to fine-tune back down to the nominal value in terms of the pressure on the crimp bar.

If it were below or above that nominal value, based on those samples of one, he would maybe make these corrections. In effect, if he had had a practice that was controlled well within the two specification limits with a mean right on the nominal between those two specification limits, and the tail of the distribution reaching out to the lower and upper specification limits, he should have left the process alone. However, due to a random variation, he picked one sample and said that the sample might be one or one and a half sigma below or above the nominal, now he fine-tunes to get that one random reading back to a nominal. In effect, he's shifted the whole distribution now. He's thrown a large part of the tail of the distribution outside its specification limit, where if he'd left it alone everything would have been fine.

The next time he gets a reading, perhaps in the other direction, he shifts the distribution the other way. The effect is that he's taken the original process distribution and broadened it, and as a result far too large fractions are outside the specification limits. The lots become suspended, even though the process was inherently okay.

This overzealous operator should have been taking larger samples and working with the means of those samples if he wanted to try to use them for machine process control. Problems such as these existed in many of the plants in the industry and it became necessary to write instructional manuals that could be disseminated through chief inspectors to their staffs and then produced within the various companies.

Our greatest moment in that whole activity came from reports back from the field in Korea as to the behavior of the 105-mm large lots of ammunition that were being sent there as a result of the ballistic quality control project.

We had never expected that the battery commanders would have the time or inclination to answer some of our questions regarding the ammunition. To our surprise, however, we found they would write reports, some of them 50 to 100 pages long, about their experiences with this ammunition, how much they liked it, how much they felt it saved them in terms of time and effectiveness, and getting on targets.

At no other time did I have the opportunity to see the results of an R&D activity materialize in actual combat, and then be recorded upon by the user the way we did with the 105-mm ammunition.

Gene Visco: Do you want to talk a little bit about the international meeting?

Art Stein: To come back to the aircraft vulnerability area for the moment, one of the things that was possible to get initiated quite early in the process was cooperation among England, Canada, and the United States in sharing of information and in working together and coordinating efforts. These were known as working conferences on aircraft vulnerability.

In the first working conference, for example, small teams of two to four people were set up to look at specific problems that the group as a whole felt should, and could, be examined in the short time that the group was together, by pooling the information that various attendees brought to the meeting.

For example, one group examined the problem of decompression in cockpits due to holes in the cockpit, in the canopies, problems related to pressurized suits, and what effects would be at different altitudes or different hole sizes. Others looked at what the needs might be in fuel and being able to predict the results of fuel fires.

We incorporated the best of what we could find in the various efforts going on in the three countries into the proceedings of the working conferences, which appeared as BRL reports.

Skipping back to the work at the Ordnance Ammunition Command, experience indicated the need to build up a competent staff that work could be delegated to with high trust.

In the 105-mm Howitzer Ballistic Quality Control Project we found as a result of numbers of experiments and trials that the sources of dispersion in range and muzzle velocity are due to the shell itself: the rotating band diameter, the rotating band tighteners, the surface finish, dynamic unbalance, and to some extent, other factors relating to the boat tail [*Editors' note: A boat tail bullet has a slightly tapered base at its rear.*]. Insofar as propellant was concerned, there was a problem related to moisture content, and its control. Different amounts of "dry" powder were found in bags even though the total weight was the same when weighed out.

We found that the moisture content of powders varied sufficiently so that one could obtain up to 11 feet per second variation in muzzle velocity from one day to the next. Powder is not weighed in climate control conditions.

It was necessary to obtain the voluntary cooperation of the companies producing 105-mm shells because there was no time for renegotiations of contracts. There were about 20 manufacturers of the steel shell using various processes and they were all paid, and their products accepted, if they were in conformance with the original agreement.

However, they could do 100 percent, or detailed inspection on the variables that we had recognized as being important. They could reject or rework shells that fell outside of the specification limits, and the net result of all that being a fairly uniform distribution within the specification limits rather than the more ideal normal distribution peaked near the center and tailing out at the specification limits.

To obtain this voluntary compliance and to apprise the industry of how it was doing vis-à-vis other members of the industry, I took it upon myself to publish a newsletter. I sent it around to the various companies and I indicated how many shells were rated A, B, or C in terms of quality from the standpoint of ballistic uniformity for each of the shell characteristics that we had found to be important and that were impacting on dispersion.

Some of the companies responded very quickly and were producing the AA rated shell within less than a month after the program was started. However, this was not true of all and, in particular, one company called me. The president called me to complain that his company's C rating had been disseminated to the rest of the industry through this newsletter.

I told him that we had received a number of requests from people who wanted to know how they were doing vis-à-vis the others and we felt we were providing a service. However, if he didn't want to be included, we wouldn't include him and just make a note that the Such and Such Company didn't want to be included. He said, ''That's blackmail,'' and I said, ''But it's true, isn't it?'' I said, ''But why should we have this argument regarding whether or not one is included in the letter when it would be so easy for you to always have gotten the highest rating?'' I said, ''I understand how this has come about and if I could speak to you I think you would agree that this was possible and also desirable.'' I visited this company CEO and described how much we thought of the capability of his plant manager, who had written a number of articles in *Machine Design*. The articles discussed how to control diameters using lathes versus centerless grinders, and how much he could obtain at a lathe. And it was true, he could get more at a lathe than others seemed to be able to, but they did not and could not produce as well as some of those grinders did.

It seemed that it was about time to move toward the use of the centerless grinder, which they did, and that enabled them to produce the highest score of the ammunition in the field.

We had a number of those instances where for one reason or another it was necessary to persuade the people involved. I found, for example, that foundry workers, who were a breed apart from all other men, had been very difficult to convince or to have any concerns at all about what the shell ratings per se were, or any of the other advice. But when explained to them in terms of the impact poor dispersion had on increasing numbers of casualties, it meant a great deal to them and their complete cooperation was obtained. They were eager to participate and make suggestions as to how to improve the weight and cavity of the projectiles, which had determined the piercing operation of the bullets that they started with.

Similarly, we had problems with the largest shell plant, which was producing almost as much as the rest of the industry together. It was possible to demonstrate that one man on a centerless grinding operation could handle both the rough and finished grind and would have little or no inspection if he was able to incorporate the suggested process-control procedures. There was a great deal of opposition initially from the plant manager who said, ''Either you fellows show me how you could save me money and I produce the same quality, or how I can improve my quality and not cost me any more.'' He said, ''But I don't believe any of this quality control stuff.''

Well, we had selected the centerless grinding operation and were able to demonstrate these savings. Their process had a line going past each of the 12 banks of rough and finished grinders and on this belt would go many, many shells to the ''hospital'' for rework and there was a great deal of scrap produced in the way in which they had been operating. He was skeptical of savings until after using our process-control system design to remove the scrap. It turned out that after a full day's operation there were just three pieces that were slightly oversized. The plant manager was completely won over.

The 12 banks were operated three shifts a day. Each grinder had an operator and there was an inspector for each. By going through an inspection scheme where they would leave the process alone and not measure again for five minutes, it was possible to have the same inspector operate two of the banks with no difficulty whatsoever. The reset limits were based on new, medium, or old wheels in terms of the rate of trending that you get with the wear on the wheel and the process was very easy to adapt.

The addition of the two readings, the sum, which was in effect the average of two, the difference between them was the range, which was an estimate of the dispersion, and all the operator had to do was to put in the numbers, the sum or the difference of the two and if they were higher or lower than the reset limits, then he reset, otherwise let it go for five minutes.

The plant manager liked the savings in personnel so much that it turned out he wanted to use the same system on all the other operations in the plant. This is where he ran into trouble because while the people from the centerless grinding operation could be distributed among some of the others, there came the point where there just were too many people and some people would have to be let go. The union objected to the whole procedure that was producing these results. The plant manager felt he had less trouble staying with the old system than to buck the union on this issue.

At the same time, there was a problem at one of the well-known wheel companies that was making the 105-mm Howitzer shell. There the union had a grievance and complained that the system should be used throughout the plant, not just on artillery shells. Their men were being paid on an incentive basis based on how many good pieces they made in a day. The process controls that were put in for the 105-mm Howitzer resulted in those men making more money than the others in the plant. This resulted in dissatisfaction to the degree that they insisted that the Army help them put in the same kind of controls on everything that they did there in the plant, not just the 105s.

The period was very interesting. Many interesting side problems were examined, such as how to detect whether operators were eager or shied away from finding a defect that would reject a large lot of ammunition or which would shut down a line. There had been indications that some inspectors would shy away from reaching that decision point, and others would leap forward and were only too eager to find the serious impacts on their operation. We found it very interesting regarding the statistical methods that were used to separate these out.

All and all it was a very active period, both in the growth of quality-assurance methods generally and the use of analytical techniques in the control of the internal operations in the firm. This had to do with the examination of different vendors, whether they were responsible for the outgoing quality of the plant, how much was contributed internally within the plant, how to manage a quality-control operation most effectively, and ultimately how to write government specifications so that the government could receive a product that had been produced under process control.

In October 1955, after quite a bit of discussion on the part of various companies, RAND, Cornell Lab, University of Chicago, and others, I decided to join Cornell.

The areas of aircraft vulnerability and systems analysis really broadened after I joined Cornell Lab. I was involved with all aspects of aircraft survivability and the examination of requirements for tactical air as well as tactical ground operations.

However, by this time the entire area of operations research had expanded significantly and was much more well-known insofar as the public record is concerned. So I'll stop here in 1955.

In thinking back about those early days and the various activities in which I have been involved, it would appear that those activities related to the sampling inspections and process control have continued development along very much the lines that we had been implementing within ordnance.

However, when it comes to the matter of operations research with respect to certain optimization problems, questions related to aircraft vulnerability and weapons effectiveness, I believe that we have both gained and lost something with the advent of the computer.

What we have gained, of course, is the ability to tackle complex problems and to do so with far more different types of cases than would have been possible otherwise. Many of these could only be solved by numerical means and the computer has enabled the whole area of finite element analysis to be used for the depiction and explanation of complex interactive events.

It is, however, also true that the tool has sometimes obscured the object that was being fashioned and that we find many modelers and analysts who do not have a great deal of subject matter knowledge. They cannot recognize the reasonableness of some of the outputs that occur, the difficulties or means whereby appropriate inputs would be obtained, and the loss of ability to analytically conduct at least bounding investigations to see how worthwhile it is to go into a large-scale examination.

We have seen people react to a new problem somewhat like Pavlov's dogs and immediately start to model the problem without thinking through, at least for a few hours, as to whether it was possible to determine a much more general appropriate solution analytically. Or as I sometimes have said to people, ''How would you do the job assuming you didn't have a computer available to you? What would you do to give me the answer tomorrow? What would you do to give me the answer next week if you didn't have a computer available to you?'' I have found that just in thinking through those methods, people have found more efficient ways of employing the computer rather than brute force, head-on attempted simulation.

It was very interesting to us to find the degree to which we could impact, by analysis, the operation of extremely serious problems relating to the survival of our personnel and equipment and the effectiveness of our operations.

Little did we dream then that the entire area of operations research and management science would mushroom to the degree that it did from a group that may have numbered no more than 50 or 100 working nationally.

DUSA(OR) PROJECT INTERVIEW OF DR. ANNETTE STEIN

Dr. Jim Williams
US Army Military History Institute

INTRODUCTION

Dr. Annette Stein was the wife of Mr. Arthur Stein, FS, the third President of MORS. Annette Stein was one of the human ''computers'' during World War II and worked for Dr. John von Neumann. Von Neumann, a true polymath, was famous for being a principal member of the Manhattan Project and for the development of game theory and the digital computer, among numerous other accomplishments. Dr. Stein received her PhD from the University of Buffalo and then taught on the faculty at Buffalo State College for 15 years, specializing in diagnosis and remediation for children and psychometrics, the measurement of their abilities and disabilities. Dr. Stein is now a Professor Emeritus at Buffalo State College. This interview was sponsored by the Office of the Deputy Under Secretary of the Army (Operations Research), DUSA(OR). The interview was conducted on June 5, 1992 at Dr. Stein's home in Williamsville (near Buffalo), New York.

Jim Williams: Dr. Stein, I'd like to ask you just to give a little bit about your background, where you were born, raised, and that sort of thing.

Annette Stein: I was born in New York City and grew up there. Most of the time I lived in the Bronx, and I went to school there. I went to Hunter College, which at that time was a college for women and was also in a growth period; so my first semester I went to school at Number 2 Park Avenue, an office building in downtown New York. Then we went to the new Bronx Campus, so I was able to walk to school from home and I was there for a year and a half. Then the newest of the buildings, the one that was erected on 68th Street and Park Avenue, was ready and I attended there for the second two years. I was a member of the first graduating class from that particular building.

I was an English and journalism major and I studiously avoided taking courses that I didn't particularly enjoy. I took the required courses in math. I did not take any education courses. I really didn't take any of the difficult science courses either. I had physiology and anthropology and all kinds of good things like that, a lot of economics and political science, and I thought I had a pretty well-rounded bachelor of arts.

Jim Williams: Did you enjoy anthropology?

Annette Stein: Yes. I had a wonderful professor, Dr. Elizabeth Keuer. She was married to an archeologist who was on the faculty of Columbia University and the two of them would spend their summers in the Southwest on various archeological digs. I was very fortunate I thought, and still do, to have been invited to her home one time for afternoon tea and I was really impressed. Her home was beautiful and it was filled with treasures of Native American art. I had known that she had wonderful silver and turquoise jewelry because she always wore them to school and there were always different pieces—beautiful belts and squash blossom necklaces—just marvelous. I took her courses in 1939 and I graduated from Hunter in 1941.

The art of the American Southwest really was not very well known in a big city like New York at that time. It later became very popular. But when I went to her home for tea I was overwhelmed by the beautiful artifacts that she had, rugs on the walls and on the floors, and pottery. It was just like being in an informal museum. That was the beginning of my interest in things like that. So imagine what a thrill it was when Arthur returned from a trip to New Mexico some years later after we'd been married for about six years, and he had a pair of silver and turquoise earrings for me and several small pots from the Pueblo Indians, which we still have today.

That was the beginning of a love affair with the Indians of the Southwest and we've returned many times and we keep adding to our small collection. They are a continuing source of real pleasure and joy, I think for both of us, but I know for me.

Jim Williams: When did you get married?

Annette Stein: We were married December 28, 1941. Actually we had originally planned to be married over the Thanksgiving holiday. Arthur was already working at Aberdeen Proving Ground, Maryland, and so our plan was to be married at Thanksgiving time. He would be home for a long weekend, home meaning New York, but he was not able to get any housing for us until the end of December. We delayed our wedding and we were married, instead, December 28. We didn't expect that between those two dates that we would have the beginning of our involvement in the war with the bombing of Pearl Harbor.

Jim Williams: What is your recollection about the Pearl Harbor bombing?

Annette Stein: Well, Arthur was home for the weekend and he got tickets for a New York Giants football game at the Polo Grounds. I guess that doesn't exist anymore—no, it's gone. [*Laughter*] But at any rate, we were at the football game and there was an aura of excitement all through the game, as there usually is, but then something different became apparent after a while. For one thing, on the public address system there were calls made for General *Somebody* and Colonel *Somebody*, and the name that I remember hearing was a Colonel Doolittle and these people were asked to report to the office and that seemed a little bit ominous. But we really didn't know what had happened, though we knew something had occurred. We didn't know what it was until the game was over. We left the stadium and by the time we got to the street we heard that Pearl Harbor had been bombed and we could hardly believe that.

I remember so well the next day when I was at work. I was working for a printing company at the time and everything stopped when President Roosevelt made a speech on the radio. We all listened to it and I don't think I'll ever forget that particular day and how people looked and how frightened we were. It was such a scary thing for us. It was an experience that you're not likely to forget. So that was where I was. I was at a football game when the bombing occurred and heard officers being called. There were a lot of officers at that football game. Then the next day at work everything stopped so that we could all listen to the President.

Jim Williams: You said that it came as quite a shock. What do you recall about the build-up though? The war, of course, had been going on in Europe for some time and I would expect in New York City folks were pretty well aware at least of some things that were going on.

Annette Stein: Oh, surely, but somehow the idea that the Japanese would fly over Pearl Harbor and bomb our Navy—I don't think that had occurred to anyone as a possibility, certainly not to me. Of course, I guess I was pretty young at the time. I was just 20 and even though I was very much aware of the war in Europe and very sad and worried about that, somehow I never expected that we'd have such a terrible thing happen to us at Pearl Harbor. It's hard to believe even now, even though I've been there several times and stood right there at the monument over the sunken ship.

Jim Williams: You went to Aberdeen at the end of December then, when you were married and when Art was able to get housing?

Annette Stein: Right.

Jim Williams: And you were there some time before you went to work at the Proving Ground?

Annette Stein: Yes, I guess I was there for about three months. We had ordered our furniture in New York City over that Thanksgiving Day weekend and because we were young and inexperienced, and I guess a little bit stupid about money, Arthur had paid for our furniture before it was delivered. [*Laughter*] I guess there was no real compulsion on the part of the dealer to see to it that it got to us because he already had his money. But what it meant was that we waited for almost three months until all of the furniture was delivered. The day after we arrived in Aberdeen our box spring and mattress were delivered to the house and that's all we got for a month. We had that sitting on the floor in the bedroom on top of the packing cartons that they had come in and that was the extent of our furnishings.

We had a very kind neighbor who lent us a card table and two chairs so that we had a place to eat. Also, when we had come down, my mother had packed some essentials that she was sure we would have to have when we got there and so we had some linens, a down comforter, pillows, and a little radio. We carried all of that with us wrapped in boxes with our suitcases on the Pennsylvania Railroad down to Aberdeen. I guess we must have looked like immigrants getting off the train. [*Laughter*] But I was very grateful that we had those things with us, otherwise I don't know what we would have done those first few weeks.

Jim Williams: Did you live on post?

Annette Stein: No, we had wartime housing just off the post, adjacent to the post in an area called Swan Meadows and these were small duplex houses. That is, there were two units in one building. They were single-story houses without a basement and in the kitchen we had a hand-stoked coal furnace and a gas water heater that was not automatic. You had to light it with a match and remember to turn it off after an hour, otherwise it would start hissing steam. This was really interesting because both Arthur and I grew up in apartments in New York City. A furnace was something in the basement of the apartment house and hot water came out of the water faucets and we never had to think about where it was coming from.

We had another kind of situation in the house. We had a coal bin outside the kitchen and we had to remember at night to stoke the furnace and be sure to dampen it after the new coals were going; otherwise, we'd get up in the morning and we'd have a cold house and that happened because we didn't always remember. [*Laughter*] We also had to remember to light the hot water heater and to turn it off, but we didn't always remember to turn it off. So we frequently had scalding water hissing out of the faucets, but, you know, these were minor inconveniences. It was an adventure. It really was. We loved our little house. We planted flowers and trees and cut the grass. It was just so different from growing up in an apartment house in New York City and, besides, we were newlyweds. Even a hand-stoked coal furnace was not too much of a chore.

Jim Williams: Were those housing units built just for the people that worked at the Proving Ground?

Annette Stein: That's right. We were very lucky to get one of those units because shortly afterward they were all occupied and other housing was built at a greater distance from the Proving Ground. They weren't nearly as nice either, if you can imagine something that was not nearly as nice as a house with the furnace and the water heater in the kitchen. But we had the most elegant accommodations that were available at the time.

Jim Williams: Were the people who lived in the other units people that worked at Ballistics Research Laboratory (BRL) as well?

Annette Stein: No, they didn't work at the Lab. Two of the women worked at the Proving Ground, but not at BRL, and the neighbors on the other side were Army people—a sergeant and his wife and child. It was a very congenial community. We all got along and enjoyed each other.

Jim Williams: When did you actually go to work at BRL?

Annette Stein: After about three months. Everything was delivered. We had curtains up on the windows. I guess I had washed the windows and waxed the floors just as many times as they needed to be done and I felt I could leave my house safely and go find a job.

I told you earlier that I had been an English and journalism major in college. I thought that I might get a position on the Proving Ground in the Public Relations Office. And one fine day I went to the personnel office and told them what I was interested in and I was told that there really weren't any openings in public relations, but did I say I had a college degree? And I said, "Yes." He said, "Well, do you have any college math?" I said, "Yes, the required course." "Well, we'll send you over to Ballistics Research

Laboratory to see Dr. Dederick.'' And off I went. Of course I had heard about Dr. Dederick from Arthur. I felt just a little bit intimidated. I was out of my depth.

Dr. Dederick also asked me what math I had had in college and I told him the one required three-hour course as a freshman, some calculus and trigonometry, and I don't know what else. I got an A, but that was all I had. He said, ''You know, that really isn't very much.'' I said, ''I agree.'' He said, ''Stein, are you related to Arthur Stein?'' ''Yes, I'm his wife.'' He said, ''Well, it really isn't enough math, but maybe Arthur can teach you at night.'' So I got a job and I was hired to be a computer. And I knew what a computer was then. A computer was somebody who worked on a calculating machine and did computations, but today if you tell somebody that you were a computer, that's really pretty funny. There were many female computers, mostly young women who had started within, about six months of the time that I started and some within a few months after I did. Most of them were graduates of very nice women's colleges in the South and it was my first introduction to many different southern accents. I became very adept at distinguishing between North and South Carolina as against Alabama, as against Mississippi, but all of these young women—they were all about my age. I was 20 at the time and they were anywhere from 20 to 22 years old. There was one woman from Boston. I remember her well. We were good friends.

Jim Williams: Had these women that had come from the South been specifically recruited to come up there? Do you know how they came to be there?

Annette Stein: I'm really not sure, but all of these women were math majors and they all had much more extensive backgrounds in math than I did—obviously anybody had more than I did. But they were math majors and I will say that even though I wasn't a math major, there was nothing that I had to do that required me to use anything that was beyond what I could do or what I had learned in my one college course. It was fairly routine work and we had, for example, bombing tables that we filled in. We had formulas. We had data and we worked the calculating machines. It was a question of accuracy and speed, and I guess I was just as accurate and just as speedy as the math majors. [*Laughter*]

But the interesting thing was that when we would finish one of these enormous sheets, we would bring it to a supervisor, Elvin Martin. I was always amazed at the way he would take this sheet and run his finger up and down the columns and out of these thousands of numbers if there was one that was wrong, he'd find it and it would take just a few minutes for him to do this. At that time I had a hard time understanding how he was able to do that. Of course in later years I was involved in checking the work of college students at the college where I was teaching. When I reviewed their test results that they had done from testing children, I could immediately spot numbers that didn't look right. At that point I frequently thought back to the days at BRL when Elvin Martin would go up and down those columns in lickety-split time and find an error, if indeed there was one. You have an expectation for what is to be there and if something is not right, it stands out. But I was impressed when I watched him do it.

I guess I worked on bombing tables for about a year and even for a nonmath major like me, it got to be boring. Not only that, we were so busy during that year, there was so much work to be done and there wasn't enough space or machines. It wasn't a question of getting more people because there was no place to put them. We had shifts: there was night shift and there was swing shift and there was day shift. I can remember coming home from the night shift, walking down Liberty Street from the Doodlebug [*Editor's note: the Doodlebug was a self-propelled railcar*], the railroad that used to take us into the Proving Ground. And as I'm walking down the street to my house, Arthur would be walking up the street to the Doodlebug to go in and we'd say, ''Hi, there.''

Jim Williams: The Doodlebug was —

Annette Stein: The Doodlebug was the little train that ran from town and made a stop at Swan Meadows and went all the way into the Proving Ground. It made stops along the way. We didn't have a car then and you couldn't get a car then. So we were really very fortunate that we lived so close to the Doodlebug. It was the equivalent of about a three-block walk to the railroad to pick it up. But when I was working night shift it was really amusing to be coming home and Arthur would be going off to work and we'd meet on Liberty Street.

Jim Williams: Did you folks have any days off or anything like that, or was this a seven days-a-week job?

Annette Stein: No. It was a five days-a-week job. We did have weekends and I think we were two weeks on a shift and then we'd go to another shift. I was only on night shift two weeks out of six. The swing shift wasn't as bad, but there were two aspects: I guess I was getting tired of the shift and I was also getting bored with the work. I asked if I could be transferred to do something a little bit different. That was when I went to work for Dr. Alex Charters in Exterior Ballistics. I did some calculations there, but I also did some other things. They had pictures of projectiles that we used to make measurements of the shock waves and of the wave lengths along the projectile.

I have to tell you that I didn't truly understand what I was doing or why, but I did it well. [*Laughter*] Again, it was work that was precise and required accuracy and I enjoyed doing it. It was a change from just doing calculations and it was also an opportunity to do two different kinds of tasks rather than the same thing all the time. I found that much more pleasant. This is where I had the rare experience of meeting and working for Dr. John von Neumann, who was on the Scientific Advisory Committee for the Laboratory. Of course, I had heard Arthur talking about the Scientific Advisory Committee and I knew who many of the people were, but I didn't know them personally. Dr. von Neumann was a legendary name and I was a little bit apprehensive about doing work for him, but he was really a very sweet and very gentle man.

After I had done some calculations and some measurements for him, he used to ask for me to do tasks at other times when he came. That was very flattering, very pleasing. He was, as I say, really such a nice man. Then I'd hear Arthur's stories about meetings that he was at with Dr. von Neumann and some of the funny stories that he had to tell. That was a whole other dimension because that was not the Dr. von Neumann that I knew. But I did know that he was truly a very special person and it was nice to know that I was doing something for a very brilliant man, especially since I really didn't have very much in the way of qualifications except that I was a careful and meticulous worker.

Jim Williams: Did you still do computations for Dr. von Neumann?

Annette Stein: Yes. He'd bring things for me to do and he'd tell me what to do and I would do them. So, the human computer. [*Laughter*]

Jim Williams: Did this require you to learn more math?

Annette Stein: Not really. You know, I never could really understand why we computers had to be math majors—I was the notable exception—because we didn't need to know very much to do the job. I will say this, there was no way that I would ever go beyond the rather menial tasks that I was doing, whereas there were other women who were able to advance because they did know what they were doing.

Jim Williams: Was there much opportunity for advancement for women in your section at that time?

Annette Stein: Not much. As a matter of fact, I think that at the time I was there, only one woman had professional rank. All the rest of us were subprofessionals. I didn't deserve to be anything but a subprofessional, but there were people there who were capable, knowledgeable, and well-trained and they still didn't have professional rank. At the time it seemed to me grossly unfair and looking back, it certainly does. After a while there were a couple of women of professional rank. One of them was Dr. Jane Dewey. She was at the Laboratory. I think she came to work there after I left. I worked for two years and then I became pregnant, and in those days you really didn't work up until two days before your baby was born. I left when I was three months pregnant and never returned. I was a happy housewife and mother.

Jim Williams: Art was drafted, or enlisted, and went into the Army for a period of time. Did you stay there at Aberdeen through all that period?

Annette Stein: Yes. He was drafted. He was under 26 and he did try to enlist in the Navy. He had a commission that was promised to him. He met the people he was going to work with and was shown his desk. It was all very exciting, but was not to be because when he was inducted into the Army there were papers at every step of the way that said that he was going back to Aberdeen; and that's what happened. I'm not complaining because we were together and he got back to Aberdeen about two weeks before our daughter was born. So that was a comfort. He had several port calls after that and his possessions were shipped out a couple of times, but somehow or other he never did go and he was at Aberdeen most of the time. We stayed in our little house on Liberty Street and both of our children were born there and we lived there for a total of 10 years—and they were 10 good years. The house was very inadequate, but we were happy there.

Jim Williams: Did you leave the house on Liberty Street because your husband left BRL or did you move out for other reasons?

Annette Stein: Well, we had been looking for a house to buy for some time and we hadn't found just the right thing. Then Arthur was transferred to Joliet, Illinois and we moved to Illinois. We lived in Park Forest for three and a half years while he was at Joliet. Then when he was ready to leave Joliet, a big decision was whether to go back to Aberdeen to BRL or to go to Cornell Aeronautical Laboratory here in Buffalo. It was not an easy decision to make and I know that Arthur vacillated. He had a hard time making up his mind, and I remember that we drove down to Aberdeen and we looked around. Even though we had been very happy for our 10 years there, after having spent three and a half years in Illinois in a different kind of community, we were no longer in what was essentially a company town. We were living in a community with people from many different walks of life and I liked that. I also liked the proximity to Chicago and the things that it had to offer in the way of cultural activities; not that Baltimore didn't have any, but I think it was mainly the idea of living in a community with a varied population rather than people who were mainly Proving Ground people.

We could have lived in Baltimore and Arthur could have commuted, but that just seemed like a lot of traveling. Even his commuting from Park Forest to Joliet was less than it would have been from Baltimore to Aberdeen. We came here to Buffalo and looked things over here and we made the decision to come here. There are many ways of looking at a decision. I've always believed that once you make up your mind you need to be happy with your decision and not look back and say, "Well, what if?" I don't think that's productive. We really had a hard time deciding, but once we decided, the die was cast and we were really very happy here. There were good schools for the children, and there was an opportunity for me to go back to school and I went to the University of Buffalo. I had a Johnny-come-lately career, which was nice too. I did that after the children were in high school. That's when I started to go to school myself so I didn't have to have guilt feelings about neglecting them. That sounds funny today, because today women with small children work and have their careers. But in the 1940s and the 1950s that was not the usual thing. I knew that my obligation was to be a mother and a wife full time and it was when we were in Illinois that I got my first part-time job. I saw an ad in the newspaper for a copy editor for a publishing company, and I got the job editing college-level textbooks in economics and management. It was very good for my self-esteem to have a job that somebody was willing to pay me for after all those years and one that I felt qualified to do. I didn't have to say, "Well, all I've had are three hours of college journalism," because I'd had 60 hours of college journalism, literature, and writing. I felt qualified and that was good.