An interview with
SAUL ABARBANEL

Conducted by Philip Davis
on
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at the
Department of Applied Mathematics, Brown University

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ABSTRACT:
ABARBANEL describes his work in numerical analysis, his use of early computers, and his work with a variety of colleagues in applied mathematics. Abarbanel was born and did his early schooling in Tel Aviv, Israel, and in high school developed an interest in mathematics. After serving in the Israeli army, Abarbanel entered MIT in 1950 as an as an engineering major and took courses with Adolf Hurwitz, Francis Begnaud Hildebrand, and Philip Franklin. He found himself increasing drawn to applied mathematics, however, and by the time he began work on his Ph.D. at MIT he had switched from aeronautics to applied mathematics under the tutelage of Norman Levinson. Abarbanel recalls the frustration of dropping the punch cards for his program for the IBM 1604 that MIT was using in 1958 when he was working on his dissertation, but also notes that this work convinced him of the importance of computers. Abarbanel also relates a humorous story about Norbert Weiner, his famed linguistic aptitude, and his lesser-known interest in chess. Three years after receiving his Ph.D., Abarbanel returned to Israel, where he spent the rest of his career at Tel Aviv University. He used the WEIZAC computer at the Weizmann Institute, and in the late 1960s worked on a CDC machine and an early transistorized Philco computer owned by the Israeli army. Although Arbarbanel's early work was more computational, his later work reflects his mid-career realization of the importance of theory in achieving practical results. He thoroughly enjoyed the time at NASA's now defunct Institute of Computer Applications to Science and Engineering (ICASE), which he believes served an important role by bringing together outstanding scientists and mathematicians and allowing them an opportunity to become better acquainted and to collaborate more extensively. Besides his extensive collaboration with David Gottlieb, Abarbanel worked with a variety of colleagues during his career, including engineers Earll Murman and Ajay Kumar. He discusses well- and ill-posed equations, and distinguishes ill posedness and instability from chaos. Abarbanel, who has taught courses ranging from introductory math lectures to advanced seminars, has come to believe that teaching contributes significantly to research because students pose questions that the researcher would not ask himself. He believes that his training in engineering and aerodynamics gave him an advantage in doing applied mathematics thinks that today's students in numerical analysis can benefit from exposure to the sciences and should receive training in a broad range of mathematical tools. Abarbanel thinks that his work with David Gottlieb demonstrating that linearized Navier-Stokes equations can be symmetrized and highlighting problems of boundary conditions for infinite fields in electromagnetics as among his most significant contributions.

DAVIS
This is an interview with Professor Abarbanel conducted by Davis on July 29, 2003, at the Division of Applied Mathematics at Brown University in Providence, Rhode Island. I would like to begin by doing a little background. Can you tell me where you did your high school work?

ABARBANEL
I did my high school in Tel Aviv, Israel.

DAVIS
Were you interested in mathematics in those days?

ABARBANEL
Of all the subjects it was my favorite one. I wouldn't say that I was interested, in the sense that some people know from very young age that it is the only thing that's going to matter in their lives, but of all the subjects I took it was the one most interesting.

DAVIS
So the interest in mathematics did not occur at a very early age but somewhere in high school.

ABARBANEL
Somewhere in high school.

DAVIS
High school. Then did it grow and take over?

ABARBANEL
It took some time. Actually I started my first two degrees were really in engineering, in college, but even as I was taking engineering I kept noticing that I took more extra work in math and that I liked math courses more than engineering courses, so when I started my doctoral work I sort of shifted more and more towards applied mathematics.

DAVIS
Your college work was at the University of Tel Aviv?

ABARBANEL
No, I was at MIT.

DAVIS
You went to college at MIT?

ABARBANEL

MIT. I got all of my three degrees there. Yes.

<u>DAVIS</u>
In what year did you come to MIT?

<u>ABARBANEL</u>
I came…I was a freshmen in September 1950.

<u>DAVIS</u>
In September 1950.

<u>ABARBANEL</u>
I was out of the army and went to college.

<u>DAVIS</u>
Which army?

<u>ABARBANEL</u>
The Israeli army.

<u>DAVIS</u>
September 1950, actually I was teaching at MIT, I was teaching a section of first year calculus, but also working in one of the defense projects that were at MIT at that time, but I didn't know you.

<u>ABARBANEL</u>
No, very interesting. My section of calculus – one of two in each semester – was the great old algebraist [Adolf] Hurwitz.

<u>DAVIS</u>
Hurwitz? I see, you mean the Hurwitz that did –

<u>ABARBANEL</u>
Criteria –

<u>DAVIS</u>
But he did a book on ordinary differential equations?

<u>ABARBANEL</u>
I don't think it's that Hurwitz. I think it's the Hurwitz from the Hurwitz criteria for the zeros – [1]

<u>DAVIS</u>
Oh, that fellow.

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[1] This is a reference to Hurwitz's paper on zeros of Bessel functions in *Mathematical America*, 1889, v.33.

ABARBANEL
Yes.

DAVIS
Now, restricting your attention to undergraduate, who were your math teachers in those days?

ABARBANEL
Well, we started, of course, from the book by Thomas, but Thomas himself was not – [2]

DAVIS
Thomas wasn't there?

ABARBANEL
No. Well, you know the system there, there were something like thirty sections, and there were no large lecture rooms. The next year when I went to advanced calculus. [Francis Begnaud] Hildebrand was the teacher and he taught us advanced calculus. Franklin, old man Franklin –

DAVIS
Philip Franklin -

ABARBANEL
Philip Franklin, from him I took the first course in differential equations.

DAVIS
You know that Philip Franklin was the brother-in-law of Norbert Wiener. He was married to Norbert Wiener's sister, who I believe had a job as a programmer in the first of the zeroth [0^{th}] generation of computers at Harvard. I used to see her at that time. Moving to your graduate career at MIT, you were totally concentrating on mathematics?

ABARBANEL
Not in my first graduate year. The first graduate year I was still in aeronautics, but the shift started because I had to think of a major and a minor, and I started taking more and more math courses, so my master's degree was still in aeronautics –

DAVIS
There was a wind tunnel there, the Guggenheim, was it called?

ABARBANEL
The Guggenheim building. It was called the Wright Brothers, but also there was a big supersonic tunnel downriver, which was built for the Second World War. I would say

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[2] This probably refers to Henry Thomas, perhaps his popular *Mathematics Made Easy,* first published in 1940.

that my real interest in **serious** mathematics was due to Norman Levinson; he was really my mentor.

DAVIS
You did your thesis under Levinson?

ABARBANEL
He was one of my two supervisors.

DAVIS
What was the subject of your Ph.D. thesis?

ABARBANEL
Well, the title of it was "On some problems in radiative heat transfer." It had two sections, one of which was more pedestrian, if you wish, from mathematical point of view. It had to do with radiation, solving the steady state heat equation, but the input was due to free molecular flow…I will not go into all the details. But then I had the important mathematical section that had to do with unsteady heat equation with nonlinear boundary conditions and that work I did with a lot of –

DAVIS
I will come back to this a little bit later, but let's get back to some of the personalities. What exactly was your relationship with Norman Levinson, that is to say, how did you work with him?

ABARBANEL
Well, it started by my taking a course – the year course – from him in function theory, and I did fairly well, quite well. The next year when I started looking for thesis problems and supervisors and a thesis community and so on. I went to talk to him and to my surprise he was very agreeable and very willing. From then on we would meet maybe once a week and I would tell him what I was doing, [tape glitch…missing text?] I really consider him my first mathematical mentor.

DAVIS
Actually his publications were more in the pure line, but your thesis was applied –

ABARBANEL
That is true, but the part that I did on nonlinear heat equation had things like existence theorems and uniqueness theorems, as well as finding asymptotic solutions.

DAVIS
At this stage of the game you weren't into numerical work with this –

SAUL ABARANEL
No, actually, there was a numerical aspect, which introduced me to computers and showed me that numerical work was important. When I found an asymptotic solution to

this problem with the nonlinear boundary conditions, I felt it would be interesting to see if I could solve it also numerically and compare. It was not that easy because it was a Volterra-type integral equation, and the trouble, as you know, with Volterra is that you've got to start it, for every time,… the whole process. We used the computer, at that time at MIT it was an IBM, I think it was a 1604…

DAVIS
What year was this approximately?

ABARBANEL
Nineteen fifty-eight.

DAVIS
Nineteen fifty-eight. This was after the punch card –

ABARBANEL
No, I more than once almost was driven to tears when I dropped my cards. Actually, the height of frustration was when people told me "you've got put a rubber band around it," so I did, and then one day I dropped it and the rubber band snapped and the cards still were all over the floor.

DAVIS
The computers have moved quite a distance from those days. Who were some of the other mathematicians that you intersected in your graduate work?

ABARBANEL
Well, my graduate math courses…one, there was a course in mathematical physics, which the year that I took it was given actually by a physicist, namely [Herman] Feshbach –

DAVIS
[Philip A.] Morse and Feshbach, the famous book[3] –

ABARBANEL
That's right, basically he taught us that book and –

DAVIS
The second volume probably –

ABARBANEL
Both of them, both volumes, I went two semesters, and I'm trying to remember, I think I took a course again, an advanced course in ordinary differential equations that was with Levinson, and…ah his name will come to me, I took a course which was a potpourri of advanced methods in various [tape glitch…missing text?] problems and so on, Harvey

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[3] Morse, Philip A. and Feshbach, Herman, *Methods of Theoretical Physics, Parts I-II*, 1953, McGraw-Hill, New York.

Greenspan taught the course it was called perturbation methods [tape glitch…missing text?], and a course in which we were taught such things as integral equations [tape glitch…missing text?] and numerical methods by, and I forgot the name of the person who taught us that one.

DAVIS
Did you have any courses with Wiener?

ABARBANEL
No. I've met Wiener, I've interacted with him, "socially" as I met him a few times, but no I did not take any courses [with him]. In those days, when I was a graduate student, his courses were mostly in Tauberian analysis and I didn't take any of his courses. The first time I met him was actually in an elevator. I was, I think, a research assistant. I was going to the faculty club and he heard me speak Hebrew to somebody. So he asked me – he actually said something to me in Hebrew, and I answered him and he then said to me, "You know I speak eighteen languages."

DAVIS
You know he used to buttonhole people in the hall and speak Chinese or something to them.

ABARBANEL
Yes. He bragged to me that he learned Chinese in three months – that's amazing. I was warned not to play chess with him. It was very interesting. He often said to me, "Do you play chess?" and I said yes, and so he said, "Why don't we play sometime," and I said well, [tape glitch…missing text?]…you know the great man, and I was a very young assistant at the time, and somebody who was in the elevator (or maybe it was not an elevator) who heard the conversation, took me aside and said to me, "Don't you dare play chess with him." I asked why, and he said, "Because he's a terrible player and he doesn't know how to lose, he is a sore loser."

DAVIS
That's very interesting, it shows that there are different kinds of mathematical brains, the combinatorial brains of chess are not necessarily Tauberian brains.

ABARBANEL
Exactly. I agree a hundred percent.

DAVIS
Do you remember any fellow students from that particular period of graduate work, fellow students that went on to careers in mathematics and so on, applied mathematics?

ABARBANEL
No. I think…applied math…I certainly lost touch with anybody who was around me at the time. [tape glitch…missing text?] There was one guy, his name was Tobius(?), and for a while I saw his name on publications and so on. You see, a relatively short time

after I got my degree – I stayed at MIT for three years – I went back to Israel and so I lost social contact.

DAVIS
When you went back to Israel, did you get a job at Tel Aviv –

ABARBANEL
Tel Aviv University –

DAVIS
And you have been there all this time?

ABARBANEL
All this time, with exceptions for sabbatical, of course.

DAVIS
What do you see as the relationship between your Ph.D. work and the subsequent work that you carried out?

ABARBANEL
There's very little direct mathematical connection. I would say the connection is psychological because in the thesis for the first time I saw the power of computers, although computers were not very powerful at that time, but I did, as I said, work some very serious –

DAVIS
When did you get into computation in a very serious way?

ABARBANEL
In Tel Aviv.

DAVIS
What computer was available then?

ABARBANEL
Well, at that time there were basically two computers in the country. One was at the Weizmann Institute –

DAVIS
The Golan?

ABARBANEL
No, the WEIZAC, which was run on paper tape, not even punch cards.[4] We got, in Tel Aviv around 1966 maybe, a CDC [Control Data Corporation] machine, 3400 or 3600, I forget which one it was. In those days it was very modern . Another interesting thing

[4] The WEIZAC acronym stood for WEIZmann Automatic Computer.

was that the Army headquarters had a Philco machine, which was probably more advanced than any, and they didn't know really what to do with it in the beginning –

DAVIS
Was this still in the day of tubes?

ABARBANEL
Yes – ah, no, I think the Philco machine might have been the first transistorized, we're talking about 1967, 1968, and –

DAVIS
1957, 1958?

ABARBANEL
No.

DAVIS
1967, 1968.

ABARBANEL
I left MIT in 1964, and that machine in the beginning had a lot of capacity that we didn't know –

DAVIS
The Philco machine –

ABARBANEL
The Philco machine. So they let people from outside the Army use it; it's a little bit like National Laboratory's here letting people –

DAVIS
Sure. This was fairly common in those days and they ran things like a million digits of the number pi and so on, just to keep it going. So what applied problem did you put on in those early days?

ABARBANEL
Well it's very interesting, I did some aerodynamic problems with shock waves and stuff like this. I had a graduate student – I remember him, he died since then as a young man – Zwas, Gideon Zwas. I don't know if you met him –

DAVIS
I don't remember the name.

ABARBANEL
I sent him to take a summer course in Germany, and Peter Lax lectured there, and he got interested in all those weak solutions, and he said to me "I would like to work on that." I

said, “Well fine,” and we studied together some of the papers that were coming out in the mid 1960s –

DAVIS
How do you go from weak solutions to numbers?

ABARBANEL
Well, that was part of the problem, how do you…of course later on people invented switches and the TVD [total variation diminishing] schemes, which Ami [Amiram] Harten did, but in those days basically you were…. I mean, you used something like the Lax-Friedrich algorithm; you smoothed it out.

DAVIS
Would you say that your – you’ve done a lot of work in hydrodynamics and electromagnetics and so on – would you say that that interest already was there as an undergraduate, when you were in aeronautics?

ABARBANEL
Well, not the electromagnetics necessarily. Actually, it’s interesting. I’ll talk about electromagnetics separately because that’s a relatively recent interest. It came not from the application side but more from the mathematical side because I noticed – together with David Gottlieb – we noticed that some of the stuff that people were doing, the formulation was not strongly well posed, which is a mathematical point of view. So we got interested in how to make it more posed. But the hydrodynamics was due to my background in aerodynamics, yes, definitely. And it was a big advantage. First of all, it gave me a physical insight so I knew roughly whether the answers made any sense or not. And it gave me the knowledge; the physical or engineering knowledge of hydrodynamics allowed me also, I think, to choose some problems that made sense.

DAVIS
In your career what has been the balance between theoretical work and computational work?

ABARBANEL
In the beginning it was more computation, in the 1960s and even in the 1970s. Although we were devising algorithms – you know, saying here’s a new fourth order method and so on – the emphasis was on running, even showing that it works. Also, I had some research projects in which we had to produce answers, like what would be the drag coefficient of a certain shape –

DAVIS
This is aerodynamics?

ABARBANEL
This is basically aerodynamics, or what you would call hydrodynamics, whatever. But I would say that towards the late 1970s early 1980s I started getting interested more in the

theoretical side because I realized that people can misuse or not know how to use algorithms properly and so on. I think the one example that told me that theory **is** important – you cannot forget application – was a paper that I did with David Gottlieb. It was started by the fact that somebody did some experiments at Yale on flow past a cylinder at very low Reynolds number, and he got an answer for the frequency of vortices that did not correspond to what people knew up to that point –

DAVIS
Experimentally –

ABARBANEL
Experimentally. Some people did not believe this experiment, but it wasn't that simple because he's a very good experimentalist –

DAVIS
This was Swede Vassar(?)?

ABARBANEL
Swede Vassar(?) yes. And then at NASA some people decided to duplicate it on the computer, you know, simulate his experiment. Basically, what you do is you look at the power spectral frequency of the shape vortices, and what he found was that, besides the main frequency there was another one, which was not commensurate with the main frequency and it was not a subharmonic or superharmonic, and it was there. And there were people who thought that this might have been the precursor to chaos and this sort of thing. When they ran their program at NASA they got the same answer that he did within very few percent, you know, five percent maybe, which is very good. [tape glitch…missing text?] But I have to give them credit to say they were not completely happy because their answer varied a little bit depending on the size of the domain that they used. But the long and the short of it – and it's a long story – it turns out that both the experiment and the computation were wrong, for different reasons. But this is a pretty frightening thought if you think about it: somebody does an experiment, somebody else does a simulation, they agree with each other; well, you know, for most people, they will say –

DAVIS
This sounds…if they're both wrong, there's some sort of a paradox here –

ABARBANEL
No. They were wrong for different reasons, obviously. What it was, it turns out…we did some analysis and we found out that the way you impose the boundary conditions at the far field was –

DAVIS
At infinity –

ABARBANEL

Well, so-called infinity, except on a computer you don't have infinity, so it has to be a finite distance. It's not that it was imposed in the wrong way, but you can be stable and still in a complex plane produce certain frequencies that don't change the amplitude of planes. And those frequencies can be predicted from analyses by the theory by [Heinz-Otto] Kreiss and [Bertil] Gustafsson. And we showed that the way those boundary conditions were imposed would produce that particular frequency that agreed. And that particular frequency, of course, masks the whole field and it happened to be the same. You see if the experiment would have shown a new frequency and the simulation a new frequency but the two frequencies had **not** agreed with each other, you wouldn't have generated such an interest; but because everything agreed they were convinced it was right. So that point, I would say, it was one of those moments when I realized how important the theory is, not just in the broad picture but actually in doing it absolutely correctly.

<u>DAVIS</u>
The numerical methods that you used, what would have been the textbook name for them, did you just call them finite differences?

<u>ABARBANEL</u>
Finite differences.

<u>DAVIS</u>
Finite difference methods. This on the grid, this was two-dimensional?

<u>ABARBANEL</u>
Two-dimensional and three-dimensional, on different grids. Usually it was not the algorithm itself, the basic algorithm, but how do you impose boundary conditions when you have a body shape that does not conform to your grid. Now there are two schools of thought; one school of thought was: use local coordinate systems that matches the body shape and then, of course, the algorithm becomes much more complicated because you have to compute the local Jacobians.

<u>DAVIS</u>
What do they do now that they have these automatic triangulations of odd shapes?

<u>ABARBANEL</u>
Well, this is in finite elements and that…well like most methods has advantages and disadvantages, finite elements is not my own field but in terms of avoiding the difficulties that I mentioned before, finite elements are really great because you can tile anything with triangles.

<u>DAVIS</u>

You used the term, a few moments ago, stability and well-posedness. There's a famous paper of [Richard] Courant, [Kurt-Otto] Friedrichs, and [Hyman] Levy, I think 1928 or something, you must have read this at some point.[5]

ABARBANEL
I think it was in German –

DAVIS
The original one, yeah –

ABARBANEL
But I read the translation, yes.

DAVIS
How influential was that paper?

ABARBANEL
I would say that it was extremely influential because Friedrichs came to this country and was at the Courant Institute (and was at Brown, of course, during the war) and so he remembered that paper. That paper had nothing whatsoever to do with numerical analysis.

DAVIS
It's inconceivable in those days, that they could have–

ABARBANEL
That paper had to do with existence of solutions to the heat equations, and the method that they used was of finite differences. And they saw that…part of the paper is that when you go to the limit you cannot take delta t and delta x approaching zero arbitrarily, that is the way –

DAVIS
They have to be linked –

ABARBANEL
They have to be linked, delta t over delta x squared remains a constant; and that, of course, is the basis –

DAVIS
In some sense it is a remarkable paper, considering how primitive the computational facilities were in the 1920s.

ABARBANEL

[5] Richard Courant, Kurt-Otto Friedrichs, 1928: Uber die partiellen differenzen-gleichungen der mathematischen physik. *Math. Annalen*, v.100, pp.32-74.

No, I keep saying it had nothing whatsoever to do with computations, they didn't do any computations.

DAVIS
Somewhere in there…you don't think that they even contemplated –

ABARBANEL
I don't think they even contemplated…that's my own interpretation. I never asked Friedrichs about that, you know whether they ever…but as an aside, an interesting thing, you know that maybe, about fifteen, twenty years ago, I forget, [Hans] Lewy got the Wolf prize in mathematics.

DAVIS
Is he the Lewy that went to Stanford?

ABARBANEL
No, he was at Berkeley, at the time.[6] He got it basically for two papers. Well, he got it for his work, but…One was the one in the 1950s where he showed that if you have differential equations with analytic coefficients, the solution would not be analytic, which was –

DAVIS
It was a shock.

ABARBANEL
It was a shock, but oh well, you see it's not the previous theories were wrong except that they assumed that the analytic…that the coefficients were real. He showed that if they were complex then the theory does not go. And the paper from the 1920s...

DAVIS
You spent some time at ICASE, haven't you?

ABARBANEL
Yes.

DAVIS
For the sake of the translator here, ICASE, which is a think tank of NASA [National Aeronautics and Space Administration], something like that, would you say?

ABARBANEL
Well, first of all, ICASE does not exist anymore –

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[6] Lewy did spend a short time in Stanford in the early 1950s. Source: 1988, University of California, In Memorium, http://dynaweb.oac.cdlib.org:8088/dynaweb/uchist/public/inmemoriam/inmemoriam1988/1740, accessed: 19 Jan. 2005.

DAVIS
It doesn't exist anymore.

ABARBANEL
It died about a year ago.[7] ICASE – the acronym stands for Institute for Computer Applications in Science and Engineering – ICASE was what I call an institute and laboratory, "owned," by a consortium of universities, called USRA, University Space Research Association, which is like a sister organization to URA, Universities Research Association, which maintains the big accelerators for the Department of Energy [DOE], except that USRA was funded not by DOE but by NASA. And they had laboratories all over NASA bases and other places, and not just in computations; they had institutes having to do with space biology and so on and so forth. But that was the one devoted to numerical methods which are applicable to problems of interest to NASA. I think it was a great institution because mostly they set aside core money to bring people to visit in the summers. I think that I saw many of my colleagues – that I got to know well – I met in them there under circumstances which were much more conducive to exchanging ideas, problems, and so on, than meeting, let's say, at a conference for two or three days or even visiting each other.

DAVIS
Do you find collaboration easy?

ABARBANEL
Yes. Well, let me say it's easier to collaborate with some people than with others. But I like collaboration because I really think that, especially if you have people coming from slightly different directions, you can enrich each other.

DAVIS
Who are some of the people over the years you have collaborated with? I know David Gottlieb –

ABARBANEL
David Gottlieb, of course, I worked a lot with. But it will start with names that are connected with fluid mechanics, let's say. I did some work with Earll Murman, who was in engineering, but he and Julian Cole were the first ones really to use a switch for weak solutions in the early days. [tape glitch…missing text?] …when I was at MIT on sabbatical I worked with them. I have worked some with people from NASA, Ajay Kumar and Ware [Blair?], again they were both in the engineering side and I might have supplied analysis.

DAVIS
In these collaborations was the emphasis on numerical work?

ABARBANEL

[7] It closed December 31, 2002.

The emphasis was on numerical work, but again, not always what I would call production runs but doing something novel, either in an algorithm or analyzing old algorithms and having old myths be put to rest. For example, the ADI methods, alternating direction methods…and the big advantage of those methods were that, being inclusive methods, there were no restrictions, no stability restrictions on the time-step size. That's a big advantage because you don't have to worry about stability; you might have to worry about time-resolution or how accurate it is, if you take the time very long. On the other hand let's say you march towards steady state, which many problems do, you don't care. When it was first invented it was for one dimensional, and you could take and basically the bigger you took the time-step the faster you converged. Well, people assumed that it was true also in two dimensions, and we have shown it's not true. In fact, in two dimensions, it turns out that the speed at which you converge depends on what we call the Courant number – the CFN, the Courant-Friedrichs-Lewy – number. And it turns out that there is an optimal place and this optimal Courant number is, depending on the problem, usually on order of unity, which is what you use in explicit methods. So all of a sudden you realize that implicitness is not as big a deal as you thought it was; it's still a very important property but –

DAVIS
What do you consider, up to this moment, are your main contributions, or the contribution of which you are the proudest?

ABARBANEL
It turns out, I think, almost all of them were done with David Gottlieb. Well, maybe not all, but I'll mention a few. One was, we were the first ones to show that the linearized Navier-Stokes equations can be symmetrized, by that I mean if you write the Navier-Stokes equations as vector equations where the coefficients are matrixes, [tape glitch…missing text?] all of them can be simultaneously symmetrized. Why is this important –

DAVIS
Y wasn't the same?

ABARBANEL
Yes.

DAVIS
Simultaneously –

ABARBANEL
Simultaneously. Why is this important? It's important because then much of the stability theory that was developed by Peter Lax is applicable; otherwise it's not applicable. We had a young colleague, who has since then passed away, [tape glitch…missing text?] Ami Harten, and he used the mathematical idea, but to prove something much more difficult: mainly that the nonlinear case can also be cast in a symmetrical form. That led him to do extremely important work on weak solutions for Navier-Stokes equations, and

PDEs[partial differential equation]. That was one paper. Another paper that I think had some impact is where in electromagentics many of the problems have to do with infinite space, and the question is what boundary conditions do we put far away. There was a method developed by a French engineer named Beringer (?), a very clever idea; he put some kind of a layer at the end. Rather than imposing boundary conditions only on the boundary itself, he put it in a layer. The reason he didn't want to do it on the boundary itself was because if you do it in a standard way, by extrapolating your own characteristics or any other way, the reflection coefficients that you get, they are artificial and they are relatively high. So you are really inside the domain of interest; you are corrupting the solution after a certain finite time. And he wanted to do away with it, and he developed a method – by this layer – and he wrote a new set of equations, which basically he did by taking Maxwell's equations and doing operator splitting on them (it's a technical term, but I won't go into the details).

DAVIS
Is this factorization?

ABARBANEL
No it's not factorization, it's splitting. You take one equation and make two out of it by saying: the operation in the x direction and in the y direction [are so?] independent, if you wish. But in any case, we found out that his method was only weakly well posed rather than strongly well posed, and that can lead to ill posedness under certain perturbations. I think that we did that work about the mid-1990s. About a year or two after this paper came out, I think there were many, many electrical engineers and so on who had to change their programs.

DAVIS
What, speaking generally, is the current status of ill posed problems?

ABARBANEL
I'm not sure I'm the one to talk about it. I think there are – I want to distinguish first between ill-posed problems and ill-posed formulations, numerically, of problems which are well posed. There are some problems which are ill-posed (maybe) by their nature, I'm not sure. Well, I suppose if you write down the heat equation with the wrong sign, you know, with a negative coefficient of conduction, that's an ill-posed problem. But I think it's easy to take a well-posed problem, try to write it in some kind of a numerical model for it, and if you're not careful your numerical model might be either ill posed…in other words, if it's ill posed you'll discover it because when you try to solve it numerically it will blow up. The danger is if it's weakly well-posed, because if it's weakly well-posed sometimes it will run well and sometimes it will blow up and you won't know why, and it will have to do with initial conditions and the length of the [word?] on your computer for that matter.

DAVIS
Very briefly, what are some of the unsolved problems that you personally are interested in?

ABARBANEL
One of them…I can tell you right now I'm interested in, and am working on with somebody in Tel Aviv, is the very old open problem, the Couette flow problem. It's one of those cases where everybody believes that the conjecture is correct: namely that Couette flow is the only example of a completely stable hydrodynamic flow – no matter how large is the Reynolds number it remains stable. I'm interested in this problem because I think – who knows we might be on the wrong track, we think we are making some progress, but who knows – we think that if you understand why this is so, we might get in insight why other problems could lead to turbulent flow.

DAVIS
Speaking more generally, away from your own particular interest, what would you say are the major problems facing numerical analysis in the future, and numerical computation in the future?

ABARBANEL
I think one of the problems is to distinguish between large systems, which could be either PDEs or even ODEs [ordinary differential equations] but for many particles. And the question is, can one distinguish between a chaotic system or a system which is not well-posed, although the two seem to be similar. But in a chaotic system, presumably where you have several domains of where the solution can concentrate in [phase?] space, the solution can jump from one domain to another but the norm of the solution will remain bounded, but you can't always tell. And yet it will still be very, very sensitive to small changes in the initial conditions. This is originally what [Edward] Lorenz found out: that if you change the initial conditions a little bit the solution deviates a lot.

DAVIS
This is chaos?

ABARBANEL
This is chaos, but in a sense this is also true for ill posedness. If the problem is ill posed, one characteristic – besides decides the mathematical definition – one characteristic is if you change initial conditions a little bit, the solutions will diverge. And yet chaos and ill posedness are not exactly the same. So I think as people try to solve more and more complicated systems, especially in material science where you try to do it by solving problems with millions of particles interacting, it will be hard to tell (for them) when they have "final results." Are they due to the fact that the system is chaotic, and then you have to find out what are the domains of attraction? Or is it really that the formulation is really not well posed? And I think that this is one area where not much is known.

DAVIS
I used to think – I don't know if I'm correct or not – but I used to think that chaos was first discovered by numerical analysts in the days before the electronic digital computers, because they used to invent schemes, and sometimes the schemes would converge and

sometimes the schemes would blow up. The schemes that blew up they threw out, and the schemes that converged they kept and they said that's good.

ABARBANEL
Well, if you…Now I'm not sure how to answer that…

DAVIS
It's related to instability effects.

ABARBANEL
Yes, I was just going to say…chaos and instability are not necessarily the same. A system can be chaotic and yet stable, in the sense that the norm of the solution is bounded. But usually, in the non-mathematical way, we expect well-posed problems, or maybe well posed not in the mathematical sense…if you change initial conditions by a little bit or boundary conditions by a little bit, then the solution will change by a little bit.

DAVIS
That's stability.

ABARBANEL
This is stability, but it's not just stability, it's…because the implication of stability…stability…Well maybe, as I've said, there's still confusion…even if you talk just about stability, there's confusion; at least some people are confused. For example, Peter Lax of course put the whole thing on a rational basis when he said, "Look I have this equivalence theory," which says that convergence….Let me step back. If you have a problem which you represent by a numerical model, and the numerical model is what's known as consistent – namely, as delta x and delta t go to zero you recover the original technique – if the model is consistent, then stability implies convergence. And since it's a lot easier to study stability than convergence, that's a huge step. But you can have an algorithm which is stable under this definition, but if you run the problem for longer and longer times the errors can grow exponentially in time. It's still a convergent solution, it's just that in order for it to converge it takes smaller and smaller and smaller time-steps, and then it becomes prohibitive. And now we talk about two different kinds of stability: one is called just the usual stability, and one the Swedish school calls strict stability and we call it temporal stability; but in any case those are two different kinds of stabilities. So people have done that, they write a scheme and they say, "Ah, we tested it and it is stable under the Lax criteria (or whatever criteria) and because it's stable it means it's converged." Then they run it and if it's some problem that has to be run for a long time, they get surprised when it blows up. [laughter]

DAVIS
Well if I get to interview Peter Lax this is something that he will be able to talk about for a long time, I'm sure.

ABARBANEL

I'm sure, yes. Although the people who are really working on the question of strict stability, among other things, is the Swedish school. And by the way this difference between the two kinds of stability appears already in David Mayer's first edition, a book on numerical analysis for PDEs; in his introductory chapter, he actually points out the difference –

DAVIS
This is [Germund] Dahlquist, in Sweden?

ABARBANEL
No, in Sweden it was Heinz Kriess and Gustafsson, Bertil Gustafsson.

DAVIS
Let me change the direction of the discussion quite a bit. You have been a professor at University of Tel Aviv for many years and you've had to do a lot of teaching. How do you see the relationship between teaching and research?

ABARBANEL
I used to think, when I was young and brash, that teaching interfered with research. I'm now convinced that teaching enhances the research.

DAVIS
Could you say a little more about the way it enhances research?

ABARBANEL
I think that if you teach courses – of course it's true maybe more of advanced courses, but even sometimes for introductory courses – especially if you have good students, they will ask you questions that sometimes you wouldn't expect, and you hadn't thought about. Then you find out that this has implications for the kind of work that you do, so I found that teaching makes me think about aspects of the problems that otherwise I wouldn't.

DAVIS
Has your teaching been more or less towards your specialty or have you taught general courses?

ABARBANEL
I taught both. I taught freshman calculus, I taught introductory courses in numerical analysis, I taught introductory courses in PDEs, but I also taught advanced courses in PDEs and advanced courses in perturbation methods and also numerical methods for PDEs. I taught both.

DAVIS
What knowledge or training is now, today, important for a young person to work in computational methods, numerical analysis? What courses, training, and so on?

ABARBANEL

First, let me say it has to go beyond the classical boundaries of mathematics. I'll say something…maybe I'll say it in the beginning; I believe it's very important to have **some** kind of background in some kind of either physical, biological, or some other field to which then numerical methods are relevant. I'm not saying that you cannot – obviously people can work on numerical methods as a pure mathematical game, and that's fine. But I think that if you have an input from outside, it helps in choosing your problems better. In order to prepare somebody to do numerical work and numerical analysis and so on, first of all, I think it's very important to have course like not only linear algebra but what people call numerical linear algebra: all the things having to do with matrixes and so on. I think this is very important. I think that some course on what I would call the classics of numerical analysis, namely, …you know, methods for solving ODEs, old methods before…there are things that came before computers, but still were helpful, like the Picard method for ODEs. You didn't do it on a computer but I think learning it is useful for… Then I think it's important for people who do numerical work to know the non-numerical mathematics underlying the area to which they want to apply it. Let's say…and it's hard to say now, of course, the first fifty years after second World War numerical analysis was aimed at solving problems in continuum mechanics: elasticity, fluid mechanics, electromagnetics, and so on. The future of course could be different; it could be material science, biology, and so on. There are, or there will be, some mathematical formulations for the laws of those new fields; they're not as clear today as the ones for continuum mechanics, and that's one of the problems. But I think you have to teach people the fundamentals of the mathematics of the areas into which they might be going. So if you go maybe, into biology, especially the part having to do with genetics.

<u>DAVIS</u>
How important is classical complex variable to computation?

<u>ABARBANEL</u>
It is not a daily necessity, but I think it's a very important tool. Some formulations are easier put on paper when they are done in complex variables. For example, electromagnetics, plane-wave solutions, that can be written in terms of sines and cosines, but they're much more easily written as in terms of complex exponential. You have to know certain basics of complex variables. I think that complex variables are a basic necessity for any mathematician, whether or not applied or numerics or not, because you learn a lot… Also the idea of squared quantities, namely norms, becomes very natural in complex variables.

<u>DAVIS</u>
I'd see the expression of Lie group methods here and there, do you intersect that at all?

<u>ABARBANEL</u>
I personally don't; I think it's a gap in my education. In general, I think I don't have a sufficiently good background in modern algebra, let's say. There are parts of physics, of course, where Lie groups are important. I have not seen the impact on what I would call

classical scientific computations. It's possible in the future, I don't know enough to comment.

DAVIS
How about the importance of what are called special functions theory?

ABARBANEL
Again it's like many other things: until you need it, you don't realize how important it is. I think it's very important at least to have an educational background so that if the need arises it's not something completely alien; you can go and say, "Oh, this is the book I used to learn," and you go back and –

DAVIS
This is the hypergeometric function and so on.

ABARBANEL
Yes, anything from … polynomials to Gegenbauer; whatever you might need, I think it's important to have it, yes.

DAVIS
This ends the interview, thank you very much Professor ABARBANEL.