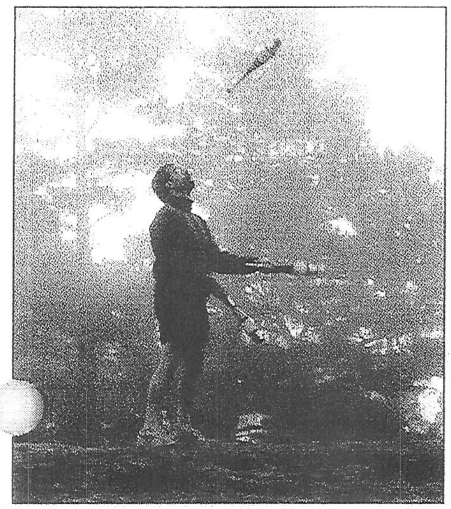
The EMISSARY

JUNE 1998

MATHEMATICAL SCIENCES RESEARCH INSTITUTE

Notes from the Director

David Eisenbud



MSRI is flourishing! This is not a self-congratulatory statement: much of what is happening now was planned by the previous administration, under Bill Thurston, and far from being able to claim credit, I have all I can do just keeping up with what's going on. It's hard even to organize a list because no one continuum seems to hold the different categories. Here are notes on some of what I find exciting.

(continued on page 2)

Inside This Issue

Interview with Jim Simons

On March 5, 1998, Jim Simons, David Eisenbud and Hugo Rossi met in the director's office at MSRI to talk about Simons' several careers. The following are excerpts from that conversation.

DE: I am interested in your transition from an academic career as a Professor and Chair in Mathematics to becoming an entrepreneur in the financial world.

JS: Well, it wasn't as sudden as it appeared. I wasn't always an academic: I had been at the Institute for Defense Analyses as a code cracker in the 60's. After receiving my Ph.D. from Berkeley, I went back to MIT (where I had been an undergraduate) as a Moore Instructor, and then moved the next year to Harvard as an Assistant Professor. After one more year I went to IDA and four years later was fired by General Maxwell Taylor who was then the President of IDA. He had written an article in the New York Times Magazine about how we were doing gloriously and winning in Vietnam. Well, it struck me as stupid, so I wrote a letter to the Times. I wasn't, in fact, involved with Vietnam-related projects, but was doing quite interesting work on high level systems. My letter simply pointed out that not everyone who worked for General Taylor shared his views on the brilliance of our efforts in Vietnam. Although the letter was published, nobody at IDA said a word about it. Six months later I was interviewed by a guy from Newsweek doing a story on government people who had spoken out against Vietnam. He asked me what I was doing about it. So, I said, "I'll tell you what I'm doing. IDA has this policy: 50% mathematics, 50% IDA work. I've elected to do only mathematics until the war is over, then I will do an equal amount of IDA work to make it up." That was my algorithm to deal with this. Then it occurred to me that maybe I better tell my boss, Dick Leibler. So I told Leibler that he should know that I gave this interview. He agreed and then called his boss. Taylor said, "Fire him." So Leibler said, "You're fired." It went straight up and back down again in less than a day - this astounded me.

HR: Then you went to Stony Brook to be Chair?

JS: Yes. I had been lucky enough to prove some pretty good theorems in that period: work on minimal varieties, including the solutions to Plateau's problem

(continued on page 3)

Interview with Jim Simons
(continued from page 1)

and the Bernstein conjecture. I had the credentials. They had been looking hard for five years for a chairman, but they kept striking out. Then they decided to go for a younger guy, and offered me the job, which I thought was good. Having just gotten fired, I thought it would be interesting to be somewhat in charge of my own destiny. I stayed there from '68 to '76 when I left to go into business. I had always had an interest in business. I remember distinctly at the age of 23, in my first job at MIT, sitting in the library thinking, "Well, now I'm on the faculty. Let's see what happens here - I'm an instructor, I become an assistant professor, then an associate professor, then a full professor, then professor emeritus and then I die." Everything was mapped out and I felt too constrained. I loved mathematics - I still love mathematics - but I didn't feel, I think, that I was religiously committed to mathematics - or to anything. I just wanted to be a little more free.

As a student I had talked with some friends from South America about starting a company. In 1958 we rode on Lambretta motor scooters from Boston to Bogotá. I loved the excitement of Bogotá; everything was developing, people were building. You could do anything in those days. There was no competition and you could start a company; it was an open country. So in 1961, by borrowing from my family and others, I invested in a company that my friends had started. At first I thought, "Well, maybe I'll leave mathematics and go to work at it," but my friends had the sense to say, "Maybe Simons ought to stay in Boston, we'll run this business and let him do mathematics," which worked out fine. All through this period I was very active in mathematics and didn't think about business, though I was aware of my investment. I'd ponder it and I'd periodically pray that things would go right. Anyhow, it became quite successful, and in '73 we sold half of it, and then decided that I should invest some of our money on everyone's behalf.

Jeff Cheeger and I knew a mathematician who had started trading commodities and was really doing very well at that time. Cheeger said talk to him, so I did. I felt I could trust a mathematician. We decided to let him manage the money for us, for a percentage. I wanted to be prudent, and said, "If we lose too much, you'll have to stop." And then, as an afterthought, just as I was leaving his apartment, I said, "What if we do well? Maybe we ought to stop also." We decided that if he multiplied the money by a factor of 10, we'd stop and reconsider. Eight months later we stopped on this basis: he had made a fortune - at least it seemed to me a fortune. It was years before I realized that much of the outcome was simply good luck.

HR: At the time you thought it was brilliant?

JS: At the time I thought he was the smartest guy in the world. Years later I realized that you couldn't make ten times your money in 8 months unless you traded outrageously large positions. He was very highly leveraged and a few things moved considerably and he made a fortune. Thank goodness we stopped. He never made money like that again; he'd make some, he'd lose some. But I didn't understand any of that at the time.

At that point in '74, all of a sudden I was independent. It was enough money so that I didn't have to work; it wasn't a fortune, but I didn't have to work. Then I became interested in trading foreign currencies, and started reading about them. I was extremely frustrated with my mathematics. I was working on problems which were very hard (of course, we're supposed to work on hard problems), and I wasn't making much progress. I was still in Stony Brook, had finished the chairmanship, got a divorce and just met the woman who became and still is my wife. It was a time we all go through - sort of a mid life crisis. So I thought, well, maybe I'll try something different. Somewhere between 30 and 40 everyone thinks, why am I doing what I'm doing? Most of us say, keep doing what you're doing, that's really the best thing. But I was going through a lot of turmoil, and my life was changing in many ways. In any event, I thought I would give trading a try. It turned out I was good at it.

(continued on page 4)

Interview with Jim Simons
(continued from page 3)

HR: Did you feel that you had some insights or were you attracted by the adventure or the change?

JS: Both. I did feel that I had some insights. Once again, initially I was pretty lucky. I was smart enough to hire people who could give me different points of view. I've always had good partners in mathematics; for example, Shiing-Shen Chern - I was pretty lucky to collaborate with Cheeger and Chern. I brought in a guy named Leonard Baum who was the best cryptanalyst in the US at that time. Over the next few years Lenny and I did well. Again, I think we were lucky, but we did have some insights to add.

HR: Were the insights in the form of mathematical models?

JS: No, no. Not in the least. When I started the company, in '77 I thought that a mathematical model would be good to build. Lenny was the best mathematical modeler I knew. He had cracked all these codes and he could see what was going on under the pattern of all these 0's and 1's. So I showed Lenny some graphs and said, "Maybe there is some model building you could do," persuading him to come to work with us for - at first - the summer. We started to build a model for currencies that seemed to work all right. But one day, after about three months, Lenny came in to work around noon and said, "I'm tired of doing research - it's time to do some production. Buy the British pound. Margaret Thatcher has been holding it down and there is no way she can hold it much longer. It's going to go through the roof - you better buy it." I said, "I wish you had come in an hour ago; she let it go at the close in London, and it has already gone up 5 cents." He said, "That's nothing, buy those pounds." So I bought some and it did continue to go up. From that day on Lenny stopped modeling. He felt he could predict these markets better than any computer model we were ever going to build. There was no way I could persuade him differently, but that was fine. He stayed with us, and in the subsequent years we made a lot of money.

But always in the back of my mind I thought, well, mathematics ought to be able to do something here. Then in '80 or '81, Jim Ax came to work for us. We showed him the work that Lenny and I had started several years ago. He's a very smart guy and he looked at all the data, and we got a very good programmer, a mathematician named Sandor Straus. Working together, they observed that the behavior we had modeled for currencies is present in many commodities. It hadn't even occurred to me that you could generalize what little we learned about currencies.

We weren't, in my opinion, hiring enough people. I wanted to broaden the base of research. I'm a great believer that no one has a monopoly on great ideas and you want to get younger people. That's when Henry Laufer joined us; he turned out to be absolutely brilliant in this kind of work and loves it. Henry came in and made some very good observations that became the basis of much of what we do today. So, that company, which was originally about 12 people, is now close to 90, of whom more than 40 have Ph.D.'s. We manage a fair amount of money and have done extremely well. And it's all mathematics, essentially mathematics.

HR: What kind of mathematics is involved?

JS: Well, the mathematics is not terrifically deep. A lot of linear algebra, a lot of statistics - Bayesian statistics - fairly sophisticated statistical models in many cases. It's an experimental science. You're given a lot of data and in the end the questions are: is there something going on underneath this data that will produce some statistical anomaly? Where should I look for this anomaly? How should I organize the data so that I might find something that jumps out at me? Some level of mathematical sophistication is required, but also sort of a love - a fascination with trying to know how things work.

DE: There is a whole subject called "Financial Mathematics." Is what you do close to that?

JS: Well obviously, this is mathematics and it is finance. However, we have avoided most of the usual things. In this subject, people are using mathematics to value complicated instruments. We haven't done that. Of course the Black-Scholes approach to option values has been very productive, and has gone through many improvements. It points you to instruments that maybe are underpriced or overpriced, so you should buy one and sell the other and at some future time things converge. But it doesn't tell you when that will be, and in that sense it is not predictive. In our models we're trying to make predictions, on the scale of a day, a week, or a month, but with specific timelines. We analyze a lot of data, build models to characterize those data, and finally run simulations to check their predictive value.

DE: Do you think that computerization has speeded up these processes?

JS: That and telecommunications - the fact that people are able to react faster to news. The time it takes for news to percolate through a market, so that everyone knows it, is obviously shorter today than it was twenty years ago.

DE: Do you see a basic difference in the culture of the academic world and the financial world in which you are now operating?

JS: I operate in a corner of the financial world that is closer to the academic world than normal. In the early days, my business friends would ask me, "How does it feel to be in the real world?" I would say, and I still feel, that mathematics seems much more real to me than business - in the sense that, well, what's the reality in a McDonald's stand? It's here today and gone tomorrow. Now, the integers - that's reality. When you prove a theorem, you've really done something that has a substance to it, to which no business venture can compare for reality.

DE: Was it a big change to go from being a department chair to being a company president?

JS: Well it was, but it didn't happen all at once. Running a company is different from running a department. In a math department you really can't tell people what to do. If you have a full professor, you can't tell him, "Well, work on this." It is interesting that you can guide people in business, but how do you guide research? It is a very interesting question. I think we don't do it nearly as well as we should. Take, for example, the Manhattan project - how did Oppenheimer get all these smart guys all to work together so well on such a short time scale to solve such a hard set of problems? He must have been quite brilliant at it. Of course, everyone felt the urgency. Nobody was saying, "No I don't want to work on this because it won't lead to a paper." People were willing to be directed because they knew they had to coordinate and work together. Nonetheless, it is a challenge in our business to get smart guys working together.

"How does it feel to be in the real world?"

Mathematics seems much more real to me than business.

HR: Do you have some idea how to do that in academic research?

JS: I think the best thing you can do is talk and work with young people, and make sure that they always see good problems. Of course, what we have are groups of people collaborating in areas that need to be attended to. So, we need to assign one or two people to those problems that have come to the surface as being the most important things.

DE: In mathematics it is hard to predict what will be most important. In business, do you feel that it is clear enough?

JS: It is clear enough from a development point of view. You know you can make some money by doing X, Y or Z. And you can tell how much you ought to be able to make with reasonable probability. But those are the nuts and bolts things; they don't tend to be breakthrough things. As in mathematics, nobody can say, look at this and you'll get astounding results. For that, you need the dreaming - the same kind that goes on in science. You need some people who have some time not to be directed in the way we've discussed. We hired a young guy last year, 21 years old, who just got his degree in combinatorics. He had been at IDA and liked this kind of work. He became interested in some things we had looked at some time ago - we didn't point him to it, but he started looking at it. After a while, he showed us a graph that no one had studied very seriously, and pointed out a periodic structure that led to an exceptionally profitable improvement in our approach.

(continued on page 6)

Interview with Jim Simons
(continued from page 5)

DE: Let's go back to the earlier days.

JS: I came to Berkeley originally because of Chern, but that first year he wasn't there. So I started working with Bert Kostant and learned a lot from him. He was my thesis advisor, although Is Singer also helped me quite a bit. I got to know Chern in my second year - I was there only two years. After I left Berkeley I started communicating with him when I started to work on minimal varieties, because I knew this was something that interested him. That is how we started our mathematical dialogue in the late 60's, and continued in that way for the next ten years.

HR: Did you, or Chern, have a feeling that the Chern-Simons paper was going to lead to a lot of further development, in terms of either techniques or ideas?

JS: I was quite pleased with it. When these ideas were first developed, they seemed very natural, and I thought that they might lead to something in Physics. I talked to Frank Yang about it. At that time he was trying to reproduce - without realizing it - a lot of geometry that had already been done. When I realized that, I started giving his group some lectures in geometry, so that he didn't have to reinvent connections and parallel transport, which is what he was doing. He was astounded. Chern said once that Yang had asked, "How did it occur to mathematicians to do all this stuff, all this bundle theory and connection theory?" "Well," Chern said, "it all was natural." I showed Yang our secondary invariants, because I thought they had to have some physical significance. So these recent developments don't surprise me that much.

Later I started working with Jeff Cheeger, moving forward with these invariants. It was easy to work with Jeff because he is smart and he was at Stony Brook, whereas Chern and I were never in the same place together. The kinds of problems Jeff and I turned to were about volumes. When the bundle is flat it is given by a representation of the fundamental group, and the invariants become cohomology classes of the base modulo Z. What you would like to do is to calculate some: can they be irrational? Just as in topology, when you tensor with the rationals, things become much simpler. The same thing is true here, except, instead of dealing with R/Z, you're dealing with R/Q. Now, R/Q is not a group that everyone looks at, although it's a rather nice one. It's large as a vector space over Q. Anyway, I think R/Q is terrific; I like it. For these flat bundles, the question is: are any of these invariants, some of which can be explicitly calculated as sums of volumes of geodesic simplices on spheres, nonzero modulo Q? I wanted to get some irrational ones. Thurston showed me an example. It starts with a manifold whose fundamental domain is a simple figure in hyperbolic 3-space with dihedral angles which are rational multiples of π. He cleverly constructed a representation of this manifold's fundamental group in SO(4), and the key invariant of the associated bundle turns out to be rational equivalent to the volume of a geodesic simplex on S^3 also with dihedral angles which are rational multiples of π. Now, could the volume of such a simplex be an irrational multiple of the volume of S^3? It's a great question. I was fascinated with the fact that these number theoretic questions came in. Of course it turns out that the volumes of certain 3-manifolds of constant negative curvature are also values of these invariants. We must have spent a year on the volumes of these things - I was driving Cheeger crazy. We didn't have the right background and were completely unequipped to answer this question. Maybe if I were corralled with Carl Ludwig Siegel I could do it. Those kinds of things drove me out of mathematics; I was never going to find out if those numbers were rational or irrational.