Robert G. Gallager Wins the 1999 Harvey Prize

Prof. Gallager receiving the Harvey Prize. Left to right: Prof. Adrian Segal, Prof. Israel Bar-David, Prof. Robert Gallager, Prof. Jacob Ziv, and Prof. Abraham Lempel.

The American Society for the Technion-Israel Institute of Technology has named Robert G. Gallager, Professor of Electrical Engineering and Computer Science at the Massachusetts Institute of Technology, the recipient of the1999 Harvey Prize in the field of Science and Technology “in recognition of his fundamental contributions to information theory, and for his contributions to the theory of communication networks.” The prize is one of two given annually, and consists of a cash award of $35,000 and the opportunity to lecture at the Technion. The Harvey Prize, established in 1972 by the late Leo M. Harvey of Los Angeles, honors major contributions to progress in science, technology, and medicine, as well as to advancement of peace in the Middle East. The first winner of this prize was Claude Shannon in 1972. Since 1990, three winners have received the Nobel Prize: Claude Cohen-Tannoudjhi (physics-1997), Pierre Giles de Gennes (physics-1992), and Bert Sakmann (medicine-1992). The prize was presented in Israel on June 16, 1999. At the request of the editor, Dave Forney held the following interview with Bob Gallager in honor of his receipt of the prize.

Forney: Bob, congratulations on winning the Harvey Prize! We were talking a while ago about your early life and education, and I wonder if there was anything that you feel was particularly important along the way?

Gallager: I don’t know how important any of it is, but it might be helpful to new people in the field to understand what a random path I took. When I was young, I had very little intention of becoming a scientist or engineer. When I went to college at the University of Pennsylvania, I went into electrical engineering primarily because I lacked aptitude for languages. EE was the only course I could take that didn’t require foreign languages. I enjoyed the mathematics much more than the engineering, but found that when I was in classes with math students, I tended to think a little more like an engineer than most of them. I kept trying to understand what was going on at an intuitive level. One of the things that I've noticed about myself is that I don't deal easily with the abstractions of pure mathematics, but also I dislike the plethora of detail in many engineering problems. Trying to thread the path between abstraction and messy detail has pretty much defined the path that I have followed. I did well academically as an undergraduate, but didn't take my studies very seriously. After my Bachelor's degree, I went to Bell Labs, because that was where all the action in communication was at the time. I started at the princely salary of $350 a month (not bad at the time). Bell Labs had an internal school called Kelly College for new engineers, which was probably the best place in the world to study communication at the time. I remember being taught by Dave Slepian, John Tukey and Bill Bennett. After a year and a half, I was drafted into the U.S. Army. My unit had people drafted out of the Atomic Energy Commission, Bell Labs, and graduate schools everywhere. It was a bright bunch of people, but unfortunately our officers and non-commissioned officers were only marginally literate.

Forney: What was this army unit?

Gallager: It was called a scientific and professional personnel unit. It was the army's effort to make good use of people who had an engineering or scientific background. It was not so much a communication unit as what is now called C^3I.We worked on something called battlefield surveillance. It was not only communication but also networking and control. It is amazing to me that over the last forty years, there has been so little real progress in that field.

Forney: Were there any personalities that particularly affected you in the army?

Gallager: Well, there was a colonel who had very different views on the way things should be done than I did. I remember that at one point he had all of us out on the field, running around with little slips of paper, which was his idea of how battlefield surveillance would be done. The officers sat in the van and wrote notes, and we peons took the notes and ran from one van to another with them. At one point I wrote to my senator that we weren't being used as scientific professionals. The colonel found out about this and he was very upset, to put it in printable language. He assigned me to stockade guard duty for three months. This was one of the best assignments I ever had, as I had nothing to do and spent the time studying lots of things and thinking through problems. It was a far more academic environment than anything I have experienced since.

Forney: I suppose that is where you first read Claude Shannon?

Gallager: I had heard about Claude Shannon at Bell Labs. In the army, sometimes in the morning I had a terrible hangover and would go to the library for peace and rest. That was where I first started to read Claude Shannon. Before that, Ihadtried to read about information theory as interpreted by others, and had no idea what they were talking about. When I started reading Shannon's own papers, it all seemed so simple.

Forney: You have consulted for the army subsequently, haven't you?

Gallager: I have been involved in a number of committees trying to help the U.S. Army. The army has always been planning for fifteen years ahead, but never thinking very much about the near future. As a result, soldiers in the field still don't have the kind of personal communication devices that we civilians have.

Forney: How did you happen to go to graduate school at MIT?

Gallager: Well, I was planning to go back to Bell Labs, which I had thoroughly enjoyed. But then I learned that draftees could get out of the army three months early by going back to graduate school. I planned to go to graduate school for one term. If I liked it, I would stay for two terms and get a master's degree. I applied to the EE Department at MIT and to the Math Department at Yale. I got a fellowship offer from each, but MIT started one week earlier than Yale did. Since my main objective was to get out of the army as soon as possible, I went to MIT, which has shaped a great deal of my later life.

Forney: That was fortuitous, because MIT at that time was a very exciting place.Howlong did it take you to find the information theory group at MIT?

Gallager: I think it took me about two or three weeks. When I first arrived at MIT, I thought I was interested in switching theory. I talked to Dave Huffman, who was the switching guru at that time. After about ten minutes, he essentially told me that all the action was in information theory. Pretty soon I was talking to Bob Fano, who was suggesting all sorts of crazy things to work on. They weren't actually that crazy, but I was expecting something a little more oriented toward applications. Despite myself, I found myself doing research.

Forney: Could you paint a picture of what MIT was like in those days?

Gallager: It was a very exciting environment. It was just a year after Claude Shannon had come to MIT from Bell Labs. Along with Claude, the faculty interested in information theory at the time included Bob Fano, Peter Elias, Dave Huffman, and Bill Youngblood. Jack Wozencraft was finishing his thesis and about
to join the faculty, and Bill Davenport soon moved to campus from Lincoln Lab. Jack's work on sequential decoding was very hot in that era, and still has a lot to recommend it. There was another large group doing what was called statistical communication, under Norbert Weiner and Y.-W. Lee. That group tended to be more oriented towards continuous mathematics and control. The information theory group was more oriented towards discrete mathematics, algorithms, and the kinds of the things that information theorists still do. It was very exciting because there were many different currents swirling around. The early days of computer science were taking place in the same building. John McCarthy and Marvin Minsky were there, feeling somewhat frustrated that MIT wasn't paying enough attention to the emerging field of computers. The brightest new graduate students were strongly attracted to Information Theory. The group of students who were there then and shortly after includes many of the best-known people in information theory and related fields today. They included Elwyn Berlekamp, Roger Brockett, Dave Forney, Irwin Jacobs, Fred Jelinek, Tom Kailath, Bob Kahn, Len Kleinrock, Jim Massey, Larry Roberts, Harry Van Trees, Jan Willems, and Jacob Ziv.

Forney: How long did it take you to find a thesis topic?

Gallager: It didn't take very long to find a master's thesis topic. Bob Fano suggested a problem, and it was fun playing with it. I don't even remember what it was any more. In retrospect it wasn't very important, but it was good training in doing research. As I said, I wasn't planning a life in research; I was just intending to go back to Bell Labs. However, after a year I was really hooked on MIT and decided to stay for a Ph.D. It was relatively easy to find a Ph.D. problem in information theory since almost nothing was known. One could work on almost anything and it would be new. After looking at Pete Elias' iterative coding schemes for error correction, I got the idea that long block lengths were good for achieving small error probability, but that the parity-check equations should be kept simple to avoid decoding complexity. This led me rather quickly to the idea of low-density parity-check codes. However, it took quite a bit of work before I could see how to analyze them.

Forney: Was Bob Fano still your advisor at this point?

Gallager: No, I switched to Pete Elias since he was closer to the ideas that I was pursuing at that time. He was a wonderful thesis advisor, always willing to listen even though he was Chairman of the Department by the time I turned in the thesis. Even after that he was always interested in what I was working on, and seemed able to understand new ideas almost instantaneously.

Forney: Your work on low-density parity-check codes was more or less forgotten for thirty years, but recently has become one of the hottest topics in coding theory. What would you say about that history?

Gallager: Well, actually there were a few people who kept toying with the idea. Some years later Mike Taylor was trying to construct reliable memories out of unreliable components, and it turned out that low-density parity-check codes were a nice way of getting around any type of failure, whether in memory or in computation. Also, after the Russians translated my LDPC monograph, there was a flurry of activity there. I think the reason that interest died out was that other techniques seemed more promising and were closer to the technology of the day. LDPC coding required block lengths so large that even though hardware requirements went up slowly with block length, they were too intensive for those days. Thirty years later, the technology was available. People working on turbo codes recognized that they were very similar to low-density parity-check codes. That's what led to all this new activity.

Forney: How valuable did you think your thesis was at that time?

Gallager: I think that all of us who do theoretical work jump between thinking our work is wonderful and thinking our work is totally useless. I remember an interview at IBM during which I said that I had developed a coding technique which would allow one to transmit at an arbitrarily small error rate at a data rate arbitrarily close to channel capacity. The interviewer was very offended. He thought that this was really bragging and that my technique wouldn't work in practice because it was too complex, and of course at that time he was right.Onthe other hand I was right too. So while I was happy with what I had done, I also realized that it wasn't something that could be used at that time. As time went on it seemed less likely that it would ever be used, but much later people found that it might in fact be practical. It's sort of the way of many theoretical developments. When we are honest with ourselves, we have to admit that we don't know whether our work is going to be useful or not. Even if it is useful, there are usually so many other further steps by other people that we can't really say how important our idea was in the whole development.

Forney: What did you work on after your thesis?

Gallager: I worked on a strange channel model in which, along with ordinary errors, the channel could also delete and insert symbols. I applied sequential decoding to this channel model, and proved some nice things about decoding with arbitrarily small error probability. I was quite proud of the resulting paper, and submitted it to the 1962 Symposium on Information Theory in London. I got a polite letter back from Colin Cherry saying that they liked my paper but couldn't accept it because there were too many papers from MIT, and all the other papers were by more senior faculty members.

Forney: Then what?

Gallager: I interviewed at a number of places, including Bell Labs because of my fond memories there. I remember clearly talking to Brockway McMillan, who had done good early work in information theory. He told me what some MIT people were saying in the early 1960's, which was that information theory was sort of dead. McMillan told me that I ought to get into military communications, which is what he was doing at that time. I felt that at some level he was right, because after this tremendous spurt of activity at MIT in information theory, we didn't see much happening, because the technology wasn't ready for the complexity that we were thinking of. At the same time, I was still fascinated with the field and wanted to work in it. So when MIT made me an offer, I decided it was an offer I couldn't refuse and thought I would stay there for a few more years. I was very lucky because whereas most young faculty members feel great pressure about getting tenure, I never did because I was never sure whether I wanted to continue to teach. So one day I just found out I had tenure.

Forney: I know that you were involved in the founding of Codex Corporation in 1962, and I would be very eager to hear your recollections.

Gallager: When I was a graduate student, I consulted for a company called Melpar, which was a Washington firm that did mostly defense work. Its research division was in Boston, and the Director of Research was Arthur Kohlenberg. Arthur was a person who most information theorists at that time knew well and respected highly, a wonderful engineer and scientist and a wonderful person. I enjoyed working there. Jacob Ziv consulted there also for a while. The management at Melpar evidently felt that these researchers up in Boston had too much freedom and eventually decided to bring them back under closer control. Arthur and Jim Cryer, the manager of the Boston division, thought that this might be the time to start their own company. They thought that this coding business could be a hot thing for the future, so they wrote up a business plan and started Codex. I was a consultant. I told them about the threshold decoding work that Jim Massey was doing in his thesis. They decided that this would be a great place to get started, so they asked Jim to consult for them also, bought the rights to his invention, and started developing products. We think that today is an unusual time in which people can start Internet companies and have market values of billions of dollars before they make a single dollar. But this happened in the 1960's also. Codex lost money for a long time, but it went public and the stock went up and up. I think the most valuable thing I ever did for Codex was to tell you about them and them about you when you were finishing your thesis. I thought that you would probably want to go into an academic environment, but much to my surprise you found this entrepreneurial seat-of-the-pants operation very intriguing and started to work there. That made consulting for Codex even more enjoyable.

Forney: Did you ever consider leaving academia and going to work for Codex?

Gallager: Yes, I thought about it a number of times. For me Codex was sort of a vacation from MIT, where I could sit and think and nobody would bother me. But then I saw that the people who were actually working there were scrambling constantly, and that their lives were just as chaotic as mine was at MIT. So I decided to stay at MIT, and continue consulting at Codex, which was much more fun.

Forney: I can certainly testify that you did a lot of valuable engineering there. And your interests evolved as Codex's interests evolved. In the 60's you were very heavily involved in coding, and developed some burst-error correcting codes for Codex. At the end of the 60's you were the one who got us on the path to QAM modems, which was probably the most valuable thing you did for Codex. Then later your interests evolved into networking. Was this just pure happenstance?

Gallager: Well, I think my change of interest was partly influenced by Codex and partly by other things. In the late 60's, coding ideas had gotten far ahead of what we could build, while the more mundane parts of communication had somewhat languished. So there was wide interest in how you could actually build better communication systems. I got into QAM because Codex had hired Jerry Holsinger, who had some nice ideas for building 9600 b/s modems. That doesn't sound like much now, but it was a lot then. Jerry was doing this with a single-sideband modem,which was what most people were advocating in those days. I started trying to figure out how these things worked and got so frustrated with Hilbert transforms and the like that I said that there must be an easier way. Why not just build a double-sideband modem? It would be much easier, carrier and timing recovery would be much easier, and we could understand what we were doing. So I spent a couple of years trying to flesh that out. Then Codex went through a rather bad period in 1970 where both its two founders died, the military business was doing poorly, and the modem business wasn't going well either. To survive, the company suddenly had to develop a new modem very quickly. Dave Forney took the project over, decided the best bet was the double-sideband approach, and in an amazingly short period of time developed a viable modem product. The company's fortunes were made for quite a long time.

Forney: Probably your most famous paper is your 1965 exponential error bound paper. Is that your favorite paper?

Gallager: That is certainly my favorite paper. However, I've always liked my books more than my papers because what I have always enjoyed doing is taking ideas and trying to make them simpler. I get frustrated by things that are too complicated. The way that work actually began was that shortly after I was hired, MIT decided to teach a double graduate course in information theory and coding. Part of it was to be on sequential decoding, and part was to explain the theoretical development of the coding theorem and so forth. Bob Fano had just published a book on information theory which had a proof of the coding theorem, and I was going to teach this in this class. But I couldn't find a way of presenting that work because there were a number of optimizations, and I couldn't see why they were maxima instead of minima. I had four or five almost sleepless nights trying to figure out how I would present this stuff. Finally it all just came to me in a rush. This was a good example of teaching and research really fitting together well. Many times, trying to understand something well enough to teach it has given me nice new ideas for research.

Forney: Was there more or less a straight line from this to your 1968 book?

Gallager: There was certainly a straight line to the coding theorem part of the book. The other parts of the book took an awful lot of work. That book was probably five or six years in the making. As I went through it I found that many of the things that I thought I understood really weren't very well sorted out anywhere in the literature. All the things that people thought that they knew about the Gaussian channel seemed to become very vague when one tried to present them precisely. I probably wasted about a year of my life trying to do the Gaussian channel precisely. I'm not sure it was worth it because everyone understood what was going on anyway; it was just a matter of crossing the t's and dotting the i's. I think it was worthwhile, but now when I write papers, I usually say that this is just the way it is.

Forney: Are you ever going to produce a revision of that book?

Gallager: I really don't know. I am currently writing a book on stochastic processes, which is a second edition of an earlier book. I am trying to put in detection and estimation and Gaussian processes and other things. There are some other books and projects that I have in mind. If I ever get those things done, then I will go back to the information theory book and rewrite that.

Forney: We are up to the 1970's. What happened then?

Gallager: Well, networks were starting to become very important at that time. I started focussing on them at MIT, and also it was something that Codex was getting involved in. I first got involved in routing, which is what most academic people first get involved with because it's a very nice analytical optimization problem. Then I got more interested in congestion control, and later in all queuing aspects of networks. After that I got more and more involved in network architecture. Information theory and communication are fields in which theory and practice are probably closer than in any other field of technology that I know of. The network field is probably at the other extreme, where theory and practice have very little to do with each other. The more successful developments are usually totally ad hoc. The Internet protocols were developed by people who thought very well architecturally, but there was almost nothing of an analytical nature in that work. I'm still very curious to see whether we will ever have a networking theory that allows people to understand what's going on in networking in a cleaner way.

Forney: Was it frustrating to try to develop a theory of networking?

Gallager: It was frustrating, but it was also fun. It was fun because nothing was known, so in that sense it was like the early days in information theory. But it was frustrating because one got the sense that no one who built networks really cared. They were going to build networks the way they were going to build networks. In retrospect, I think we are starting to get a better idea of what's going on in networks; the theoretical work that's been done has had some impact, but not nearly as much as one might have hoped.

Forney: Many people have wondered what happened to information theory and communication theory at MIT. What can you say about that?

Gallager: It's a long story. As I said, even in 1960 MIT had started to partly turn away from information theory, and later much more so. In the 1970's, U.S. universities tended to feel that the pendulum had moved too far towards making a science out of engineering. There was a feeling that engineers should go back to being engineers. They should use insight more and mathematics less. They should use simulation and experiments more and should think less. It was a great change, which I believe is still going on. I think that there was good reason for some of these changes. Much of the academic work in engineering had gotten over theoretical and divorced from real problems. What has been surprising in information theory is that the theory just kept going. Moreover, it kept getting closer to practice, and practitioners somehow managed to make use of the theorems. In part this was due to a number of very good theoretical people who were also very good engineers. Irwin Jacobs is an example of somebody who understands how to build a company very well, but who also understands all of the scientific underpinnings and therefore moves in the right directions. I think that now we've moved too far in the direction of emphasizing building things, in a broader but shallower kind of learning. People are studying complexity a lot these days. To me, a complex system is a system we don't understand. When even very large systems are well designed, such as the telephone system or today's mobile systems, in some sense they are not complex because they are architected right and they follow simple principles. Even though there are an enormous number of devices, people can understand what they are all doing. To me, complex systems are simply systems that nobody has taken the time to understand. So I think that we should be focussing more on trying to make things simple.

Forney: You've also worked on optical and wireless communication. What is your view of these fields?

Gallager: Optical communication is clearly an important field. However, it doesn't have many nice problems for students to work on. Wireless communication, on the other hand, is a field with an enormous number of very nice communication problems. The fact that the channel has multipath and fading makes it much more interesting than the pure Gaussian noise channel.

Forney: You've produced a great many good graduate students. What are your thoughts on the student-teacher relationship?

Gallager: It's clearly one of the most rewarding and enjoyable parts of being a faculty member. The opportunity to work with first-rate students is wonderful. Sometimes I've found that working with students who don't seem to be quite first-rate is also wonderful in terms of seeing them evolve. Then you feel that you have had an impact on them, whereas the very best students will probably do first-rate research regardless of their advisor. One of my earliest Ph.D, students was Elwyn Berlekamp; I probably didn't contribute much to his thesis since he knew exactly what he wanted to do and how to do it. However, many other students need quite a bit of guidance to do research.

Forney: What are your plans for the next couple of weeks?

Gallager: I will be travelling with two grandchildren and with my wife, Marie. We're going to Israel first for the award of the Harvey prize. Then we are going to South Africa and Greece for two Information Theory Workshops. After that I think I will spend the rest of the summer recovering, trying to finish my stochastic processes book, and reading some thesis proposals. I have 10 doctoral students right now, which seems peculiar for someone who is supposed to be half-retired.

Forney: Any final words of wisdom?

Gallager: Well, I guess one piece of wisdom that I've picked up over the years is not only to listen to wise people whom I respect, but also to listen to myself even more carefully. You can't do good research unless you have insight. If you're doing something suggested by somebody else, then you probably won't have much insight about it. You really have to pick your own problem, and you have to play with it and think about it on your own. If you listen to other people to learn what's important, they usually don't know. All the wise men were saying that communication theory was dead in 1970. 1970 was probably the time when the largest number of new communication applications were really starting to happen. It was the worst possible time to get out of the field. And I think that this is true throughout technology. Many people who forecast technology are beyond their most productive years, and are not the people who ought to be trying to figure out where the field is going.

Forney: Thank you very much. Have a great trip!