Computer Oral History Collection, 1969-1973, 1977

Interviewee: John V. Atanasoff (1903-1995)
Interviewer: Bonnie Kaplan
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KAPLAN:

This is Bonnie Kaplan at an interview with John V. Atanasoff at his home in New Market, Maryland. Today is August 16th, 1972. We're going to work on Atanasoff's biography from approximately 1941-42 to date. OK?

ATANASOFF:

Perhaps I should attempt to outline the situation as it existed in 1942. You see I had, with the assistance of Mr. Clifford Barry, we had the computing machine nearly completed but I couldn't keep Clifford Barry's work on the machine because of the press of the draft. And Clifford Barry was determined to go somewhere else and work on something that would make him draft resistant.

KAPLAN:

Defer him?

ATANASOFF:

I mean so he could get a deferment, yes. Perhaps I should also outline the portions of the machine that were unfinished. The principle portion was the development work necessary to make the base two recorder work dependably. It would work and then it would stop working and it may be that this is a characteristic of the type of base two recorder which we devised and developed for this machine and which has not been used in other places.

KAPLAN:

This is the base two punch or the base two reader or both?

ATANASOFF:

The combination. The question is that the combination should work. We don't care whether we improve the punch or improve the read back but we had to improve something so that the combination would work reliably.

KAPLAN:

Were most of the problems with the work in getting the punches to punch properly or in getting the reader to read properly?

ATANASOFF:

You see it's a wealth (?) of matter -- it doesn't matter which you improve. If you make the combination work, that's first class and if you don't make the combination work, why, you're in trouble. I think though, if you allow me to express just an off-hand opinion, the principle difficulty was with the material which was used as the dielectric which was to be punched. We had sufficient voltage available in the machine to punch any reasonably dielectric to, to break down the dielectric so that -- this is all we expected the punch to do which is break down the dielectric. Now, some dielectrics leave a nice, neat charred path which is easily read and some do not. We tried a variety of dielectrics which mean, roughly speaking, paper.

I had been working, as is otherwise recorded, on war research under NDRC and /or OSRD and this had made me deeply aware of the press of the war effort of the United States. And I was being strongly opportuned to go into war-connected research on a full-time basis.

KAPLAN:

Excuse me. When did you start doing the war project and of what nature was this project with your at Iowa State?

ATANASOFF:

Well, the question has been raised about the date at which this research for NNDRC began. It really began, whether officially or not, at a conference between myself and Dr. Warren Weaver which took place I believe during the year 1940. Now, you see, Pearl Harbor didn't really occur until December of '41 -- right? So, this is early but the press of the war in Europe was upon us and the subject matter of the research which we did were various aspects of the anti-aircraft fire control problem, including such things as design of equipment and tests on the psychology of following a target. It was necessary in those days that the target be acquired visually and followed by manual controls and this was called "laying." And the questions of aided laying -- by aided laying, we mean a laying system in which the man not only controlled the ___factor which gave you an increment in the displaced of a telephone but through another factor which changes speeds simultaneously in a useful combination so as to make the effort of following the target as simple as possible. It is fairly obvious that this combination would differ slightly from individual to individual and we made tests to discover how aided laying was best designed.

Also did some fundamental researches in connection with the mathematical subject of

extrapolation which, of course, plays a dominant role in the anti-aircraft fire. But, in the end, that anti-aircraft fire met a dead-end inasmuch as the extrapolation, the fanciest mathematical extrapolation would not ... The finest mathematical extrapolation would not furnish an extrapolation that was particularly valid for the periods of time of flight of missiles. That is, if the airplane had any appreciable elevation and so one was forced to change the method of approach from anti-aircraft fire in which the path of the missile was determined when it left the ground to other flight and would come into coincidence (?) with the target. And, of course, these are called guided missiles and in guided missiles, one has a much easier problem of extrapolation.

KAPLAN:

Did you do any work on other methods of anti-aircraft?

ATANASOFF:

A guided missile type of thing?

KAPLAN:

Such as guided missile ...

ATANASOFF:

Well, I did not do guided missile work at this time at Iowa State College but later for the Naval Ordnance Laboratory I invented one guided missile and perhaps we can say a few words about it when we come to the proper time.

KAPLAN:

Let's backtrack and ...

ATANASOFF:

On losing Clifford Barry, I became discouraged about the computing machine and thought perhaps the computing machine could wait until the end of the war. I didn't realize that the computing machine would be of great interest to the war effort. I recognized that it would be of some interest to the war effort but there wasn't easy bridge by which I could connect it with the war effort. And I decided in the fall of, during the late summer of 1942 to move to the Naval Ordnance Laboratory and pursue full-time war connected research. This was at the Naval Ordnance Laboratory at the Gun Factory in Washington, D.C.

To recapitulate a bit, Clifford Barry was married in June, during June 1942 and left for California with another graduate student of ours by the name of William Stone on July

1st, 1942. We have a letter of July 26 showing that Clifford Barry is at work on his new job in California.

In the middle of September 1942 I went to the Naval Ordnance Laboratory in Washington, D.C. Work at the Naval Ordnance Laboratory was at first connected with pressure mines and later I was placed in charged of a test section covering the entire field of acoustic mines. Acoustic mines, of course, included pressure mines because pressure waves are considered a very, very low frequency acoustic wave. Acoustic mines are mines laid in the bottom of the sea such as the current mines laid at the Hyphong Harbor. And, as a matter of fact, mines which are very similar to the ones we developed at that time have been currently laid at Hyphong. They are actuated by acoustic means or by motions, or vibrations or pressures in the sea as contrasted with other mines which work on magnetic principles due to the magnetic influence of passing ships. They're laid in water -- mineable waters are considered waters that are somewhat less then 200 feet deep. There are other classes of mines which are used in deeper water which are anchored and float upward so that they are nearer the surface of the sea. But, as far as bottom mines go, they're laid in the bottom in waters which are less than 200 feet deep and most acoustic mines were bottom mines.

KAPLAN:

Did your research involve new means of producing or inventing these mines, or exactly what did you do with them?

ATANASOFF:

I had charge of all the standardization and testing of acoustic mines for the Navy. I -- presumably most of the designs were initiated before my arrival at the Naval Ordnance Laboratory but I participated in detail designs or improvements on various mines after that time.

I had a gradually developing group of men which carried this responsibility. Our activities multiplied. We were running various test stations where these mines were actually laid and tested with ships running over them but we also had special test signals which we devised and applied to the mines both in the open seas when the mines were laid at the bottom and in the laboratory when the mines were resting on the bench. The mine didn't have to be present; just the mine mechanism.. This is even true when the mines were tested at sea. Under most conditions, the mine mechanism were laid in a dummy case which was not loaded with explosives.

Up to 1943 I believe I was first visited by a doctor -- J. W. Mauchly -- who came to the laboratory with some clearance from someone upstairs (that meant in the research part of the Naval Ordnance Laboratory). I tried to trace his clearance with the man then in charge of the research section of the Naval Ordnance Laboratory, Dr. Johnson, Dr. Ellis Johnson. And Dr. Johnson could not remember ever taking steps to give Mauchly clearance. He

remembered exactly who Mauchly was because Mauchley's father worked in the department of terrestrial magnetism, I believe with Dr. Johnson before World War II and he had had some experience with John Mauchly but he couldn't remember ever having reason for giving him special clearance.

But he arrived at my desk with a special clearance from someone else and after awhile he wanted to participate in our work and stay close to me and he actually took a position, a job in my division under contractual arrangements of two different kinds in which he spent some time at the laboratory. Most of his work at the laboratory was not done with me personally but was done with Dr. Herman Ellingsen. And Ellingsen and I have discussed the matter at intervals before he recently died and Dr. Mauchly's attention to this business was spasmodic and brief. And, as a consequence, Dr. Ellingsen didn't remember a particular contribution of Dr. Mauchly in this operation.

KAPLAN:

First of all, when you said Mauchly first visited you, do you mean that he first visited you while you were at NOL because he did visit you previously?

ATANASOFF:

Right.

KAPLAN:

OK. Do you have any idea of exactly why he wanted to work on your project at NOL, what his duties were with Ellingsen and whether or not the computer project at NOL was going on at the time?

ATANASOFF:

No, the computer project at NOL was not going on at this time. The reasons why John Mauchly selected me at the Naval Ordnance Laboratory, I have no very good way to tell. If he told me, it was obvious that this was just an explanation, a verbal explanation, of his presence. I do not know whether he felt the need to stay close to me for computer machines reasons. As it later developed, he was himself working on computing machines at the time and spending the remainder of his time in the computing machine field.

KAPLAN:

Was he working on his computing machine at the Moore School at the same time he was also working at ...

ATANASOFF:

I believe so. I am not quite sure. It seemed -- I wondered at the time if he were, he didn't seem quite secure in a computing machine project. It seemed as if he was looking around and he felt it necessary to maintain a contractual relation with me under which he would come down and spend one, or two or zero days every week with me. And he would come down and he would go in and talk to Dr. Ellingsen and statisticians. He was partly a statistician himself and he had ideas on statistics and he carried on discussions with these people.

KAPLAN:

Was there a discussion of computing machinery or ideas?

ATANASOFF:

There were very limited discussions of computing machines. Occasionally computing machines would come up and he told me that he was interested in computing machines during this period but I had not comprehension of the scope of the work that was developing at the Moore School. He didn't give me any idea at this time.

KAPLAN:

Would you be a little bit more specific about the date when he came and how these dates correspond to his ...

ATANASOFF:

These dates are hard to fix at this time because we know that this happened sometime during 1943. I think he first arrived at my -- you see I didn't get to the Naval Ordnance Laboratory until September of 1942 and I remember there was a period when I didn't see Mauchly at all. And, then I hadn't seen him. As a matter of fact since the day he visited me at Iowa State College in June of 1941, I believe, I had not seen him until he arrived one day at my desk at the Naval Ordnance Laboratory and that was during 1943 but at the moment I do not have at hand a reference which permits me to date this more specifically. And during 1943 and "44, he visited me at NOL and, as a matter of fact, worked in my employ.

KAPLAN:

He also brought Eckert with him on some of these visits?

ATANASOFF:

On just one of the visits he brought Eckert with him. And this was a visit which was on August 30th, 1944, Mauchly and Eckert visited me at -- and actually we had a conference in building 210 at the Gun Factory in Washington. At this time the mercury delay line

was discussed.

KAPLAN:

When did Mauchly leave your influence, or his contract work with you at NOL?

ATANASOFF:

Well, he maintained some kind of a relationship until 1945 and I guess he visited us in late '45. Perhaps I'd better go back to the rest of the history and then return to this subject because if I don't history won't be chronological.

On the whole our work in acoustic standardization was quite successful and the levels at which we determined that the acoustic mines were to be used in the war were closely followed in the actual application of these mines. And I can say, historically speaking, that these mines were a very effective device against Japan and, of course, they're only exceeded in their success by the Atomic Bomb which we used against Japan.

It is believed by people who have followed the mining of Japan closely that the airplanes used for laying mines around the coast of Japan were many, many fold more effective that the planes used for bombing Japanese cities with the exception of the planes which dropped the Atomic Bomb.

KAPLAN:

You played a large part in the development and testing of those mines...

ATANASOFF:

Well, yes, I played a substantial part, I believe it's fair to say. The war drew to a close and the Naval Ordnance Laboratory, which was very crowded in the Gun Factory, commenced to think about having new quarters in a more spacious area. Finally an area was selected near White Oak, Maryland and land was procured and work began in the development of the present headquarters of NOL. There was a great deal of money in the military department at that time and it was easy to procure money for, and the necessary attention so the development of a new laboratory area could be carried to fruition.

KAPLAN:

When was the move to White Oak?

ATANASOFF:

I actually moved to White Oak in late 1945. And, by this time my position at NOL had been changed and I had moved into a position where I directed all of the acoustic work at

the Naval Ordnance Laboratory.

KAPLAN:

When did you become ... was that also 1945?

ATANASOFF:

It was during, I believe this was during 1945. I don't have a date here for that actual change either but it was during 1945 and that was at the end of the war.

KAPLAN:

What was your position before that; was it sub-director?

ATANASOFF:

No, I was in charge of the acoustic division of the technical department of NOL.

KAPLAN:

And when you became director, what were you ...

ATANASOFF:

Well, then I was in charge of all research in acoustics including pressure devices. For the research department of NOL, I left the technical aspects. The technical aspects had had good men during the war but after the end of the war, they draw their best staff members away from the technical aspects of, the pressures of building immediate materical was not so high and they needed more emphasis on the development of ideas and they commenced moving their best staff into the research department for this purpose.

KAPLAN:

OK. And what sort of duties did you do then ... what sort of duties did you have there? Was it mostly executive work as director?

ATANASOFF:

Well, I had a staff perhaps between 50 and 100 men, people somewhere who had been selected from this ... technical divisions of NOL, technical department of NOL and the research department of NOL and these were the best men that were available in the field of acoustics and we commenced doing fundamental work in acoustics leading towards development of mining and/or other devices necessary for the prosecution of the Navy's business.

KAPLAN:

Did you actually do any research or did you merely direct?

ATANASOFF:

Well, of course, I have always been an idea man and, certainly, the ideas which were worked on by my division were partly originated with me. I had rather able men. Several of whom I have kept in close touch with until the present day. In the above I have intimated that the mine program of the Naval Ordnance Laboratory was a considerable military success. This was not only true of the American mines but those of other nations and our enemy as well.

For the sake of the record, I will recount one event. Early in the war someone conceived of the possibility of a mine which works not on the free field acoustic field but upon a pressure field which vanishes with distance at a different rate. When a bullet moves through the water, there exists a change in pressure as a boat passes at a given point. This pressure can be interpreted in terms of the Bernue (?) effect on the water currents which surround the boat.

Early in the war someone conceived the idea of operating a mine on this principle. A little investigation showed that this mine would be tremendously effective. People in high places were so impressed with these results that a conference took place between President Roosevelt and Prime Minister Churchill. The import of this conference was as follows: if the United States used this kind of a mine, it would quickly be developed by the Germans, who are particularly apt in such matters.

As a consequence of this, Britain being in an extremely vulnerable position as far as the necessities of shipping would be completely destroyed by pressure mines dropped in its mineable waters which give access to its ports... agreement between Churchill and Roosevelt that the United States, although it would develop the mine and have it on a shelf, would never use it.... late in the war the people having to do with the German mine fields detected a mine which was behaving in a very different, erratic, and unexpected and effective way. In 10 days one of these mines rested on the table in the Naval Ordnance Laboratory and it proved to be the pressure mine.... in our job to put our acoustic mines to work but we had an even more difficult problem. That of finding a way to overcome the German mine.

As far as I was concerned the mine had been developed, invented and developed before I went to NOL. I should say more accurately, the invention had taken place, the theoretical exploration had taken place and most of the development work was over although it continued during my stay.

Now we were faced with a new problem -- the invention of a countermeasure. Most

everybody participated, more or less, in this because it was the most important thing that the country faced in a give and take of the mining program. There's no question but what the countermeasure for the pressure mine required a very large and unruly structure and making it almost an impossibility in an impractical way. Various people have tried a variety of simple devices for the use of a countermeasure and they all failed miserably.

I, too, was caught up in this fervor and I invented a sweep or countermeasure for the pressure mine. I have my tongue in my cheek about the whole process. I don't know whether to congratulate myself or not. Perhaps I should congratulate myself as far as anyone should ever be congratulated for his private efforts. The sweep which I invented was large and difficult to use but it was effective and cheap. And, it was the only sweep in the field and I will not give the details here because I am not sure of the exact security classification of this device.

Invention and development tool place in 19_ -- the war ended late in '45, didn't it? The European war ended late in '45. I think the invention took place either late in '44 or early '45. The Navy felt so favorably inclined towards this device that they gave me the Distinguished Civilian Service Award, the highest award given by the Navy to civilians and my citation was signed by the then Secretary of the Navy, Forrestal.... citation from the Navy describes a number of my other works. I believe that the invention of the countermeasure for the pressure mine represented a major and important item thereof. Time in mid-1945 there were a few building constructed at the new laboratory space at White Oak and the operational officer of the Naval Ordnance Laboratory -- you must remember that there was a military command here -- commenced putting pressure on various people to move out to the new space. What they asked us to do was to move a proportionate number of our people out to the new space. I resisted this pressure as long as possible and then I said, "If you will give me enough space, I will move my entire division to White Oak.' The executive officer had had so much trouble getting people to move to White Oak and he badly need to vacate some space at the Gun Factory so he said he would give me the required space and at that time I moved my entire staff to White Oak. I believe this was the first division that was entirely moved to the new location.

The space which I was given at White Oak, however, was not the space which finally housed the acoustic division when the main building was completed but was space that was prepared for a variety of purposes on the area on which White Oak now occupies.

This is the parentheses we're placing in here. I was just telling Bonnie that, of course, I left academic work in 1942. Before that time, I had eight Ph.D. students who received a degree and some, and many more Master's degree students but this was in the early part of my career. The next 20 years I spent first as an employee of the Naval Ordnance Laboratory then in the employment of the Army Field Forces and then back to the Naval Ordnance Laboratory, and then in a business which I created myself which its principal activity was doing various jobs for the Armed Services of the Unites States. And this continued until 1962 so the last 20 years of my life were spent in military work where one does a few things and doesn't publish much of anything. His only output is military

sales.

During these years at the university and since, I have been interested in new devices and inventions of various types. I kept no record of inventions or the patents which derived there from but in the end I was engaged in these lawsuits about computing machines. And lawyers investigated the patents which had been filed in my name at the Patent Office of the United States and found 32 or 33 entries thereon. Many of these have military application, of course. But, there was one, I believe, patient filed on an ice cream stick, an ice cream stick made of paper. And, another one, another patent filed on a machine for cutting nylon cloth.

KAPLAN:

Did any of your patents at all have anything to do about computing? Wasn't there some sort of effort to patent you IBM tabulator addition?

ATANASOFF:

It is interesting to note that not a one of these patents have a single thing to do with computing machines. There was an awful lot of effort put into drawing a patent on computing machines and specifications and even claims were started. But, due to some idiosyncrasy of the organization at Iowa State University, these were never filed. I can shed little light on this subject as to why this occurred. One difficulty was that the patent attorney said you have not been sufficiently explicit in the details which you have given me.

KAPLAN:

(inaudible comment)

ATANASOFF:

This refers to my patent, to my proposals to patent the computing machine which I had invented. On the other hand, patent lawyers of today tell me that the specifications which we drew and which are still in existence were sufficiently explicit to blanket the field. This close this parenthetical material... up to the acoustics division of the research department of the Naval Ordnance Laboratory was well settled in White Oak. But, just after the Navy proposed that there be a computing machine project instituted at NOL. This is in perhaps November of 1945.

There was some discussion about who should head this project. There were a few people who it was deemed would have the ability to carry on a satisfactory operation but the choice soon evolved on to J. V. Atanasoff.

KAPLAN:

Did the Navy know of your previous computer work?

ATANASOFF:

The Navy, of course, did not.

KAPLAN:

Did the Navy know of your previous computer work?

ATANASOFF:

No. The Navy in an official capacity did not but certain people at the Naval Ordnance Laboratory, including my superiors, knew of my previous activities in computing machines. I was clearly the only person there with any previous experience in the field but it is also true that my specialty was very close to what was required to do computer machine work.

As a consequence of this beginning, the Naval Ordnance Laboratory worked on computer machines for just about a year. This project was instituted as an additional duty for the acoustics division of the Naval Ordnance Laboratory. As a consequence I assigned some of my regular acoustics people who were interested to this project and collected a few others from various sources and hired a few directly including Dr. A. E. Brant (?), who had been a good emotional factor in my previous development in computer work at Iowa State University.

People working on this project including A.E. Brant, Calvin Moore, Bob Elbrin (?), David Beecher (?), Mr. Ernest Cohurd (?), Mr. David Brop (?), and a number of others including a man who is now known in other fields by the name of Singer. The project -- there was means for a large project. There are all means for a large project except space, equipment and personnel. That means there was plenty of money.

During this period we had various discussions with various people. Perhaps one or two discussions with John Mauchly but very active discussion with John Von Neuman. Mauchly was not around very much during this period. The activities of NOL during this period comprised ... of the various approaches to computer techniques in particular the memory field. The actual experimental activities of the Naval Ordnance Laboratory group were confined largely to memory employing a cathode ray tube. We had no facilities for producing new tubes and we had to employ various tubes which were made up for other purposes in a memory capacity.

The cathode ray memory has not _ well in over-all computer machine groups, in modern computer machines for a variety reasons. At the time, it looked like a feasible device. It probably still is except that there are others that are more feasible. Of course, I could not

employ my previous item called a condenser because one had to have mechanical switching associated therewith. However, in a way a cathode ray tube is a little condenser and switching is provided by the cathode ray so in a way I was continuing my earlier work on condensers.

I knew this exactly at that time and this was in no wise a later edition to theory. We enjoyed very much our discussions with John Von Neumann but, of course, the number of episodes of this were limited to perhaps five.

KAPLAN:

About what did you speak when you spoke to Von Neumann?

ATANASOFF:

When you speak to Von Neumann, you speak about theories of computer machines mainly. And I would have to spend some time regenerating the exact nature of the discussions. I suppose I could do it if I wanted to spend the time but this is pretty much an impossible situation at this date. It should be emphasized again that our facilities for the computing machine were very limited. We had on our hands in acoustics division at the Naval Ordnance Laboratory a primary responsibility for instrumentation at Projects Crossroads, which was being done in the Pacific.

KAPLAN:

Can I just backtrack slightly and ask you for what purpose was this machine being built? Was it a special purpose machine also did the Navy have some special function it wanted it to serve? Or were you working on a generalized computer?

ATANASOFF:

It was hoped that this would be a general computer and, of course, we hoped to make a start at building in programs of the kind and visage, let's say by all of us but more specifically by John Von Neumann.

KAPLAN:

What sort of programs were used?

ATANASOFF:

Well, I can’t tell you. We planned to build programs into the machine. We just didn't get that far.

KAPLAN:

Oh, you mean the machine itself to store programs within it?

ATANASOFF:

Yes. Let me -- what's that? Yes, well, we were reaching for it at that time. That's what I'm trying to tell you here. We didn't do it. You know that. We were just reaching for it. The Crossroads Project began in the Pacific about the first of July, 1946 so the whole Crossroads Project dug right into the center of the computing machine project and we had to supply a major number of staff members for this project in the Pacific. I believe that perhaps eight of our personnel, including me -- I traveled to the Pacific and participated in the first test, Atomic test after the war explosions of Japan.

KAPLAN:

Was that the nature of the whole Crossroads Project? Was atomic testing after the explosions in Japan?

ATANASOFF:

That's right, yes. This is the first testing project.

KAPLAN:

What were you testing for?

ATANASOFF:

We were attempting to find out what the dynamic effects of the atomic explosion were.

KAPLAN:

You mean the explosion that had already occurred in Japan? You were testing the effects of the Japan explosion?

ATANASOFF:

No, we were testing the effects of the then current Atomic bombs which were current among military people. The mines, I'm sorry, the Atomic bombs, which the military possessed, they tested in Crossroads for the first time after Japan. Between the time of Japan and the time of Crossroads, considerable development had been done in atomic explosions and Crossroads was the first substantial testing effort put in to measure these destructions. I spent perhaps six to eight weeks in the Pacific and during this period, I witnessed two explosions. We were asked to turn out faces, to avert our faces at the time of explosion. We were on the deck of the ship perhaps ten miles away.

KAPLAN:

To avoid damage to your eyes?

ATANASOFF:

To avoid damage to our eyes but I planned to turn rapidly after the explosion without exactly following the restrictions and I turned in time to find two battleships up in the air by distances of approximately, I mean distance up in the air approximately equal to their length.... mine ships and, of course, they were primarily, there was one or two old American ships, but they were primarily ships which came out of the Japanese Navy which we captured during World War II.

KAPLAN:

What were you testing in these experiments? Were you testing air shock waves or ...

ATANASOFF:

We were testing, we were measuring both air waves and water waves; principally water waves because we were an underwater acoustic outfit and we had cables laid and various devices laid in the bottom of the bay and measured the magnitude of the force due to the explosion of the Atomic bombs. Of course, there is always the question that these atomic explosions were so close to the surface of the water, depths of approximately 250, 300 feet, maybe a little bit more ...

KAPLAN:

... (inaudible)

ATANASOFF:

Yes, yes, they were. One of them was and one of them was dropped from an airplane flying over into the water, yes. And, of course, these mines weren't well impacted and so what we really measured was a kind of a superficial shock wave which took off at the instant of atomic explosion.

I also measured the air pressure which occurred on the deck of a ship by employing an air pressure wave and knowing our distance, we had another coordinate (?) on the air pressure wave which surrounds an atomic explosion taking place in the open air.

KAPLAN:

Were you doing anything at all with radioactivity measurements or light?

ATANASOFF:

No, other people were doing the light and the radioactivity measurements.

KAPLAN:

Your division only did acoustic related research?

ATANASOFF:

Yes. Our responsibility was that and we were one of the departments who were prepared to do that within the Naval organization. And, late in the year, it became apparent, what was apparent to me all the time, that it was rather useless to attempt to run a computing machine project at the Naval Ordnance Laboratory where this effort would be secondary to so many other things. And it was decided in official circles above my level, in the chain of Naval command, to abandon this project. As far as I'm aware, no one had any complaint about the activities which we had but they understood the difficulties under which our investigation of computing machines at that time were carried.

KAPLAN:

Would you have wanted to continue with the project?

ATANASOFF:

I would have been very glad to if I could have -- I asked that the Navy put someone else in charge of the acoustic division and allow me to continue full-time in computing machines. I actually requested this change but this was something the Navy could not or would not do.

KAPLAN:

So, this computing project was really very dear to you? It was something that you wanted a lot?

ATANASOFF:

Well, it was something -- a major love of mine. Before the computing machine project was out of our laboratory, or completed or reported, we were visited by an officer of the United States Navy, who knew of the situation in the force of occupation in Europe and knew that a date in the early summer or late spring of 1947 that the British government would (end of side 1) attempt to destroy all the explosives which remained on the German island of Helgoland.

CORRECTION, OPERATOR. It turns out that perhaps a part about Crossroads should be modified a little while. The Crossroad Project was initiated in late '45 and the preparations for it began at that time and the actual Crossroads reached its fruition with explosions in the Pacific, at Bikini Atoll in the middle of the summer 1946. And, after that time, there were numerous reports to be written so that Crossroads Project in its entirety blanketed the entire Naval Ordnance Laboratory computer in its entirety.

Now, we will return to the Helgoland project. The Helgoland Project began early in the year of 1947 and it was a project that had to be carried out at a much faster rate. Perhaps I'd better tell the story a little in detail. The officer had visited all organizations, many organizations let's say and requested that they instrument the explosion which would take place in Helgoland. And these institutions had all said no, we do not have sufficient time to do instrument for this project and we refuse to do so and be made a laughing stock of the acoustic world.

It should be mentioned that this effort at Helgoland was to measure the forces of these explosions at distance reaching from five to ten kilometers to 1000 kilometers or more. At first, the officer from the Navy was only interested in the seismic disturbances created by the Helgoland explosion. After thinking about the project for a few days, I informed the officer from the Officer of Naval Research as follows that if he would procure the highest priority in the Navy for our activities that I would instrument his project.

At that time, there remained only eight weeks before the date on which the British Navy proposed to destroy Helgoland. In a day or two, he informed me that he has as a matter of fact secured this high priority for my project and I agree to go ahead and started immediately on the work which had to be done so rapidly.

You must understand that at this moment there existed not a single seismograph, portable seismograph available for this work. And I had to invent one, do the development work and construct a device for shipment to Europe within this very short space of time. In addition I knew that in order to secure useful information that I had to train a group of observers in the use of these instruments and this work all had to be done simultaneously.

But, the world did not know that I had in my private files a design for a seismometer which I had made some years before which seemed suitable for this purpose and colored my decision.

KAPLAN:

Why did you build a seismometer any time before this?

ATANASOFF:

I never built one. I just thought about building one.

KAPLAN:

Well, what led you to think of one?

ATANASOFF:

Because I'm interested in seismometers.... that the Naval Ordnance Laboratory Acoustic Division would instrument this explosion I received a very sincere call from Dr. Merle Tuve of the Carnegie Foundation. He said as follows: "Atanasoff, you're not a seismologist. You don't know what you're getting into. This is a field in which even experienced men have no idea how to proceed. How can you hope to move in in a period of eight weeks and do a job of this character?"

I will record for the reader that you must remember that this is the period before in the invention of the transistor and we had to instrument in terms of vacuum tubes in order to make a good portable, sensitive device. From the very beginning I knew that I had no way of computing the levels at which the various stations, which I would build, would operate. That is, we didn't know how intense the signal would be at each respective point.

The difficulties which this caused are as follows: if you do not know, you will either make them too sensitive in which case the recorder will move off scale and you will get no signal, except a few vertical lines. Or, if you make it too little sensitivity, the recorder will stay at the center of it path and will make excursions in neither direction except perhaps a small, useless squiggle. (BK's comment - inaudible)

So, I determined that the devices which I would construct would record by a logarithmic method so that I could have a very high dynamic range. Back over these days, I believe that I agree with Tuve that we were a rather fool-hearted group.... also true that the Acoustics Division of NOL had within its ranks experience in measuring and dealing with mechanical effects of the frequencies of seismic disturbances. And these techniques proved in the end to be of paramount importance.

This meant in part that we had transducers, that we had within our laboratories, transducers which were suitable for these frequencies.... device which transform from mechanical into electrical energy. Had no sooner started to work on this project that I remembered that this explosion would probably emit high levels of barometric disturbance in the earth's atmosphere. And I had read of one or two previous high levels of barometric pressure including the __ explosion and the Siberian meteor.

Frequencies of the signal which would eventuate from the seismic and the ... microbarographic disturbances were of a similar order, were of the same order. We decided to investigate whether it would be possible to make the measurements upon the seismic disturbances and then employ exactly the same apparatus with a different transducer for picking up the sonic disturbance. This great simplified the station in case we wished to recorded both types of disturbance and we made an immediate resolve to do

so.

I should remark that the two signals would never be coming simultaneously because the seismic disturbances transmitted through the earth at a relatively high speed; perhaps five to eight kilometers per hour. And the sonic disturbance -- no, I'm sorry. Five to eight kilometers per second and the sonic disturbances would be transmitted through the air at the speed of sound which amounts to about four/tenths of a kilometer per second.

CORRECTION, OPERATOR -- it's .34 meters per second, kilometers per second.

This means that at stations near at hand, you'd have to convert from one to the other with great rapidity but at stations further off, where most of our stations were located, there would be ample time for the conversion. My memory was that at Frankfurt ... where I operated a station near Frankfurt, there was a period of 20 minutes to change from one method of, from one transducer to the other.... remember the progress of this project, I recall that I was not only plaqued by scientific affairs but by the effects of the bureaucracy and military organizations to which I belonged. Our first priority came unstuck and I soon realized that our designs imposed upon the shop were being side-tracked to the benefit of the work of others.

Now this admiral in charge of the Naval Ordnance Laboratory and told him I was writing a letter for his signature for the Chief of Naval Operations stating that we were discontinuing the project in connection with the Helgoland explosion. He became enraged and said he would sign no such letter and I told him if he did not sign the letter that I would resign. The only thing that remained was for some face-saving operation to take place but my priorities were restored.

In the end we had a small respite because the British government decided, because of their own problems, to put off for a matter of three weeks the time of explosion. But, by this time our apparatus, the design -- I mean the invention, design and construction of our apparatus was nearing completion and the apparatus was going in boxes. Besides I had prepared a manual of instructions for those who were to operate stations and we had instrumented a total of 22 stations to be shipped to Europe.

I could recount the various difficulties which arose in Europe because of the _ nature of a military organization, the deficiency of transportation and jeeps, the theft of some chronometers which were necessary for timing of explosions and other things but, in the end, all 22 stations in Europe brought back useable data with one exception. This exception was caused by the injury to a jeep driver who was assigned to the project and not due to any failures of the recording apparatus. Every station that was in operation brought back useful data and these stations extended from Basamunda (phonetic sp.) on the north coast of Germany to Gorizia (phon. sp.) in North Italy.

We were awarded a citation by the branch of the Geophysical Union and by the Chief of the Bureau of Ordnance, who was citing us because of a hearty commendation which he

received (?).... I believe it was fair to state that this pioneer work was among several that laid the foundation for long-range detection of atomic bombs.

END OF INTERVIEW