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Interviewee: John V. Atanasoff (1903-1995)
Interviewer: Henry S. Tropp
Date: February 18, l972
Repository: Archives Center, National Museum of American History

TROPP:

This is an interview with Dr. John Atanasoff in the Project office of the Smithsonian Institution, on Friday, February l8, l972.

ATANASOFF:

I did a bit of work for this conference.

TROPP:

Great. Well, as I mentioned as we were coming down the hall, I'm most interested in hearing your view of the history of computing, the contributions that you felt you were most significant in making, and their role in the development of ideas.

ATANASOFF:

You take time out, and let me see that list of yours.

TROPP:

Sure.

ATANASOFF:

We'll do that first. You know why, because I will unconsciously try to relate may material to that list, and that would be useful, when the time comes to do relations.

TROPP:

OK. Let me turn this off, and I'll go get that list.

ATANASOFF:

Sure.

TROPP:

We've been talking about this notion of significant ideas in the development of the electronic computer, and what I'd like to do is to have you talk on that point in terms of your own view and your own contributions.

ATANASOFF:

I won't attempt to express my own view on it, really; I'll attempt to express my historical activities in the field and try to relate them to what I think the current ideas in regard to computing machines is. Will that be satisfactory?

TROPP:

Great.

ATANASOFF:

Now, I'm going to run along and just bring myself up to the position, where I felt a strong need for computing elements, showing how this need came about, attempting to give as much original material as I can in coming to this position, and then I will attempt at the very and, to summarize what I think the contribution of the Atanasoff-Berry operation was to computing machines. You must realize that this activity was very early. It was very lonesome, and it was far away in the middle of the United States, in a place that was isolated, Idea wise in those days, from the rest of the world. So that the other activities of computing machines, which were to some extent, partially, at the same time as mine, simultaneous with mine. I didn't really find out about in full until the latter part of my work.

This work of ours at Iowa was original, in the sense that it was done alone. It may not have been original. Only history will tell how original it was, in the history of ideas in modern computering.

TROPP:

I'm delighted that you want to talk about the motivation; the things that led you in to the ideas that you developed on computing equipment--what your needs were and what the motivation was, because to often, we only see the final result, and we never can find out why something developed the way it did, or why ideas flowed in a particular direction. Your isolation at Iowa wasn't just geographical. Nobody else was doing anything, and so you could have been in New York or anywhere else in the United States that was a center, and been just as isolated as you were at Iowa, because other people weren't doing anything.

ATANASOFF:

It occurs to me that perhaps I emphasized isolation slightly too much, and I'm reminded of your remarks of this. As a matter of fact, I was in the East, to mathematics and physics society meetings many times during this period. I also generally attended, during these years, the meetings of the AAAS, and similar things.

As a matter of fact, my first encounter with John Mauchly was at a AAAS Meeting in l940, in Philadelphia, Pennsylvania, where the AAAS just returned for another meeting. I think that was the first one in Philadelphia since that date.

Well, you know I graduated from the University of Florida in l925. My history previous to that time, I will just remind you, that I was the son of a man who had emigrated to the United States from Bulgaria, and who had become an electrical engineer, in his own right. However, I had been living in the country in Florida, and when I say country, I mean country. Places far from the civilized centers of the United States, where I was relying for ideas principally on my activity, and on a very small library, which my father had collected, and then later, on school libraries and the like. I had been active during these years. I had graduated from high school at the age of fifteen, and I had worked a year, and then had gone to the University of Florida.

Now, in my very earliest days, I was interested in all kinds of theoretical subjects, and this was true when I was even ten and eleven and twelve and thirteen and fourteen, and until I was finished with high school, and before I finished with high school, I knew that I would ultimately become a theoretical physicist. I had gone to the University of Florida, and I had taken electrical engineering.

I remember my decision to take electrical engineering. My father had wanted me to take chemical engineering, but I had systematically explored the different subjects at the University of Florida and found that electrical engineering was perhaps the most theoretical subject that there was going on there at that time. So, as I look back over my decision to take electrical engineering, I'm not upset at all. It was probably the best decision that was possible under the circumstances.

As I remember my idea of taking electrical engineering again, I'm reminded that at a later time, I discovered that M. Dirac also took electrical engineering as an undergraduate at the University of Manchester. Electrical engineering had advanced faster in theoretical science than in all the other branches of engineering, and in many schools, the electrical engineering course had more basic theory in it than any other course which is available to students.

I'll try to come back to Dirac in a little more detail later.

TROPP:

Okay.

ATANASOFF:

I'll tell you more about the University of Florida. I mostly made very good grades at the university of Florida. Really, few subjects gave me any difficulty whatsoever. I had done poorly in English, so I knew I should assault the English Department very hard. I studied English two or three times as much as my other subjects, and to this very day, I can make quotations from books, which we studied, from memory. I found pretty soon that I was not only memorizing English, but I was memorizing the books on the subject of English, like rhetoric and whatever. My English was improving and the instructor who was leading me in English was doing me a lot of good, and I remember from this that in English I made the second highest grade in my Freshman year. The first highest grade was made by a man who'd been an editor for The Stars and Stripes in World War I, in Europe. I'm very proud of this achievement because it was done with an extraordinary amount of effort.

ATANASOFF:

That's true.

TROPP:

Was it because of the rural high school that you attended?

ATANASOFF:

No, the teachers in high school were very unhappy about my English, and they couldn't furnish anything to bring me to a better effort in the subject. I remember an incident in connection with this. The English teacher in my high school told my younger sister that the English Department at the University of Florida was not very good. That was her only explanation of how well I did.

In any subject which I really studied, I made high grades, and in the end, most of the honorary societies which were available at the University of Florida. There was a chapter of Pi Kappa Phi, but there was no chapter of Phi Beta Kappa, but may I just record here, that many years later, they were looking over my grades at the University of Florida's English Department and decided to make me an alumni member of Phi Beta Kappa. I was fully inducted in Phi Beta Kappa at Johns Hopkins University, about l962.

TROPP:

A little late. But when you left Florida, you said you were interested in going into theoretical physics.

ATANASOFF:

Yes. Now what I'd done...I needed financial help at this stage. I had been earning my way at the University of Florida in a variety of ways, and one of them was teaching in the high school of Gainesville, Florida, where the University was located. This had provided a little extra remuneration, so I was able to study there for it, and yet work and make my living and go to the University of Florida. Now, I commenced getting out letters and asking for graduate appointments, as a man in my position usually did, and I wanted an appointment in mathematics, I thought, to begin my graduate career, pure mathematics, to begin my graduate career.

I got an early reply from E. R. Smith of Iowa State College, now the University. He said: "I would like your commitment that if I put you up for an appointment, that you will accept it." I said "yes" to Dr. E. R. Smith, and he never knew that later I was offered appointments at Harvard and two other Eastern universities. I had asked for a post-graduate stipend in mathematics. I don't know what would have happened if I had gone to one of those places, but I would not violate my commitment to E. R. Smith. I went off to Iowa State College, and began graduate work in mathematics, with a minor in physics.

TROPP:

You were going to talk about some of the people who were at Iowa State during that period, particularly in mathematics or statistics.

ATANASOFF:

Well, I will follow this in a general sort of a way. Most of the people at Iowa State College do not have a national or international reputation. Easily the best mathematician at Iowa State College was Everett S. Allen, who is still living in Ames, and who possessed a rare and capable theoretical mind. Perhaps he contributed more to my future grasp of mathematics than any other person. He taught such subjects as theory of relativity, but he was capable of teaching about anything, because his grasp of mathematics was so broad and so capable that he understood all subjects exceptionally well. He acted as my thesis advisor for my master's degree thesis, which I finished in the summer of l926, and which had to do with a problem in molecular theory, atomic molecular theory. We were seeking for some statistical performance from a theoretic kind of kinetic theoretic point of view, how the forces through the statistical distribution of charge around a nucleus would influence its deportment as an element of a gas I used, in these calculations. The theory of the virial.

TROPP:

Now, would you want to spell that.

ATANASOFF:

The theory of virial: V-I-R-I-A-L. Clausius being the great German physicist. In those days, the virial had not been used to depict the equation of stable gas, and it was regarded even by the expert on kinetic theory, Gees, as not a good approach to the equation of a state of a gas. I think in one of his early editions, you'll in his book on the kinetic theory of gas, you'll find a remark in his book to this effect. I am grateful that I made a contrary decision rather by myself, and since then, the theory of the virial has come into great prestige as the approach to the determination from the mechanics of the individual gas molecules, the transition from that to the equation of the state of the gas. A number of other men at Iowa State College contributed to my early grasp of mathematics and to physics, which was my first minor. In this connection, I would like to enumerate Dr. J. W. Woodrow, who was the Head of the Physics Department, and John Sidney Turr, who was a mathematician interested in a variety of subjects, who gave papers, but who had an intimate grasp of many erudite mathematical subjects. I had professor P. T. Robinson as an instructor in the period in the early theories of algebra, and had graduated from the University of Chicago, and brought me some clear algebraic concepts which have remained with me during life. There was another man by the name of Wholl. His first name was Vio. Vio Wholl, who worked in applied subjects, and taught me mathematical theory of elasticity.

In those days, I easily finished, in spite of having a half-time appointment, I easily finished the work for a master's degree, at the end of the first summer session of l926. After that, I commenced roving through the available courses, in the library, and in the various sources of inspiration, to bring myself to an understanding of mathematics and physics in a broad way, always aiming at becoming ultimately a theoretical physicist, but nevertheless, always being willing to turn aside to pure mathematics, because it has given me much hell as I trod these ways in the communicative life.

TROPP:

There's a good deal of pleasure, also, in pure mathematics as anything.

ATANASOFF:

And I'm speaking my mind as I go along. I don't want to wander too far a field.

TROPP:

Now, when you decided to do your doctoral work, what kinds of things were you interested in exploring?

ATANASOFF:

Well, you know, here I was at Iowa State College and Iowa State College had never given a doctor's degree in mathematics, and in a year or two, I decided to go to the University of Wisconsin for a doctor's degree. But meanwhile, I was at Iowa State

College, attempting to advance myself and my grasp of mathematics and physics as well as possible. Of course, there was a library and there were the laboratories and there were people who were interested in these subjects, and I felt myself a fairly free man, and took the best possible advantage of all these things.

TROPP:

Were you also teaching at Iowa State?

ATANASOFF:

By the second year, you see, I was teaching full-time, and so I had a full-time position, although I was perhaps doing a half... I was doing graduate work at a half time rate. During these years, I want to recount also my relationships with the Statistics Department at Iowa State College. The Statistics Department at Iowa State College has now come to be a very famous department in the United States and there are good reasons for this. Professor Snedecor, a man who also graduated in electrical engineering from, I believe, Tulane University, was Head of Statistics. Professor Snedecor, he never had a doctor's degree, had been propelled in this direction from a very strange source. There was a man named Wallace, Henry Wallace, who came to understand... (Interruption)

TROPP:

Sorry. You were talking about Snedecor's motivation from a gentleman by the name of Henry Wallace at Iowa, when we were interrupted.

ATANASOFF:

Snedecor had only a normal background in electrical engineering and was teaching elementary mathematics at Iowa State College in those years. This is perhaps between the years l920 and l925. There was a man named Henry Wallace who came from a great Iowa family, and who later became Vice-President of the United States, and Henry Wallace was commencing to get interested in seed corn. He realized that you couldn't move in agriculture, you couldn't do real basic scientific work in agriculture, without a grasp of statistics. He went up to Iowa State College looking around for somebody who would work with him in statistics. And he found Professor Snedecor, and propelled him in that direction, if you don't mind the words. As a matter of fact, Wallace and Snedecor wrote a book, an elementary book on statistics, which is well recorded in the libraries of the United States, and represented perhaps the first working treatise on the subject that ever was written in the world. Wallace not only influenced Snedecor to enter the field, he worked with him himself in the early days, and then his great prestige moved the whole agricultural institution, the whole agriculture department at Iowa State College, in the direction of much more work in statistics. So money was coming in, not only from the state of Iowa, but from the Federal Government who was supplying projects to Iowa State College. Even in those days, Iowa State College was one of the two or three

foremost Colleges of Agriculture in the United States. So there was money to work with, and so the statistics began its history at Iowa State College in this way.

Besides Snedecor in the early days in statistics at Iowa State College, there was A.E. Brandt, who was a kind of a rough and ready statistician, who also began life in some other field, but I can't think of it at the moment. But who, since there was the man, and there was money and there was a position, he entered the field, and you'll see the influence of Snedecor and Brandt on my work in computing machines, because I never really worked in statistics. I never took a course in statistics in my life. However, I've studied statistics on the side, and become fairly familiar with the fundamental import of statistics.

I was thus dumbfounded one day when Professor Snedecor said to me: "Look, why do you study the theory of probability? The theory of probability has no relation whatever to a good subject like statistics."

This was really Snedecor's serious understanding of the subject in that day. I didn't get my theory of statistics from Snedecor or Brandt, but you realize that they had their hands on most of the computing machines around Iowa State College, and my interest in computing machines went back years before this, but continued at that time and so I was looking at every statistical and at every computing machine that was around. So I was hanging around the Computing Laboratory a little bit, and of course, one of my principal *foci* of interest was the IBM Tabulator which was nothing but a large computing machine controlled by data entering via punch cards.

There was another influence of the Statistics Department that I should record. The great interest in statistics at Iowa State College, which was engendered by Snedecor and by Wallace, and by the presence there of the great agricultural activities which required a statistical activity in force, and caused visiting lecturers to be brought in. One of these visiting lecturers was Dr. R. A. Fisher, who spent the summer of l928, I believe, at Iowa State College. I will correct a remark that I made formerly. I did attend some formal lectures of R. A. Fisher in this period in statistics Now, there may be a correction at this point. It may be that Fisher's first visit did not occur until after I returned to Iowa State College in the fall of l930. It could have been in the summer of l93l, instead of the summer of l928 that R. A. Fisher made his first visit. There were, however, other lecturers here on statistics during my early days.

Now, in February or March of l929, I resolved to begin active work at the University of Wisconsin for the Ph.D. Degree. Iowa State College was kind and allowed me to take off in the middle of a quarter in order to make the second semester at the University of Wisconsin. I went to the University of Wisconsin, showing a great many courses as having been taken at Iowa State College, so in the spring semester of l929, I took only two courses. I took a course in quantum mechanics, with Professor J. H. van Vleck, and with Professor March, of the Mathematics Department, in theoretical physics. I'm sorry, it was in mathematics of elasticity. Two courses; one in the... Correction: I took three

courses. The first in quantum mechanics, the second half of a year's course in quantum mechanics, which I took with J. H. van Vleck; the second semester of a course in electro dynamics, with Professor Warren Weaver; and a course in mathematical theory of elasticity, taught from the epic tome by Love, in the Cambridge series. All these courses, I entered, were courses in the second semester, and knew that with the very good background that I had from Iowa State College, I was able to carry on the burden of the course very easily, and emerged with grades which were among the highest in the class.

I had the problem that when I had finished back at Iowa State College, I had finished doing work for a reading knowledge for German. I had had almost no French, and it was necessary for me to present a reading knowledge of French for a Ph.D. Degree. There was a time limit on when this material would be presented, because I was aiming at receiving the degree at the end of the l929-30 institutional year. So, during the summer, I had to pass this reading knowledge of French, and I studied French very hard for two weeks, which represented perhaps ninety percent of all the French that I knew at that time, with this two week study. I went up and took an examination and the Professor said: "There's something strange about your reading knowledge of French." I'm sure there was, but he passed me. This gave me some courage in regard to the study of foreign languages, and I have always been able to pick up a reading knowledge of a foreign language very easily since that time, and I have done it.

I began, in the summer of l929, thesis work with Professor J. H. van Vleck, leading to a doctor's degree in theoretical physics, in quantun mechanics, and the subject which we worked out for my thesis was the polarizability of the lithium atom, the electrical polarizability of the lithium atom. We were going to seek to make a mathematical calculation of the way an electric field, polarized the lithium atom, and the moments which we developed in lithium atom, and the result of exposure to a electrical field, electrostatic field. This was close to the main endeavor of J. H. van Vleck, who always worked on the effects of fields on atoms and molecules. This was a subject which interested me very much and I immediately commenced devoting the major part of my free time to this subject.

However, in order to support myself, I found it necessary to take an appointment as an instructor in mathematics in the l929-30 calendar year, and I carried this responsibility along with my graduate work. It was not as easy in those days to get money for pure research purposes as it is today, and so I had to do this actual teaching. I had, however, had a lot of experience back at Iowa State College in teaching elementary mathematics, and this was a relatively simple thing for me to do.

TROPP:

Again, in that academic year, l929-30, you were teaching mathematics for the University of Wisconsin, and you were also working on your thesis, under Professor van Vleck.

ATANASOFF:

Professor J. H. van Vleck left at the end of the first semester to spend a couple of years studying in Europe. During the spring of l929, we had had a visit by P.A.M. Dirac, who gave his famous basic lectures on the theory of quantum theory was in Europe, and he wanted to spend time there, so he deserted his graduate students and I believe I was the only major student that he had at that moment, and in the winter and spring of l930, he was replaced by a lecturer who was an important European specialist in quantum mechanics, by the name of Gregory Wentzel, from Zurich. He had left me with the responsibility of carrying on my thesis work with Wentzel. However Professor Wentzel was extremely competent. His advice was always succinct and valuable and work on my thesis progressed rapidly. It was easy to predict that I would be able to obtain the Ph.D. Degree not at the very end of the l930 spring semester, but at the end of the first summer semester. I want to recall, in particular for this purpose, the effect of my experiences on my interest in computing machines, because I spent a month in solid calculations in order to get the numerical results which were necessary to carry out my theoretical calculations of the polarizability of lithium. We were happy to find that although lithium was a very recalcitrant atom, is not easily accessible. to mathematical theory. As a matter of fact, easy accessibility by mathematical theory is largely limited to hydrogen. But I had commenced using as a model for the lithium atom, an approximate calculation made on lithium by Hylleraas, I believe, Hylleraas. And using Hylleraas' very efficient model in depicting the ground state of helium, I polarized this model and succeeded in obtaining the true polarizability of lithium, within a few percent.

TROPP:

You mentioned extensive calculations for a month. In what sense? In what kinds of mathematical approximations were you using?

ATANASOFF:

These calculations were pure numerical calculations with a computing machine.

TROPP:

With a computing machine. By that, you mean an IBM?

ATANASOFF:

No, I do not. I mean a Monroe Calculator. A Monroe Calculator was the only machine available which was accessible for this purpose. Without any doubt, this influenced and enhanced my past interest in computing machines, and this began my great interest in the possibility of having machines do the labor of man in this area.

In due course, I received a Ph.D. at the end of the first summer session in the summer of l930. I had been invited to return to Iowa State University as an assistant professor of

mathematics, and in the fall of l930 I returned there.

Now that, of course, wouldn't type up into good material, but it's all there in pretty good, decent form.

TROPP:

When I asked the question about the mathematical tools, that's really what I was concerned with, not the computational equipment, but the kinds of mathematical equations and approximations you had to work with, that would give us some extent of the degree of complexity of computational needs that were involved.

ATANASOFF:

Thank you very much. Your criticisms are very kindly useful and will permit me to make any dissertation better at this point. I, of course, could not write the differential equations for these equations of state and variations of equations of state of the molecule. That is not precisely right. I can write the differential equations, but having written them, there was no possibility of obtaining a solution along this path. Now, we're getting close to the exact. So, one attempted to depict the atom in terms of a principal in the calculus of variations. Following previous mathematicians, by the name of Ritz and Rayleigh.

TROPP:

Riez, that's R-I-T-Z.

ATANASOFF:

No, it's R-I-T-Z. Rayleigh is the famous man in the theory of sound. Now, I developed a pertubation method, in terms of the calculus of variations. I believe this is the first time that the pertubation method has been phrased in terms of the calculus of variations. Having obtained an integral, I then entered varium with suitable approximating functions to the para-mechanical state of a molecule. The functions had been obtained by Hylleraas as above mentioned, and made calculations on the polarizability, in terms of this pertubation method. You see, I was approximating in two ways: I was approximating through the ground state by means of integral method, but I was also then modifying these equations of state to the perturbed equations of state, whereas field is present. Differential equations would not have permitted a solution of even the ground state. Of course, when its perturbed, the physical situation was still more complicated, and it was necessary to use these double approximate methods.

Now that was good to get that in there.

TROPP:

Right. Thank you.

[END]