A Conversation with Andreas Acrivos

Andreas Acrivos[1] and Eric Shaqfeh[2]

[1]Levich Institute, City College of New York, New York, New York 10031; email: acrivos@sci.ccny.cuny.edu

[2]Department of Chemical Engineering, Stanford University, Stanford, California 94305; email: esgs@stanford.edu

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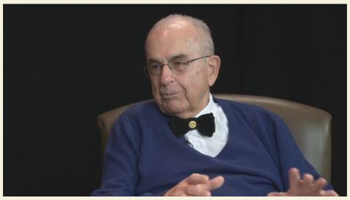

Eric Shaqfeh: Hello. My name is Eric Shaqfeh, and I'm the Lester Levi Carter Professor of Engineering at Stanford. It is my pleasure to be here to interview a person who is a giant in the field of chemical engineering and fluid mechanics. He has been my friend and mentor for 30 years, and originally my PhD advisor, and he's Professor Andreas Acrivos. Welcome, Andy. It's wonderful to be here and talk to you about your life and your impact on chemical engineering and fluid mechanics.

Andreas Acrivos: I look forward to answering your questions.

ES: I thought we'd start more or less in chronological order. Let's talk about your upbringing and how that essentially prepared you or steered you toward engineering and science. I know you were born in Athens, and I know you went through your elementary and secondary school education in Greece. Why don't you talk a little bit about that experience and how you came to be a scientist?

AA: To begin with, I was born with a golden spoon in my mouth (**Figure 1**) to very loving parents (**Figure 2**) and was brought up in a cultural environment. My parents were lovely people who were part of the Athens society, and they were very well established. I had everything I wanted as a kid. I went to grammar school in Psychico, a suburb of Athens, where I grew up. I enrolled in Athens College, which, in spite of its name, is a high school established by Americans in 1925. Athens College had very high standards and excellent teachers, especially in the classics—curiously enough, not so in the physical sciences.

But, primarily, we learned how to study and, specifically, to study on our own. My father was the manager and part owner of a textile factory, and my mother, through her brothers, was related to a very prominent family of shipping brokers throughout the Balkans. So it seemed like my life would have been an easy one for me, given that, perhaps, I was going to inherit my father's business and live the good life. And then the war came, followed by the German occupation, which

Figure 1

Andreas Acrivos in Athens, Greece, 1930.

Figure 2

Acrivos with parents, Athanasios and Anna, in Psychico, Athens, 1960.

of course changed everything. You can't imagine how dreadful the occupation was. Although we did not suffer as much as an awful lot of other people in Europe, they were the kind of years that one would much rather forget.

Now, as far as to what extent my early life steered me toward science and engineering, that's a more complicated question to answer because, although my father was trained as a chemist and worked as a textile engineer, there was never any talk about science in my family. In fact, the whole background of that kind of society I was brought up in was in commerce, so until the war came, I never thought too much about making a living, except perhaps inheriting the business from my father. And then the equation changed.

ES: How did chemical engineering or engineering itself ever come into the picture?

AA: Well, my passion, when I was a teenager, was history. Had I been, let's say, affluent, I think I would have become a dilettante historian or perhaps a professional historian. But I had to think about making a living, and in those days, the best students went into engineering. So, since I was a top student in high school, it had to be engineering, but then, what kind of an engineer?

Well, my father was a chemist. I wanted to be an engineer, so the two pointed me to chemical engineering, even though I had absolutely no idea what chemical engineering was all about. And curiously enough, many of my contemporaries in the States whom I've met after many years chose chemical engineering for the same reason, in that they wanted to study engineering and somebody in their family was a chemist.

And that's how it all came to pass.

ES: Ah, okay. You were essentially finishing high school in Greece, and I know you were an undergraduate at Syracuse here. How did that transition happen, and can you contrast those two experiences? They must have been enormously different.

AA: Well, first of all, why Syracuse?

Well, this was in 1947, and Europe was completely devastated by the war. Greece was in the middle of a civil war. So I had to get out and try to get my education in just about the only place

Figure 3

Acrivos in New York City, 1947.

that offered me an opportunity, and that was the United States. Then, thanks to the intervention of the principal of Athens College, who wrote to a whole bunch of universities in the States, I was offered a scholarship at Syracuse University.

I didn't know a thing about Syracuse University, and I went there together with a classmate of mine from Athens College who had also received a scholarship. We took a converted troop ship with lots of immigrants and ended up first in New York City. We had no relatives in New York or in the States. And then we went to Syracuse to study and, for me, to play some chess in my spare time (**Figure 3**). I'm extremely grateful to Syracuse for taking me on because, without that scholarship, I would have never come here. At the same time, I must say that the education I received at Syracuse at that time left an awful lot to be desired, especially in engineering.

ES: It prepared you well enough, though, to go to a very, very good PhD program. And at some point, you must have impressed people enough with your Syracuse education to be taken on by the University of Minnesota.

AA: Well, first of all, aside from chemistry, where I had some very good chemistry teachers, whatever I learned at Syracuse, I had to learn on my own by reading the books because, unfortunately, the poor teachers were overworked. I'll give you one example. We had this fellow who had his PhD in chemistry. He was an expert in catalysis, and yet here he was teaching courses in fluid—flow, heat transfer, process design, process control, you name it—and the poor fellow just didn't know anything about most of the subjects he was teaching. I can tell you stories about that.

And this was typical. I was a top student and I got all As, except that I got a B in lathe. I couldn't figure out how to work a lathe. And in drawing—I was lousy at drawing. But aside from that, I had a straight-A average. And then when I finished, my plan was always to go back to Greece because there was a lot of pressure from my family to go back.

ES: Of course, yeah.

AA: I was the only son and so on.

But I felt I had not learned enough and that my education was not quite as thorough as I was expecting it to be to allow me to function as an engineer. So I thought I should go to graduate school and just get a master's degree.

I applied to MIT because everybody wanted to go to MIT, and as far as Greece was concerned, there was only one engineering school in the US, and that was MIT.

I applied to Princeton; I applied to Columbia; and in fact, I went and visited these schools at my expense. But they all told me, "Well, we'll accept you, but without financial aid," which meant that I couldn't afford to attend. So then I decided, "Well, let me look at other parts of the country." Eventually I focused my attention on two schools: University of Illinois and University of Minnesota, and I was trying to decide between the two.

ES: You ultimately decided to go to the University of Minnesota.

AA: Yeah. And the question is, "Why did I decide to go to the University of Minnesota?"

ES: I know that, at the time, it was a very, very highly ranked school and there were a couple of extremely good people there, including Neal Amundson, who ultimately became your advisor, but he was in the area of applied mathematics. In fact, he was an icon in the area of applied mathematics, and you didn't really have that background even though you had a good education, as you say, of your own making. So how did that happen?

AA: First of all, your perception of what Minnesota was like in 1950 is totally off the wall, I'm sorry to say.

ES: That's fine.

AA: To begin with, Amundson, who was 35 at that time, was totally unknown.

ES: Oh!

AA: Also, although the University of Minnesota was respected, it was not viewed as belonging to the very first rank, while the chemical engineering department was, let us say, unranked.

ES: Hmm. Completely different from what it is today.

AA: Yeah, that's right.

ES: Absolutely.

AA: So it was a total unknown.

ES: Wow.

AA: I was trying to decide between Illinois and Minnesota. And you have to remember that my main objective was to get a master's degree and go back to Greece. I looked at the catalog and the kind of courses I would be expected to take. And these courses looked about the same in both places: advanced distillation, advanced mass transfer, advanced fluid flow, and that kind of stuff. But there was a two-quarter sequence of courses in Minnesota that really appealed to me called *Plant Design*. There was a beautiful description of these two courses involving the principles of designing a plant, how to take economic factors into consideration, and so on and so forth. I was thinking, you know, "That's what I need."

Figure 4

Acrivos at the University of Minnesota, 1954.

ES: Well, that's what you need for—

AA: For Greece.

ES: —for Greece.

AA: So I chose Minnesota and went there in the fall of 1950. The first big surprise for me was a new building (**Figure 4**), which I didn't expect. Like all chemical engineering departments in those days, chemical engineering at Minnesota had started in the basement of the old chemistry building, but in 1950, they had just opened up a brand new building of its own. The next big surprise was meeting Amundson, who impressed me immensely at first glance—as a person, I mean; you could see that this was somebody who was going to go ahead. He had some of that magic in him. And he was the advisor of all the incoming graduate students.

ES: Oh, okay. Was there a partition between master's students and PhD students?

AA: No, no.

ES: Or were they just graduate students?

AA: They were all graduate students.

ES: Your idea of getting a master's got you lumped in with other people who wanted to get PhDs, etc.

AA: Yes, but most, if not a good part, of us were master's students, and we were supported by teaching assistantships because there was no research money to support graduate work.

So the third surprise on arriving in Minnesota was when I talked to Amundson about my program. He said, "Well, you know what most people do is they decide on a minor, and if you want. . . ." I said, "I want chemistry and physical chemistry." "Well, in that case you should take these courses in physical chemistry, and then you should take these courses in chemical engineering." I said, "And how about, you know, this plant design course?" And he said, "What's that?" "Well, it's a course printed in the catalog." He said, "This silly course? My god, we haven't offered that course for ages. Good thing you told me about it and I'll make sure we—"

ES: That it gets out of the catalog; there you go.

AA: "—cross it off the catalog." So, you see, I was choosing Minnesota for the wrong reasons. Making the best decision of my life, but for the wrong reasons.

ES: This is a remarkable—

AA: Yeah.

ES: You go there with the idea of getting your master's and then going back to Greece.

AA: Yeah.

ES: You went there with the idea of taking these plant design courses because then you would need that to be essentially a practicing chemical engineer—

AA: That's right.

ES: —back in Greece. And really none of that happened because what you ultimately did was actually get your PhD and go into a teaching career in the United States. So how did that happen in your case?

AA: Well, first of all, I had a wonderful experience as a graduate student with superb courses that I took, both with Amundson, who was a fantastic teacher, and also in physical chemistry. We had some excellent teachers there, and I learned a tremendous amount. I really enjoyed being a graduate student. I said to myself, "This is a lot of fun. Why stop at the master's degree?" And, of course, my teachers in Minnesota were very happy to see me continue, and I was given a thesis on the hot topic of the day: distillation.

But my assignment required me to solve a very tough mathematical problem. And you know what? I still read my thesis sometimes, and I should give it to you because you're going to enjoy reading it. It is really a beautiful thesis. And I had to invent a new transform in order to solve the equations that described distillation in the system I was studying. It was for solving a difference-integral equation that nobody had ever looked at before, and I had to invent this transform. It was great. I got my PhD there. I finished in three and a half years, and I still didn't feel quite prepared to go back to Greece. So I started looking around for something to do, and in those days—this was 1954—foreign students just could not get a job in industry at all.

ES: In the United States.

AA: In the United States.

ES: Right.

AA: I started looking for a teaching position, and Amundson (**Figure 5**) would go around telling everybody that he had this great student and would they hire him? And he talked to Charlie Wilke, who was the head of the chemical engineering division—it was not a department as yet—at the University of California at Berkeley. I was offered a temporary position as an instructor for three quarters to teach applied math.

ES: Okay. So Neal Amundson essentially shopped you around as a professor hire, as a teaching hire.

AA: Yes.

ES: And you were amenable to that.

Figure 5

University of Minnesota Honorary PhD. (*from left to right*) Neal and Shirley Amundson with Jennie and Andreas Acrivos, 2000.

AA: Yeah.

ES: You wanted to do that because you weren't ready to go back to Greece at that point.

AA: That's right. Yes.

ES: And the place you landed then was a temporary teaching position at Berkeley.

AA: Not only that, but I still have the letter offering me that position from Charlie Wilke, saying that under no circumstances were they going to extend that appointment. I was always getting a big kick out of showing this letter to Charlie.

ES: Were you also expected to do research even though you only had a temporary teaching appointment?

AA: No.

ES: In that case, I suspect that you were asked to teach quite a lot given that, generally, these are teaching appointments.

AA: No, and this was the nice thing about that appointment.

On one hand, it was a very cheap appointment because it paid very little. In fact, the salaries for professors in those days were really pitiful.

ES: Really?

AA: The big money was made in industry.

ES: Right.

AA: But that was closed to me because I couldn't go into industry on account of the fact that I was still on a student visa. This temporary teaching appointment was the only job I had and no other offers. They gave me this job and I taught—I think it must have been two courses a term. I wrote

Figure 6

Acrivos with Charles Wilke at University of California, Berkeley, 1963.

the papers based on my thesis, and I did a little bit of research on my own, but I had no graduate students and it was a wonderful appointment.

ES: It was a wonderful appointment because I presume it was extended beyond the original—

AA: Oh, yes; after about a semester they decided they'd better keep me.

ES: That is great!

AA: You see, one problem with Berkeley in those days was, first of all, that there were two chemical engineering programs at Berkeley: one in the College of Chemistry, where it is now, and one in the College of Engineering, which was called Chemical Engineering Practice.

ES: I see.

AA: And the university had to decide which one to keep and which one to let go. So I joined a department that might have been abolished within the next couple of months.

ES: I see.

AA: Eventually a decision was made to keep the chemical engineering program in the College of Chemistry because it was obviously much better than the other one in the College of Engineering. And then the university decided to expand it further. The other great thing about this appointment was that I had wonderful colleagues. I mean, you cannot imagine, as I look back, how extremely supportive the five original members of that department were toward the younger people.

It was Charlie Wilke (**Figure 6**) who was the chair and intellectual leader, plus Ted Vermeulen, Charlie Tobias, Don Hanson, and Leroy Bromley. All of them were always extremely solicitous and tried to help us in any way they could. When I say "us," it was Gene Petersen, who had preceded me by one semester, and John Prausnitz, who came a year and a half later. So it was a very good environment where everybody helped everybody else, where all of us were trying to take, if you like, an unrated department and build it up to one of the very first ones on an international scale. And everybody worked together. It was a wonderful experience.

ES: That's absolutely wonderful; to have that kind of senior support for a junior person is invaluable in some sense. Now, we've forgotten about a very important thing, and I'll get in trouble if we

Figure 7
Andreas and Jennie Acrivos, Cuba, 1956.

continue to do this. At some point here, I think in graduate school, you actually met your future wife, Jennie.

AA: Yes.

ES: Was she with you in Berkeley at that time?

AA: No. I came to Berkeley in 1954, and she was still a graduate student in chemistry until 1956, when she received her PhD from the University of Minnesota. And then we got married in Cuba (**Figure 7**), where she is from, and after that she came to Berkeley.

ES: Ah, I see. So you're in Berkeley; you're now on an appointment that is, in some sense, extended. You're doing some research. Do you start to have graduate students?

AA: Yes, and the next thing I had to do is to make a decision as to what I would do for research. And the strange thing is that I never really formally studied mathematics.

ES: That *is* strange!

AA: In fact, the only formal math course I took was freshman math, specifically, differential and integral calculus, and I learned all the other math on my own and by taking Amundson's courses. For the first few years of my career, what I was doing for research is what Amundson was doing in a sense, in that I was using various mathematical techniques that I had acquired to solve various applied problems of interest to chemical engineers.

For example, I wrote, essentially all by myself, an article on the application of matrix mathematics to a variety of chemical engineering problems. It was a nice paper, very original because nobody had thought of doing that before except for Amundson, a coauthor of that paper, who had used matrix analysis to model binary distillation. But I came to realize after awhile that this approach to doing research has its limitations—I mean, just taking one problem here and solving it and then going on to another problem. I mean, I had to find a field.

I looked around for a field, and I had a friend, Tom Baron, who was a researcher and an upcoming administrator at the Shell Development Company in Emeryville and who advised me to go into fluid mechanics. He said, "You're good at math; fluid mechanics is an active and interesting field, and since none of your colleagues know anything about fluid mechanics this should be a good opportunity for you." The strange thing about this advice was that I didn't know anything about fluid mechanics, either!

ES: I mean, you hadn't taken even a single graduate course in fluid mechanics?

AA: I hadn't taken even a single undergraduate course in fluid mechanics except for a few lectures on fluid flow out of [the textbook by Walter L.] Badger and [Warren L.] McCabe. I don't know if that book means anything to you.

ES: No, I haven't actually seen the book.

AA: Okay. I will give it to you so you can see what is in there because this will come as a revelation!

ES: Okay.

AA: So I decided on what I was going to do for research, but first I had to learn fluid mechanics! How do you learn fluid mechanics? Well, by teaching it. I got three graduate students to volunteer to take a special course that I taught as an overload with the title "Special Topics in Fluid Mechanics." I then bought a book and started reading it and team teaching with the blind leading the blind.

ES: And how many people were there? Just—

AA: Me and the—

ES: —and the three graduate students.

AA: —and three graduate students. And I took it as an overload.

ES: Right.

AA: So we took [Hermann] Schlichting's book and we started learning fluid mechanics from that book. And then I also had a friend, Larry Talbot in aeronautical engineering at Berkeley, who had a fluid mechanics background, and every time I had some questions or didn't understand some things in Schlichting, I would go to Larry and he would explain things to me.

So that's really very ironic. Here I am starting my career teaching applied math, having had no formal training in mathematics—

ES: And ultimately—

AA: —teaching fluid mechanics with no training whatsoever in fluid mechanics.

ES: Amazing!

AA: These things could never happen today.

ES: No. Now, the other aspect of your career, which is unique, I believe, is not just that you moved into the field of fluid mechanics, in which you had no formal training, but that you're probably most famous for your mentoring of students. You're at Berkeley; where did you get your abilities, or how did you develop your abilities to teach and mentor students? Because that's certainly one of your strongest aspects? Did you accomplish all this yourself? Or did you have very good examples—did you look at Neal Amundson, for example, and learn how to mentor from that point of view?

Figure 8

Acrivos with new Mercedes, Pembroke Street, Cambridge, 1960.

AA: Without the slightest doubt, Neal Amundson was an excellent mentor and role model. And he had the same ability. If you look at his career, you'll find that it is very similar to mine, and vice versa.

He had this ability to draw in a certain type of student who is ambitious and who is not afraid to take on challenges.

ES: Yeah. It seems like a real secret that you also have. I mean, there's this incredibly strong group of students that you've had and they've been very attracted to your mentoring, and I think that's something that you've developed and you're unique at it. So it seems interesting.

AA: Well, the other person who, I think, is a close parallel to that was George Batchelor.

ES: Of course.

AA: You see, I spent my first sabbatical in Cambridge in 1960 (**Figure 8**). The reason I went to Cambridge is, again, that Amundson had been there for his sabbatical in 1954.

He told me that it was a great place and that he loved it and that he had met some outstanding people. So I decided to go there. On that first sabbatical I had an office in the chemical engineering department, but I met George Batchelor because, at that time, he was heading what was called a fluids unit—it wasn't even a department—in the old Cavendish laboratory on Free School Lane. That unit was housed in either two or three rooms, the total acreage of which was no bigger than the room we are sitting in here. It was so crowded that, essentially, everybody was on top of everybody else. And two things impressed me immensely: first of all, meeting Batchelor. He was only eight years older than I, but he had already established an international reputation. And he was like Amundson in the sense that he had the magnetism, which attracted the top students who wanted to get ahead.

The other thing that impressed me was that they had a very small library that contained the theses of all the people who had received their PhDs in this fluids unit. There were about a dozen theses, and when I looked at the names, I was struck by the fact that I either knew or had heard of everyone. The list started with George Batchelor and Alan Townsend and then Philip Saffman, Fritz Ursell, Anthony Pearson, Brooke Benjamin, Ian Proudman, Owen Philipps, etc.

ES: All enormous names in fluid mechanics.

AA: And I said, you know, "This is really what education is all about."

ES: And this started a relationship that you continued for years and years.

AA: Indeed.

ES: And going to Cambridge, sending students to Cambridge, etc.—

AA: Not only that, but it meant a great deal to me professionally that I, a visitor, was accepted to the club. You see, in fluid mechanics, I was an outsider, and one of the problems for an outsider is being accepted by the leaders of the field even though you don't have the credentials. This is something that I had a great deal of trouble accomplishing in the States. Everybody looked at me and said, "What's this chemical engineer doing in fluid mechanics? What does he know?" Because, obviously, I didn't know much when I started out.

ES: But soon, it was completely different, right? Soon in fluid mechanics—

AA: Yes, it was different, but it took awhile.

ES: Yeah, it took awhile.

AA: But Batchelor—he accepted me right away. I still remember how elated I was to be able to publish my first paper in *JFM* [*Journal of Fluid Mechanics*] in 1962.

ES: 1962.

AA: I felt that I had made it. 1962 was a very good year because, in addition to the *JFM* paper, I published this paper with Tom Taylor, my graduate student from Berkeley, on heat and mass transfer past a sphere that you know about it.

These papers really established my credentials.

ES: Now, sometime in this period of time—you have this relationship with Cambridge, you have been established at Berkeley, and then, so the story goes—the people at Stanford, in particular Dave Mason, come to you in late 1961 and say, "We are going to try an experiment at Stanford by establishing a first-rate chemical engineering department as part of transforming Stanford into a world-class university." In this conversation, somehow [Mason] convinces you and, a couple of years later, Michel Boudart to come to Stanford and join what was then an unrated department.

How did that happen? Because, obviously, you were and should have been very happy at Berkeley. After all, they had set you up, and you had begun to have an international reputation, at least in the area of fluid mechanics as well as broadly within the chemical engineering circles, which was quite a difficult thing to do without formal training. Why don't you talk about that transition?

AA: Of course, it was a very difficult decision. What led me to that? Well, there were lots of factors, one of them being that the department at Berkeley, as I said, was started by five people, who were the leaders. I respected their support, but eventually there comes a time when you say, "Well, I want to do something that is going to be different." And that was hard to do it in that environment because the people who controlled the department wanted it to remain in a certain direction, whereas I wanted to go in a different direction. So what do you do? I would never have dreamed of trying to throw them out because I respected them very much and they had been so supportive of me. I had this ambition that I wanted to create a department, which was in the image of what Minnesota had become or was beginning to become during that time, as well as what the fluid mechanics program at Cambridge had become, namely a small but very high class science-oriented department, in chemical engineering and fluid mechanics.

ES: So this is what you imagined Stanford was going to be like?

AA: That's right. And Stanford was starting from ground zero.

ES: Right.

AA: In addition, there were some other constraints. As you know, Jennie, my wife, had a PhD in chemistry, but in those days, getting a full time position in chemistry for women was completely out of the question. So she had temporary appointments. First she had a postdoc position at Stanford.

ES: Oh, I didn't know that.

AA: She used to commute from Berkeley.

ES: Whom did she work with at Stanford?

AA: In the applied physics department, the Hansen lab, with somebody by the name of Scott Blois, who himself had a temporary appointment. Eventually she accepted a postdoc position in what is now called the Lawrence Radiation Laboratory in Berkeley, where she worked with Kenneth Pitzer in chemistry. And although that was a very fine arrangement, this was still a temporary position, and she always felt that she was hired and kept because of me. Besides, in early 1962, Pitzer had left Berkeley to become president of Rice University.

ES: Oh, I see.

AA: And that was uncomfortable.

So the possibility arose at San Jose State for a bona fide faculty position with the possibility of doing some research, although they only had master's students there. She applied there and was offered a tenure track assistant professorship in chemistry. So that meant that we had to move out of Berkeley, as far as living there was concerned.

So, the question arose as to: Where we should live? In the East Bay or on the other side? We visited Stanford, and we saw the place where we could have a home on the Stanford campus, and [it was] only a half hour's drive to San Jose State, which was an inducement. But I think the main consideration was that this was an opportunity to develop something unique from scratch.

ES: In other words, what really induced you to make the move was that it was exciting and new and different. So, why don't you now talk about that, because Stanford's chemical engineering [department] then changed dramatically—you were there for 25 years or more. Talk about how that happened, and then how it transformed after that.

AA: Well, what happened is that when I came in, the department consisted of four faculty, and I was the fifth. A year later we hired Doug Wilde, and a year after that, we hired Michel Boudart, and then Bob Madix. In the meantime, two of the original faculty had left. One went to industry, and the other one did not get tenure and so he left. So, with six faculty, it was, again, like the original Berkeley department, small, high quality, and extremely congenial in that everybody was working together. We were helping one another and we were interested in each other's research. We also had an excellent seminar program that everybody attended, all the faculty and all the graduate students.

ES: In other words, it was, again, a great scholarly learning environment.

AA: Yes, that's right, and we started attracting some really top PhD graduate students. When the first NRC [National Research Council] Report came out with the rankings of the PhD programs, our small department, which was totally unknown at the time that I came in 1962, was ranked either fourth or fifth.

ES: Wow, that's amazing. So, essentially what, in 1962, you imagined happening at Stanford did happen.

AA: Indeed, it did happen.

ES: And then how did it change? You actually left, ultimately, 25 years later, to go to City College. Now, those aren't necessarily related, but let's talk about the changes at Stanford.

AA: Well, what had happened at Stanford was precisely what had happened to Berkeley years earlier. You see, when I arrived in 1954, Berkeley was an ideal place as far as the campus as a whole was concerned because the lines of communication with the administration were very short, in that all you had to do to get things done was to convince one administrator that what you were proposing to do was appropriate. But by the time I had left Berkeley in 1962, the place had already grown and had become very bureaucratic.

The lines of communication had become longer and more convoluted; you had to go through committees with your requests etc., and unfortunately, the same thing happened at Stanford.

ES: I see. Stanford grew enormously in this period.

AA: People don't realize that in 1962, when I came, Stanford was a dirt poor place from the financial point of view, and we really had to scrounge to balance our books in the department in that any donation meant something to us, with a donation of $1,000 being viewed as a very major gift.

ES: Yeah, it was a big deal then.

AA: Now, eventually in the 1980s Stanford had grown; it was becoming rich, and the influence of individual faculty members was beginning to be replaced by that of committees and all kinds of deans, assistant deans, associate deans, you name it. So things just were not the same, and I was no longer having the fun I had had years earlier. By 1987, I was getting close to being 60 years old; people expected me to retire in a few years, and I was feeling sort of marginalized.

So it was time to look for a different challenge.

ES: I see. Well, before we go to the different challenge, though, let's talk a little bit about your students. This must have been an enormous joy for you because—I mean, I know them because I'm one of them. We're like a family, an academic family. We know one another, we know one another's work throughout the years.

AA: And you also know some of my Berkeley students.

ES: So talk about your students a little bit. I mean, how do you feel about them?

AA: Well, I'm extremely fond of all of my students, and some of them have reciprocated my fondness, while others have not. But that's to be expected.

ES: We have events on a regular basis for you, and everybody shows up, it seems to me, with enormous enthusiasm.

Figure 9

Acrivos's sixtieth birthday celebration, Pajaro Dunes, Monterey Bay, June 13, 1988. (*first row*) J. Goddard, D. Barthes-Biesel, M. Shah, N. Amundson, A. Acrivos, F. Shair, and A. Grove. (*second row*) F. Pan, T. Lo, J. Brady, R. Anderson, A. Sangani, E. Shaqfeh, J. Klemp, and G. Leal. (*third row*) E. Herbolzheimer, F. Milos, and W. Russel. (*standing*) A. Borhan, N. Frankel, E. Chang, G. Youngren, D. Leighton, R. Davis, J. Higdon, F.M. Orr, D. Jeffrey, and A. Nir.

AA: These are very special events (**Figure 9**) and make me feel extremely lucky in having attracted these types of students. Of course, not everybody was attracted to me in that there were lots of students who would run away as soon as I showed up. But that's the way it is.

ES: Now, when you started as a graduate student, and especially when you started teaching at Berkeley and then in the early years at Stanford, you introduced a lot of these applied mathematical techniques to chemical engineering because people weren't using them specifically to solve problems in chemical engineering or fluid mechanics. For example, you talked about your paper with Tom Taylor, about using the method of asymptotic expansions to solve flow problems past spheres. People weren't doing that, and that was one of your big research contributions. How has that aspect of chemical engineering involving applied mathematics and its applications changed over your career?

AA: What has changed now is the availability of computers having extraordinary power that was unimaginable even a few years ago. Specifically, if, in the 1960s, you wanted to calculate anything of any kind of complexity, you were stuck because the kind of computers then available were downright pitiful. I mean, you have no idea how primitive things were in those days. If you chose any kind of halfway respectable fluid mechanical problem and wanted to solve it, like the deformation of a drop in a shear flow, you had to do analysis. And to do analysis, you had to solve nonlinear equations, which you couldn't solve unless you used asymptotics or restricted yourself to special cases.

So, in those days, if you encountered an equation that you could not solve exactly, you used asymptotics to examine special cases. For example, if the equation had a parameter that could take on arbitrary values, you used asymptotics to construct a solution for either asymptotically large or small values of that parameter, which you could then combine to get an idea of what the solution looked like when the parameter in question was neither very large nor very small. But to do that, you had to use a lot of physical intuition to try and figure out the kind of approximations you had to make in the mathematical description of the problem in order to come up with a solution that is useful. You cannot do this through calculations alone.

Nowadays, with the way computers are evolving, as you know, it's easier to do calculations. There is, of course, a good side to it, and there's a bad side to it. You can do the calculations and thereby get an answer to a specific question that you are interested in in a very short time. And you get an answer to infinite "precision," but whether the answer is correct or not given the uncertainty of the underlying equation that you have solved—that's another story. But you can get an answer.

This is the good part. The bad part is that you get all these answers, but all of them are for a very, very specific set of conditions because you have to put numbers in the coefficients of the equation for the particular problem you are trying to solve. In your rush to calculate these special cases, you don't devote as much attention to trying to unify these solutions to these special cases in order to get a global picture of what's going on. Unfortunately, the ability to analyze and do asymptotic expansions has not evolved to the same degree as the ability to compute things, and in fact, it's the other way around. Nowadays, there are very few people who do any kind of asymptotics, and that's a shame.

ES: Yeah. And with that, I know you feel this, or at least have expressed it to me, that perhaps people's physical intuition isn't nearly as developed as the ability to compute. In the process of doing that analysis, you develop a physical feel for the phenomenon that you are trying to understand or to model.

AA: If you do not have this physical intuition, you cannot, just using mathematical techniques, predict how the physical process will evolve when I do this or that. A great master of this was G.I. [Geoffrey Ingram] Taylor, of course, of whom it was being said that he knew the answer he was going to get and he used the mathematics just to make it look respectable.

ES: Many of his papers read like that, actually, in that he basically puts in a few mathematical steps to go to the answer that he knew was the answer to begin with. But, on the other hand, you were one of the first people in chemical engineering to actually use the computer in fluid mechanics to solve fluid mechanical problems.

AA: That's right.

ES: So you did find value in using the computer all throughout your career.

AA: Of course. But I did both. You see, I solved the problems that I could do using asymptotics, and [for] those that I could not do, I used numerical techniques and always tried to blend in the results with the ones that I got using asymptotic analysis. This is what's missing today.

ES: If you look at your students, I think they still do that because even though, obviously, most of your students are involved in large computations of things, they're still using asymptotics to develop physical intuition and to try to unify the results of the computations. This is an art that is somewhat lost in chemical engineering, but it's still being taught by your students. So hopefully, you might have a renaissance with microfluidics and microhydrodynamics.

AA: Yeah, right.

Figure 10

Acrivos at the Bosphorus Strait, 2007.

ES: Anyway, let's talk about your next challenge. You were at Stanford for 25 years. You were a few months short of 60 years old, and you decided that you needed another challenge.

AA: Yeah.

ES: And that other challenge was going to City College of New York as one of New York State's Albert Einstein Professors of Science and as the director of the Levich Institute. Talk a little bit about that.

AA: Let's do so. To begin with, City College is a great place, but the Levich Institute, which had been established in 1979 by Benjamin Levich, an internationally known physicist and electrochemist, had not really gotten off the ground. So I went there and developed it, so now, especially thanks to the efforts of my successor Morton Denn, it is known internationally as a great place for doing research in fluid mechanics and the flow of complex fluids.

ES: And you had a wonderful time there. You were there for what? A decade?

AA: Officially, I went there on January 1, 1988 and retired officially twelve and a half years later, in 2000. But I stayed on for several years more, took on students, took on postdocs, so essentially I spent 20 years there. It was a great experience. They get excellent students, and some of these undergraduates are just fantastic at City College. To be sure, some of their undergraduates don't belong at a university, but this is true in most places. Also, some of my colleagues were really wonderful people.

ES: So you're now officially retired?

AA: I'm officially retired (**Figure 10**).

ES: And you're back here at Stanford in the mechanical engineering department, actually.

Figure 11

Acrivos with his Mercedes at the Flow Physics & Computational Engineering Group, Stanford University, 2011.

AA: It's a great group under Parviz Moin, who, as you know, heads the Center for Turbulence Research and the Flow Physics and Computational Engineering Group. It's, again, a relatively small (in terms of number of faculty) and very congenial internationally famous group where everybody is nice to me as well as everybody else (**Figure 11**).

ES: Now, we see one another quite often, and I was struck by one of the things you told me just a few weeks ago. Specifically, you told me that, in your present role, young people come to you and ask you for advice about their careers—what steps to take, what decisions to take, etc. And you said to me, and this is really interesting, that maybe you're not the best person to offer this kind of advice.

AA: That's right.

ES: Why don't you elaborate on this point?

AA: Well, to begin with, I belong in a different generation; in fact, I belong to the last century. So, I don't believe that the formula I used in being successful would necessarily prove successful in today's environment. As you know, I was free to look around and choose research problems to study because I found them fascinating from a fundamental point of view. And I could always get support to work on the things that I wanted to work on. And there was never any pressure on me to have lots of students. In fact, at any given time, I never had more than six students in my research group plus, at most, one postdoc.

ES: That's remarkable, considering today's climate.

AA: Yeah.

ES: As you know, six is now considered a very small group, actually.

AA: I know. Now, in a research university, the pressure on faculty to have big groups with lots of money is immense. Consequently, faculty are often forced to work on things that, personally, they may not find all that satisfying from an intellectual point of view, just because there's money. So if I were to advise somebody by saying, "Okay, focus on what I did. Make sure you only take the very best graduate students, and if you can't find anybody one year, just don't take anybody, and work only on those problems you are really passionately interested in," this person would end up with a group of three or four people, and in today's environment, this would not get him or her anywhere. I am sorry to say that I don't think many research universities are ready to appreciate somebody who does precisely what I just finished saying.

ES: So what kind of advice do you give young people?

AA: I tell them, "You are grownups, and what you have to do is decide what you want to do out of life, take advantage of your opportunities, and do the best you can on whatever will excite you." Let me add something you did not ask me, although it may have been in one of your original questions, about my legacy. We already talked about my legacy in mentoring students; that, of course, is a major part of my legacy. In research, I also leave a legacy, as you know, in a variety of areas in fluid mechanics, most notably concerning the flow of suspensions where, together with Francis Gadala-Maria and Dave Leighton, we changed the field in a radical way. But there are other aspects to my legacy, which I'm very proud of. As you know, I was the editor of *Physics of Fluids* for about 16 or 17 years, and I'm really very proud of what I was able to accomplish. *Physics of Fluids* was established in 1956, the same year as the *JFM*. It started out as a fluid mechanics journal. Eventually, François Frenkiel, the editor at that time, enlarged the scope of the journal by including papers from the plasma physics community, with the result that, eventually, the fluid mechanics component shrunk down to essentially 5% of the journal. When I became coeditor in late 1981, I was given the task to build that 5% and make it big enough so that the fluid mechanics part could split off from the plasma physics community and become a separate journal.

ES: Yes. I remember you worked very hard at that because I was a graduate student at that time.

AA: Yeah. It was a very, very exciting and fulfilling task to do that, and I was extremely proud of myself for having accomplished this because, when I finished being the editor in 1997, the journal was well on its way to becoming essentially equal for practical purposes, in terms of influence, to the *Journal of Fluid Mechanics*.

ES: Yes. And it's very, very successful today and continues to be very successful.

AA: I'm extremely happy about this. The other part of my legacy is that I've been involved with three chemical engineering departments: Berkeley, Stanford, and City College, where by being present I really had an influence. At Berkeley, of course, the credit goes to Charlie Wilke, for good reason.

As for Stanford, I don't know how much credit I'm given these days.

ES: Oh, I think that you're given enormous credit—

AA: Yeah?

ES: —as one of the founding fathers, and I'm the chair, so every time I represent the department, I represent your legacy within the department as one of the founding fathers.

AA: And at City College, of course, I have had a big impact there, so I am also very proud of that. Finally there is another legacy of mine of which I am also very proud, and this is the following. If you look around, there is now a group of well over 100 chemical engineering professors who are of Greek origin, and they're extremely successful.

ES: Very much so.

AA: What I discovered many years ago is that in some sense I'm partly responsible for this. For one thing, chemical engineering way back in Greece attracted the best students because it was the toughest curriculum.

Those students who were ambitious wanted to study the most difficult subject because they figured out that they were going to learn the most.

ES: I see.

AA: That was their attitude.

There were these great students, and they were looking for something to do once they graduated.

They heard about this kid—he was then the only one—who had gone into teaching in the United States.

ES: And you were a role model for them.

AA: A number of them came to me much later and said, "You know, you had a big influence on me because, when I saw what you had accomplished, I figured out that 'If he can do it, so can I!'"

We meet once a year during the annual AIChE [American Institute of Chemical Engineers] meeting and have a dinner: the Greek chemical engineers. I meet these guys and gals, and it's a great joy to see them—how well the community of these Greek chemical engineers has done.

In fact, I used to say as a joke that no chemical engineering department was worth being taken seriously until it had at least one Greek on its faculty. But it was a joke, of course.

ES: Okay. Well, this has been wonderful. I'm so happy that I've had this chance, and it's been great to talk to you about old times. I learned so much from this as well. I knew most of the last half, but the first half I didn't know very much—specifically about your upbringing, so it was really wonderful [of you] to share this with me.

AA: Good. I am glad you enjoyed the experience, and many thanks for setting this up.

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