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Date: April 17, 1972
Repository: Archives Center, National Museum of American History

TROPP:

This is an interview with Dr. J. V. Atanasoff at his home on the 17th of April 1972. May we begin by picking up where you--when you returned to Iowa State College in the fall of 1930, having completed your Ph.D. at the University of Wisconsin.

ATANASOFF:

Thank you, I think that would be well. In the fall of 1930 I returned to Iowa State College as Assistant Professor of Mathematics. And for a period of years I was in the Mathematics Department alone. At first I was teaching just undergraduate work in Mathematics, but rather soon, perhaps in 1932, I was allowed to present graduate courses for such people as would take them. Almost from the beginning I was rather popular, and some of the people who had been previously teaching graduate courses were gradually displaced. Well, perhaps more accurately, some of the other graduate courses were displaced by the courses which I was presenting. This came about, of course, partly because the material which I was presenting represented a newer phase in the broad field of mathematical physics. I remember the first course I taught was a course in mechanics, or, more accurately, dynamics. I use the word dynamics to imply a theoretical course in mechanics which covered the field of mechanics in a very broad way, rather than theoretical way, and permitted the resolution of many problems on a basis that was impossible with the other courses in mechanics which had been previously taught. Now, today such a course would be called--would still be called "Theoretical Dynamics." I'm sure this material was not--did not originate in those immediate years--it had come down from the--from classical mechanics, but for some reason the transformation period of dynamics, for instance, had not become a part of the--of course work at Iowa State College; when I presented this material, there was a great deal of interest.

In the course of the next three or four years I developed courses in theoretical physics, quantum mechanics, thermodynamics, and kinetic theory. And most of these--and all of these except the last, the course had not been previously given at Iowa State College. Of course there'd been courses in heat, and courses in engineering thermodynamics, but a graduate--a post-graduate course in theoretical thermodynamics had not been given. In the kinetic theory course I expanded the previously given physics course in kinetic theory on a more and more theoretical basis. And I--(I'll stop). [Recorder off].

During these two or three years I also was made a professor of--jointly--of the

mathematics and physics department. At that time I moved my office from the mathematics department, where room was rather scarce, to the physics department. And from that period on until I left Iowa State College in l942 I had my office in room 52, Physics Building, and it was there that I spent these--these important years of my life.

The course in quantum mechanics attracted a good deal of attention, because quantum mechanics was then in its new and formative years, and the older, that theory of Bohr, as depicted by him and by Arnold Sommerfeld, had given way to the wave mechanics of Schroedinger and the matrix mechanics of Heisenberg; or, if you wish, to the broad new algebraic theory of P.A.M.--those are three initials--P.A.M. Dirac. D-I-R-A-C. I had become--while at the University of Wisconsin, I had become entirely familiar with this subject. I don't like the word "entirely"; correct that. I had become rather familiar with this subject. [Chuckle]. I like to be reasonably honest. And I had attended lectures by most of the originators of the theory, but more particularly by Dirac. And there was getting to be a great interest in every institution in the United States about the meaning of quantum mechanics in practical, everyday applications. These courses were partly given and were part--my courses were listed both in mathematics and physics--and partly given in one department, and partly in the other, as far as physical space goes. They could be used for full credit in either mathematics or physics in every case except, I believe, except, perhaps, kinetic theory. This permitted the graduate students of mathematics and physics to get a--to organize a broad foundation in the two subjects, whether he was a major in one of them or in the other.

This teaching of graduate work led, almost immediately, to thesis work in the various subjects and areas that I was--in which I was working. This resulted in a total--during the years between then and l942--of twenty master's degrees and seven Ph.D.'s.

TROPP:

Do you remember some of the areas in particular that the students did their theses work under your direction?

ATANASOFF:

Uh, yes. One subject--I believe the first one that I began graduate thesis work in was the subject of crystal dynamics. I had become interested in the mechanics of the anisotropic crystal media, and I had approached the problem of solving these tensor equations so as to permit practical solutions. Work in this field was also, was simultaneously going on in at least two other places, with Bell Telephone Laboratories, then in New York City, and with Professor Cady at Wesleyan in New England. The examination of the frequencies of vibrations of the quartz block and the interpretation of these frequencies I approached much in the same way as you would approach the frequencies of a vibrating molecule or atom, and we were able to give interpretation of most of the frequencies which occurred in an actual block. This required--however, the measurement of these frequencies required the construction of new physical apparatus which was not available at that time.

And I believe the first thesis applicant in this field was Mr. Robert Willson--W-I-double L-S-O-N, two Ls.

Now another subject in which I began thesis work was in the quantum mechanical depiction of the ground state of lithium. Mr. Curtis [?] Wells undertook this subject. And, although we had good wave-mechanical solutions for hydrogen and Hylleraas had developed a very accurate, although not, certainly, complete quantum-mechanical solution for helium, and the next molecule in line was lithium, and we did not have--we knew we had a general treatment of the lithium atom, but no--but the specific interactions of the various electrons was not known. We used--we approached this subject by using the wave functions of Hylleraas, put in a core, attempting to depict them with that and add additional terms to represent the motion of external electrons, and working this way Mr. Wells was able to come to an approximation for the ground state of lithium. I —

TROPP:

This was an extension, then, of your own thesis work?

ATANASOFF:

What? Yes, this was a thesis problem, and—

TROPP:

I say, this was essentially the problem that you'd worked on at Wisconsin, too.

ATANASOFF:

No.

TROPP:

Developing that model?

ATANASOFF:

No, it's quite different. It may look--you know, some of the same words...

TROPP:

Well, no. I say you were involved with the lithium—

ATANASOFF:

Yes--notice, yes; but you see we're working here with—

TROPP: He's working with the ground state, and you were concerned with--let me refresh my memory quickly,

ATANASOFF:

Don't need to. I'll repeat it.

TROPP:

polarization.

ATANASOFF:

I was working with the polarization of lithium. Now, he was working with …did we say "the polarization of lithium"? I shouldn't have said that. I should have said, "the polarization of helium." I'd been to work on the polarization of helium. He was working with lithium, which is an entirely different atom. I was just using the word helium in describing how we build up approximations for the wave models for lithium. These words look like mumble jumble,

TROPP:

No, no.

ATANASOFF:

I realize.

TROPP:

No, I was just referring back to our previous discussion two weeks ago, and I--my memory had told me that your thesis was on the polarizability of lithium—

ATANASOFF:

No, it's wrong.

TROPP:

It was on helium, rather.

ATANASOFF:

Yes, on helium.

TROPP:

OK. Excuse me.

ATANASOFF:

And now the--there was no, of course, no practical way to solve the differential equation for lithium, except by approximate methods, and these methods at this stage were those of Rayleigh--the methods we used at this stage were those of Rayleigh and Ritz, which had been used previously by Hylleraas in the analysis of the helium atom and previously in my thesis work for the polarizability of lithium--I'm sorry, helium. I get them mixed up myself.

TROPP:

I think we have lithium in our previous transcripts, so we ought to go over them.

ATANASOFF:

I'll correct that, for sure. And it'll be an easy thing for me to do. And I almost had myself mixed up a moment ago. And then the--I remind you that the Rayleigh-Ritz method is an indirect method of calculus--it's a direct method in the calculus of variations, in which we shift from the differential equation to a formulation in terms of the calculus of variations, and then attempt a direct solution in the problem of calculus variations by comparing possibilities--possible solutions, which are supposed to span the field of possibility. In reality, it requires ingenuity to build these systems of comparative functions, and we had to use physical--a physical sense in organizing them, and the comparison did then not include all possibilities, but only a limited range of possibilities, but one could always tell by the approximation to the truth how close to the fundamental state of lithium we were getting. This measurement of accuracy, of course, is subject to criticism.

We also worked on the approximate solution of some elastic problems. These were problems in elastic plates. Again, the problem--the differential equation and boundary conditions, the system comprising the differential equation and boundary conditions permits no direct solution in the ordinary sense. And it was only possible to use some approximate methods.

TROPP:

Why don't we--you were talking about getting your memory sharpened and going back up and picking up this train of the work that your graduate students were doing after you had begun to teach graduate courses at Iowa State in Ames.

ATANASOFF:

I think I can now give a somewhat more succinct picture of what happened among my thesis students at the Iowa State Colleges--at the Iowa State College, now University, in the years beginning about l932 until the year of l942. The problems on which my students worked were all in the area of mathematical physics. They had their origin and need in various useful problems. I think that I can summarize these problems best in the following way: There were roughly three fields in which—damn it, here's another problem.

There were roughly three--four fields in which these problems occurred. In the first place, I had developed an interest in the elastic problems in crystals, and, in particular, in the substance called quartz. Quartz was then being used to stabilize the frequency of broadcasting stations, and I knew that it was a material of great stability, and importance for many electronic applications. And I realized that it would--that there was much work here to be done.

Mr. Robert Willson commenced this study by analyzing the spectra--the frequency's response of a block of quartz--a rectangular parallelopiped of quartz cut in a--with specific relation to the axes of the crystal. In order to analyze these frequencies it was necessary at that time to develop new electronic materiel; this included the use of an oscillograph and sweep frequency circuits, which enabled all frequencies of response of a crystal block to be quickly located, identified, and measured. This work on quartz was continued by Mr. X, a gentleman whose name I'll supply later, and later by a gentleman by the name of Hart. These two latter gentlemen--these two last pieces of work together measured--interpreted the frequencies of quartz into elastic constants of the anisotropic material and into the temperature coefficients of these elastic constants, and also analyzed the transformation of quartz from the alpha to the beta state, as it is called.

TROPP:

I think maybe at this point for the less technical reader, and also for myself, I think it might be worth clarifying what you mean by a material which is anisotropic as opposed to one which is isotropic, and as much as you can about the two states alpha and beta. I think that's worth doing.

ATANASOFF:

I can do that. I can do that. Isotropy and anisotropy very simply out of head--I need to tell you that--that some substances, like, for instance, a piece of glass, or a piece of chalk, or a piece of iron in the normal, ordinary sense of the ordinary kind. If you looked at it in one direction it would look just the same if you had a picture and could look into the intricate molecular structure of the substance, it would look roughly the same as it would from a different direction. Of course, really, all of these materials would have some semblance of a crystal structure. And a crystal is always anisotropic in some sense or another. So, we have the glasses, for instance as typical illustrations of isometric

substances, but we have also polycrystalline materials as examples of anisotropic substances. I'm sorry, backwards, correction: We have as illustrations of isotropic substances the glasses, and polycrystalline materials. Now, crystal materials is always anisotropic, because if you look at a crystal in one direction that's what crystal means, it means that it looks different, that it behaves different, or when measured in different directions you get different measurements, and that—

TROPP:

Now, "looking at it different" you're talking about "according to different axes of symmetry"?

ATANASOFF:

Yes, I am. And, according to your direction of vision, or the direction of observation, as compared with the a--with the crystal axes--the typical crystal axes.

TROPP:

Mhm.

ATANASOFF:

Now, there's very little--most elastic theory of materials is done in terms of isotropic material. Anisotropy has been studied, basically, by a great number of people--I wish I could remember the name of the German author who is a--you might call the Father of Crystal Physics--I can't remember his name at the moment. But, in the United States one of the great experts in anisotropic materials was W. G. Cady, and I had an association with him during these years.

Now, as for the alpha-beta quartz: Alpha quartz is a kind of quartz that occurs at ordinary temperature, but when the temperatures were raised above a critical value, whose value I don't remember, it becomes Beta quartz and its crystal symmetry change comes. Instead of three-fold, it's six-fold symmetry about the axis of symmetry. Both substances are piezoelectric, however; by piezoelectric we mean a crystal which, when compressed or extended in one or more directions, acquires some transmission of charge or electrical polarization. Quartz is interesting because it is piezoelectric, that is, because you can place a block of quartz between two electric plates, and then the quartz will vibrate. But these vibrations were automatically coupled with electrical circuit comprising the two electric plates. That's because of the piezoelectric property.

TROPP:

I didn't mean to get you totally off the track, but I—

ATANASOFF:

That's all right. Now we'll go along, now you see we have Willson doing the preliminary examination, preparation of apparatus, and then later Mr. X and Mr.—

TROPP:

Hart.

ATANASOFF:

Hart, doing further measurements, which, I believe, remain to this very day as the best known measurements of the piezoelectric constants of quartz and temperature coefficients. That's to the best of my knowledge—

TROPP:

And these are the critical values of the—

ATANASOFF:

The critical values, which were measured in those days. Now, these theses were--were experimental, but they were, at least in part, theoretical, because the theory had to be developed, the theory was partly not known and partly had to be applied to the specific pieces of apparatus which we used. So these theses contained elements of theoretical.

Then I had the thesis by--a group of theses by--Then we had a group of theses, by Thurston Bagrials [?] on the realm state of lithium,--

TROPP:

Which is the one we talked about earlier?

ATANASOFF:

Which we talked about earlier, and a thesis by Cook on elastic plates; and a thesis by Thorne, also on elastic plates. And these theses--in each of these theses one was faced with a practical physical problem, the first one in quantum mechanics, and the other two in the theory of elasticity, in which one sought to solve systems of partial differential equations with given boundary conditions. These were linear systems.

What are the methods of solving these problems? What was done in these three theses was to apply the ordinary methods that had been used previously to these problems.

TROPP:

"The ordinary methods" you're essentially talking about methods of approximation?

ATANASOFF:

I mean--I don't mean the ordinary methods that--in which a direct solutions for the partial differential equations are found, because such--in any practical--in most practical problems no such approach is possible, but I mean the ordinary methods in the sense that they were derived from Rayleigh and Ritz and extensions thereof which were devised, or we devised or--in order to obtain approximate solutions to the problem in question, and they're practical operating problems in mathematical physics, you might say. And then we have the work of Mr. Gross, in which we sought to explore the whole field of possibilities of approaches to the solution of problems in infinite algebras--Corrections,

TROPP:

I was going to say the—

ATANASOFF:

corrections: in differential equations. Now, a differential equation from an algebraic point of view is a problem--it consists of an infinite number of algebraic equations. It corresponds to an infinite number. So we say a differential equation is a problem in an infinite algebra. Did I transmit?

TROPP:

Right. I say if you're going to accept that definition, then "infinite algebra" makes sense, in the sense of this analogy to the kinds of techniques that are applied to solutions of these partial differential equations.

ATANASOFF:

If you're going to just use algebraic problems with constant coefficients in order to get anything equivalent to the differential equation, you have to have an infinite system.

TROPP:

Right.

ATANASOFF:

And this then became a more highly theoretical work, and in two theses, one for the Master's Degree and one for the Doctor's Degree, Mr. Gross explored and demonstrated the complete system of possibilities through other solutions--for approximate solutions of

linear, infinite, algebraic systems. These systems do not only include differential systems, but they include integral systems, and other types of systems which are not easy to describe in ordinary terms. Difference equations, for instance, fall into this category; integral equations of both types fall into this category. I will say, however, that we restricted ourselves to linear systems.

To summarize the work, you see, here we are faced with the whole problem of application of mathematics to practical physical problems. How are we to proceed? We already know far more mathematics than we have the--we can already set up far more mathematical problems than we can solve. The pressure is on the solution; how one gets results; how do you get numerical results; how can you use it in design of an engine or the solution of a problem in biology, or whatever the case may be.

I might also, at this point, emphasize that, of course, the fact that we limited our approach to the problem to linear problems is a severe limitation; however, it should be understood that linear problems do comprise a great portion of the most important problems of mathematical physics, and—

TROPP:

You know it's been--this is sort of a side comment, but I remember a prominent physicist once talking to a group of undergraduates, and they were asking what courses they should take, and he, you know, said "By all means you need to take courses in differential equations, both ordinary and partial" and they said "Oh, why? Will we use that work a lot?" And he said, "Well, probably in those courses you'll run across differential equations that have solutions. And the rest of your life as a physicist you may never see another one." [Laugh].

ATANASOFF:

That's a very true comment on the nature of differential equations. [Chuckle]. And in the early days of mathematical physics the problem was to find a problem that had a differential equation that could be solved, instead of some other kind of problem.

But, now, I hope I can transmit at this time the fact that when one used these methods which would abide by Gross and which had also--I'm sorry: When we use these problems such as those devised by Rayleigh and Ritz, and other problems designed by other people, such as the more general approach which is derived--which was devised by Gross, we transform the problem in analysis into a problem in algebra with an infinite number of equations, and the practical problem is to approach the solution of this infinite system of linear algebraic equations. Now, we approach the problem of infinite system of linear algebraic equations by taking a few equations--a small part of a few equations and solving them, and, under suitable conditions, solutions obtained in this way approach the true solution of the larger problem. Now, I've never said this quite this way before, I hope this transmits fairly well. We passed from analysis to infinite algebra, and then to finite

algebra.

But the trouble is, to get reasonable approximations the linear equations have to be very large, and they have to be very numerous. And this burdens the computational effort. And these burdens on the computational effort were very evident in the work of Wells, and Cook, and Thorne. And they were very evident, also, in my own thesis work at the University of Wisconsin previously. And they were very evident in the papers published in the American Physical Society and in every other direction that man looked.

TROPP:

You just introduced the problem of the burden of heavy computation, and I think this is a good time to expand on that difficulty that may relate it to the theses you mentioned.

ATANASOFF:

I just pointed out the fact that a great many practical problems in mathematical physics can be reduced to the problem of solving infinite systems of linear algebraic equations, and that has an approximation. We can solve finite equations of sufficient size. However, the problem of solving systems of linear algebraic equations becomes very burdensome from a practical, manual calculation point of view. The amount of work goes up rapidly with the number of equations, the number of known--the number of unknowns which are, in general, equal. This is necessary in order that the-- that there be a single, unique solution for the system. The amount of work necessary for solving systems goes up with some rather high power of the number of equations and the number of unknowns. Various people have made various estimates of this, and one estimate, which I remember, is that the work goes up with the cube. If the work is arranged in other ways, for instance according to Cramer's rule for the solutions of linear systems -- is it Cramer's rule?—

TROPP:

Right.

ATANASOFF:

why, it's easy to prove, but the--my work rises much more rapidly than this. Strangely enough, Cramer's Rule does not help the large systems at all. Every graduate student that I had was engaged in the--almost every graduate student I had was engaged in these kind of calculations. And his thesis--a large portion of the time and effort of his thesis was related to these calculations.

TROPP:

Would you give me some idea of the magnitude, in terms of number of equations, that these various approximations involved? We're talking about ten--a system of ten

equations, a system of twelve, or what order of magnitude?

ATANASOFF:

I believe that in Hylleraas's original work on the ground state of helium--of the ground state of helium, he got very good approximations with what amounted to three or four equations, and three or four unknowns. But I do not have before me Willson's treatment of lithium that required a great many more equations and a great many more unknowns before a satisfactory approximation was obtained, which is true in the theory of plates as this number increases in order to yield, by practically satisfactory solutions, this--the number of equations, the number of unknowns increases rapidly with the number of dimensions of a space in which we will work, or with the degree of--the number of independent quadrants of the corresponding mechanical system.

TROPP:

So you're talking about, say nine--an axis of symmetry of order of magnitude nine, and you would need much more than you would, say, for a three dimensional...

ATANASOFF:

That's true. And this is particularly the case in solving a practical--in attempting to get a practical solution for the frequencies of vibration of a cube of quartz, because you want--you may not need so many equations for the fundamental mode of oscillation, but if you wish to get the higher frequency modes of oscillation that requires many more equations.

TROPP:

Now Gross's work was, you indicated, a very basic work, in dealing with this infinite system. Was part of this theoretical work putting some kind of a boundary condition on the number of equations you could--you would have to use in order to get a satisfactory solution?

ATANASOFF:

There is work of that kind there. It is also true that, I believe to this very day, that a great many of the practical problems have no limit placed upon the boundary. I have recently discussed this with people working in the theory of the application of computing machines to problems of this type, and I was reminded that, of course, the limit of error, which is really what you're speaking of,

TROPP:

Right.

ATANASOFF:

what error one will have under various conditions; how many equations you need in order to get a reasonable approximation. The computing art--I mean the computing machines possess so much more power today that one does not need to know what the accuracy is, one just obtains sufficient accuracy by using large enough systems. I am not entirely satisfied with this explanation, even today. How do you feel about it?

TROPP:

I don't like the idea of any problem that says I have to use an atomic cannon when, if I worked on the theory a little harder I could get by with a little pop gun.

ATANASOFF:

May I tell you, there's no--there's no question of working with the theory a little harder; it's a great deal harder. I have systems in which limits of error can be--I know of systems in which limits of error can be described. This does not apply to very many cases. For instance, now let's take the problem of--the very simple problem of expanding an unknown function in a power series in one variable.

TROPP:

The Taylor series expansion.

ATANASOFF:

The Taylor series. Now, there are simple formulas in the Taylor series for a limit of error. They're also—

TROPP:

They look simple. [Laugh].

ATANASOFF:

Well, they're pretty simple in that case, but go immediately to a Fourier series, and there are formulas there for limits of error, too. But you're almost at the end. If you, instead of trying to solve the equation $y=f(x)$, if you're attempting to solve a differential equation, then almost invariably there are no known formulas for the limit of error. Now--there are exceptions, but they are very rare. I've worked an awful lot on this problem, I wish I knew more about it. If I knew more about it, I'd know more than people know today.

TROPP:

Right. Well, I say, it bothers me though that because we have the high powered equipment, we say, "well, if

ATANASOFF:

Well, now—

TROPP:

we have to use 10,000 equations and our machine will handle that many, fine, let's just do it, and we know our error is going to be well within bounds."

ATANASOFF:

Now—

TROPP:

Without the theory, how do we—

ATANASOFF:

I'd rather be going a little bit more in detail in the--I'm glad to revise these tapes and try to get them into better order, and this tape is in kind of a difficult position at this point. I will say that Mr. Gross's work concerned itself principally with the methods for passing from the analytical problem of a linear operational equation to the algebraic problem of an infinite system. It concerns itself almost completely with that problem.

TROPP:

So this is primarily why then the operator will—

ATANASOFF:

And, and I'll add immediately that there's no uniqueness to this method of passing. But there are many, many ways by which one can pass from the one to the other, and that these methods have different utility in different applications, and they have, of course, each a separate approximation formula--I mean, error formula.

TROPP:

OK. So it's really, then, in the area--his thesis, then, is primarily in the area of what we call today functional analysis, in terms of the operator theory and the set of

transformations which apply.

ATANASOFF:

Yes, that's true. Yes, it is. You say function element?

TROPP:

Functional.

ATANASOFF:

Functional analysis. Yes, are you pretty good at that?

TROPP:

No.

ATANASOFF:

I was going to ask you two or three questions.

TROPP:

Well, you can ask and if I can answer them I will, but it's highly unlikely. [Laugh].

[RECORDER OFF]

ATANASOFF:

After the pleasure of this discussion with you, I just want to say that our knowledge, at that moment, of formulas for the limit of error was very limited.

TROPP:

I think you're right in that they still are.

ATANASOFF:

I believe that is--that that's the situation.

TROPP:

I asked the question because, really, of my limited knowledge in the field.

ATANASOFF:

I--most certainly a good question. Well, here I was surrounded by the need of computation. Almost all these computations were leading to the solution--to the practical solution of linear simultaneous equations. I had been interested in computing machines intensely for a long time, but this moment I was looking for the various methods which might be acquired through this practical solution of these linear systems. I first tried analog methods. You know in those days we had immense set-up boards which were devised for solving somewhat similar systems of electric measures for transmission lines. I tried to concede the utility of these devices for the purpose. I considered the utility, of course, of such methods as those used by Bush, [the] integraph.

TROPP:

Right. The—

ATANASOFF:

Is that the correct name?

TROPP:

Right. Then you're thinking of the differential analyzer.

ATANASOFF:

I'm sorry, the Bush differential analyzer is what I'm trying to say. I had such methods at hand. I actually developed the thesis by a man called—

[RECORDER OFF]

ATANASOFF:

I had a thesis--about this time I had a thesis done by Mr. Lynn Hannum. Mr. Lynn Hannum was examining the various methods which have been used for solving Laplace's equations in two dimensions, such as soap bubble--such as the soap bubble method. But he devised a new method--a new method consisted in developing a function satisfying the differential equation and the boundary conditions by cutting a block of paraffin into such a shape that the upper surface would be the functional--the function required. The solution proceeded by using a mechanism which when applied to the surface would automatically cut the surface into the required shape.

TROPP:

Are we talking about minimal surface methods now, or is this another problem. I'm—

ATANASOFF:

No, this is not a minimal method--minimal surface method. It's an analog method of solving a partial differential equation, in which the linear algebraic equations are by-passed.

TROPP:

Mhm.

ATANASOFF:

You pass directly from the differential equation; you'd use a machine which is designed for that differential equation, a mechanism which is designed for that differential equation. You have a block of quartz--I'm sorry--a block of paraffin, and the block of paraffin is to be shaped into such a shape that its upper surface will--the distance above the horizontal plane being z , that z will be a function of x and y, and the function of x and y is the function which will satisfy the differential equation in this case--the differential equation in this case is Laplace's equation, and, of course, this solution also satisfies the boundary conditions. This analog method of solving partial differential equations is obtained by successive applications of the mechanism to the surface, so that the surface is slowly carved into the correct shape. This is an approximate method like other methods that are being considered here, but it's a—

TROPP:

Now, let me push this a little further, because this is the first I've heard this

ATANASOFF:

Is that so?

TROPP:

mentioned from you. Are you saying that you use, in a sense, a differential analyzer in order to shape the paraffin surface in terms of

ATANASOFF:

You know,

TROPP:

approximating the solutions?

ATANASOFF:

there's a little bit of truth in that. Not too much.

TROPP:

Oh, ok.

ATANASOFF:

I used a differential analyzer, but not one like Mr. Bush's.

TROPP:

No, no, no. I'm not thinking of "like anybody else," but...

ATANASOFF:

What actually happened was, we approximated the partial differential equation by a partial difference equation, and the partial differential equation was realized by a linkage mechanism in the mechanism in question,

TROPP:

OK.

ATANASOFF:

and the thing had a cutting edge, and so at each pass of the apparatus over the top, why, coordinates of various points are taken, and the coordinates of four points are taken, and the coordinate of the fifth point is revised to meet the need, so that the thing approaches the solution to a partial differential equation, essentially.

TROPP:

So you were using successive difference equations in order to—

ATANASOFF:

The difference equation was the same one, in an algebraic sense. Of course, the values were different. In the sense that it was a succession of difference equations if you put specific values in there.

TROPP:

Right. And each of those then gives you a different set of coordinates, a further refinement to the approximate solution.

ATANASOFF:

Now this was a--I commenced to explore a--let's say, I want to say something else first. First, it soon became evident that the accuracy which one could obtain by analog methods was limited. It was too limited for most practical applications. And was--and was obtained at too great a cost. This is true, for instance, of the Bush differential analyzer. You could make the Bush differential analyzer operate to force infinite figures, but in order to do it you have to build an unduly expensive device, and—

TROPP:

By "forcing it" you mean successive machines hooked in series until you can get the error down, so that you can get the torque—

ATANASOFF:

Now you see, a Bush differential analyzer is not--it directly proceeds to the solution of an ordinary differential equation, but--and it is used in successive approximation for the solution of some partial differential equations. But let's consider it just in its application to ordinary differential equations. Even here, if you want to get an accuracy of four significant figures, you have to use an inordinately expensive differential analyzer. And, you know, he'd build about two or three models and he was just about at an end. But, you know that, during the war, the, of course, perhaps the best analog machines were made for the solution of the antiaircraft fire problem. These analog machines were made in terms--not in terms of rotation as the Bush Differential Analyzer, but in terms of electrical values. And--but I might tell you that these machines were a success here. There was--the accuracy required for antiaircraft fire for practical reasons is no greater than those machines were capable of obtaining. So that was a successful application. But these machines probably were more accurate than the differential analyzer produced by Bush for purely theoretical purposes, they are much better quality stuff. A lot more money was spent on them, of course, in the course of the war.

TROPP:

I want to back off to something that we have talked about, but I don't have on tape. We know, at least on paper evidence, of the first existence of the word "analog" as applied to a computer, and these notes I mention of Mauchly's in l94l, and you're using the word "analog" in reference to work in the thirties. Now obviously at some point in time, working with your students, you were also--you shared a common vocabulary. When did you first start using the phrase "analog computer," and how did you define it when you first used it?

ATANASOFF:

You know, about this time I was working on all the various computational methods, and it became clear that we could divide computational methods into two kind: those that worked by means of magnitude which are continuously variable, and those which--which worked by means of magnitudes which are represented by a number system, and which are therefore discretely variable only. Now those that use the number system I called computer machines proper, and those that used the continuous variable, like a slide rule, or like a Bush differential analyzer, or like Hannum's machine for producing a surface for--which solved the--Laplace's equation, I called analog machines.

TROPP:

About what time were you using this--were you defining these two terms and beginning to use them in that way?

ATANASOFF:

I think about the--about l935 or '36, about in those years, when I was thinking deeply on these questions. The pressures were very high in l935 and l936 in regard to computational needs.

TROPP:

OK, I want to back off. I keep breaking in.

ATANASOFF:

That's all right.

TROPP:

I want to back off for one thing in terms of computational needs. We talked on a number of occasions about the existence at Iowa State of, in a sense, computational labs;

ATANASOFF:

Yes.

TROPP:

because of the needs of the agriculture department, the interest in Henry Wallace in statistical work, the growth of mathematical statistics at that particular institution. What kinds of things did your students and you have available to you to perform computations

during the middle thirties, the period that we're talking about?

ATANASOFF:

You know, it's a very strange thing, but we had almost nothing.

TROPP:

[Laugh].

ATANASOFF:

This is—

TROPP:

That is strange, because I didn't expect anything so...

ATANASOFF:

We had almost nothing. You know, in the first place there were agricultural problems which could command all the machinery which is available in the statistics laboratory, and here is Atanasoff and his group working over in physics; and we had no budget, and we couldn't--I'm sure that, if we had asked for it, we could have gotten a limited amount of time, but as far as the actual use of these machines at any extensive rate, they were not available to us.

TROPP:

So most of the calculations you're talking about, then, were done by hand?

ATANASOFF:

Were done by hand, and were done with ordinary computing machines of more or less the Monroe type--with a Monroe calculator or something of that kind--I believe Monroe. Now, there are other--there are a few--a few cases in which we made use of the machinery in the Computing Laboratory and I might outline one or two of those as side issues here in order to pull those—

TROPP:

Then you will fill the picture in the sense of what was available at Iowa State that you were familiar with.

ATANASOFF:

Well, what we knew--oh, I knew everything that was available. What, of course, was there was an IBM tabulator, and the standard punched card equipment. At first--when I was first there, why it was of the, old forty-five column type, and then later it became of the eighty column type, and after I had returned to Iowa State College in the fall of l930, why, it was of the eighty column type, entirely. And Iowa State College managed to make arrangements with the International Business Machines Corporation to have on hand more of the best, and the latest tabulator, and it was a considerable budget and there were girls working continuously on these machines, doing the work in statistics, which was necessary for the very extensive agricultural activities at Iowa State.

TROPP:

Would you describe the work going on there in the early thirties as being primarily data gathering and listing, or computational, or a combination of both? Were they actually doing computational statistics?

ATANASOFF:

You know what happened was--what happened, of course, was, it was mostly data compilation. And then when they had to do a correlation or a test, why that would have to be done by the girl with a Monroe.

TROPP:

OK.

ATANASOFF:

And so there was not--so that that really was ...

TROPP:

...

ATANASOFF:

And I think that our standards there at that time were perhaps more or less comparable to those in most institutions

TROPP:

Right.

ATANASOFF:

in the world. Well, then you've answered my question, because I was curious as to whether or not they had perhaps pushed the equipment a little bit, because of those needs. But I was attempting to push the equipment. And sometimes I had an ally, and that was Mr. A. E. Brandt. There existed a problem in the analysis of complex spectra. Have I told you anything about this?

TROPP:

...

ATANASOFF:

And this--in this case one had the fre--various frequencies which are measured for a spectrum and one had to analyze the problem and determine a set of values, the differences between which corresponded with frequencies in the lines of seven levels between which the quantum mechanical transmission corresponded to, when due cognizance is taken upon constant h, one obtained the frequency of vibration of that particular line. Now, this work had been done manually, but I knew this was a very arduous problem of computation, and I decided that it could be done by an IBM tabulator. Try as I would, however, I could not get the IBM tabulator, in its present form, to do the job. What actually happened was, I built a conditional piece of apparatus which, when plugged into an IBM tabulator, would allow one number to be entered from a card, and another number to be entered from a set-up board as a W card passed, so that the—

TROPP:

By "set-up board" you mean a standard plug board?

ATANASOFF:

Plug board, yes. So the machine was fooled into thinking it was receiving the second card when, as a matter of fact, it wasn't receiving the second card, but it was receiving a number from the keyboard. By using this device I was able to organize the analysis of complex spectra by the use of the IBM tabulator. And Mr. Brandt and I worked out this method, and there is a paper published on it in the *Journal of the Optical Society.* You have a copy, I'm sure, in your files.

Later on, I commenced to explore the possibility of solving the systems of equations by use of an IBM tabulator. Now, you see, in working out a correlation you had to solve a system of equations, but in this statistical laboratory at Iowa State College they just had a lot of girls, who did that with the Monroes, electric Monroes, I guess, they were commencing to be in those days. And I knew that it would be a great boon in statistics, as well as in my work, if we could find a method of using the IBM tabulator for the purpose. Particularly, the IBM tabulator which we had had forty computing columns, although it

was an eighty column card, the tabulator itself would only carry forty columns. So I was limited to computation in those forty columns, and then those computations weren't very easy in an IBM tabulator, anyway. You had to devise routines so as to turn an IBM adder into a computer. In those days the tabulator would do no more than add and subtract. Subtraction, I believe, was by means of complements even at that time.

TROPP:

I think so.

ATANASOFF:

And I worked for some months on this problem, and I came to a very sad conclusion. The forty computing columns were just not sufficient to enable me to obtain power in the solution of the problems which I was to solve. I couldn't solve enough equations in enough unknowns with enough accuracy to provide any solution to the problems of mathematical physics which we were trying to deal with.

TROPP:

I'm going to bring you back to another, what may be trivia, but the use of the word "computer"--it still strikes me that in the thirties when you used the work "computer" you were talking about a person, not a thing--but the person who did calculations. And you—

ATANASOFF:

Sometimes I am.

TROPP:

And you used the word computer in that period in the other sense, particularly when you defined the difference between the continuous and the discrete. So you were already thinking of a computer, not as a person who sat down and did calculations, but as a mechanism which mechanically performed these calculations for you. To your knowledge when does that different kind of use of that word come in? I'm beginning—

ATANASOFF:

I would say that that use of the word computer was common with us throughout the period from '32--from l932 onward. We called those things computers--we just allowed the context to differentiate between whether the computer was a man or a machine.

Now, when I came to this sad conclusion that the capacity of the IBM tabulator was not sufficient, I, of course, explored the possibility of getting an eighty column machine. The eighty column machines had been made. There were not many of them around, and there

was no possibility of Iowa State College investing the money [from] the budget to procure the eighty column machine. And I couldn't--I knew that once [?] examined what could be accomplished with an eighty column machine, and I found out that wasn't enough to do my job. And I needed many more columns than that. And so I wasn't really very serious about having Iowa State College pay for an eighty column IBM tabulator. Besides, while I did some preliminary layouts, the problem--the solution of linear algebraic equations by means of an IBM tabulator looked like an extremely difficult proposition, anyway. It didn't work out well in other respects.

Then there existed a period of some months in which I approached the problem by the use of--I theoretically approached the problem by the use of ganged systems of Monroe calculators. We had one continuous shaft that drove all the tabulators in synchronism.

TROPP:

You mean the idea of putting in some data on one, taking the accumulation of that, whether it's a Multiplication

ATANASOFF:

No,

TROPP:

into the other, or—

ATANASOFF:

No, nope, nope. You have a lot of machines in a line.

TROPP:

MHm.

ATANASOFF:

The rotors rotate in harmony, at once.

TROPP:

Synchronous.

ATANASOFF:

Synchronism--complete synchronism. When one is--moves forward, they all go forward.

When one goes backward, they all go backward. Now you put the coefficients of one--you have as many machines as you want unknowns in--as you want unknowns plus one in the system of equations. If you want to solve n equations, you use n+1 machines. You put the coefficients in the successive machines from left to right of one of the linear equations. You put the right-hand term in the last machine at the right of the n+1 machines. You revolve the machine one revolution forward. Now--you cleared the keyboard,

TROPP:

Right.

ATANASOFF:

and you put in all of these keyboards the coefficient on the second equation, including the constant term in the last machine at the right. Then--now you know there are two operations which are to be performed; you're going to crank the machine forward and backward as the case may be, and you're going to turn the—

TROPP:

By "crank the machine" you mean each machine individually—

ATANASOFF:

No, no--all together.

TROPP:

All together?

ATANASOFF:

The combination, crank the combination. So forward and backward, always together.

TROPP:

OK.

ATANASOFF:

Now, remember there is another adjustment which the Monroe has, and that's a little device to the left by which you change the relative position of carriage and gear wheels. You know how you can move the—

TROPP:

Right, for decimal point adjustment.

ATANASOFF:

That's right. Yes. Now, the machines are all placed in the same position to begin with, and they're all moved simultaneously. So you have two adjustments, the shift in the carriage and the revolutions of the machine. And then you manage this machine--manage these two controls so that the coefficients--so that the number contained in one of the machines is reduced to zero, and then they will be contained on the dials of the rest of the machine, the coefficients of the new equation of one less unknown.

TROPP:

So if you had n equations, you had n+1 machines, you would do this precisely n times reducing it to a system that had n-1 equations and n-1 unknown.

ATANASOFF:

That's right.

TROPP:

...

ATANASOFF:

That's right. You'd do this whole operation

TROPP:

Whole operation.

ATANASOFF:

of the putting them in and starting over again

TROPP:

Right.

ATANASOFF:

n times, and then you would have a system of equations of n-1 variables.

TROPP:

You keep doing this until you've reduced it to a single equation of one unknown?

ATANASOFF:

I foresaw this--that this was possible and I thought--insofar as I know, these thoughts were original with me in any respect. I don't know how--whether anybody had conceived of this possibility—

TROPP:

I'm not sure about the detail, but I think other people had thought about this idea of hooking machines together. They may have had different concepts and different ways,

ATANASOFF:

I didn't—

TROPP:

different kinds of problems; but it seemed a natural kind of thing to do.

ATANASOFF:

Sure. Now, I foresaw that this was possible. I did an exploration as to the total cost and the reasonableness of this, and this was a much more reasonable prospect than the IBM tabulator and would have succeeded much better. You see, you might have machines with twelve places, and if you had twenty machines and twelve places, well, then you'd have two hundred and forty places on the base ten places available to you instead of eighty, but the structure would be much better than that in the decision of the system of equations. You can see that, immediately, because, you see, this really works out pretty well in the solution of equations. You actually do what the computers do in solving the system of equations. And—

TROPP:

Well, you're doing mechanically, essentially, what we could do--we would do by hand if we lived long enough.

ATANASOFF:

Exactly. That's exactly what it is.

TROPP:

And the method of reduction to zero is essentially the process we go through of eliminating an unknown, except we're doing it mechanically instead of manually.

ATANASOFF:

Why don't we quit here for now?

TROPP:

OK.

ATANASOFF:

It's a good quitting place I think.

[END OF INTERVIEW]